PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

EDITED BY

SIR JOHN Y. W. MACALISTER

UNDER THE DIRECTION OF

THE EDITORIAL COMMITTEE

VOLUME THE SEVENTEENTH

SESSION 1923-24

PARTS I & II

GENERAL REPORTS

SECTIONS:—

ANÆSTHETICS BALNEOLOGY AND CLIMATOLOGY
STUDY OF DISEASE IN CHILDREN CLINICAL
COMPARATIVE MEDICINE DERMATOLOGY
ELECTRO-THERAPEUTICS EPIDEMIOLOGY AND STATE MEDICINE
HISTORY LARYNGOLOGY MEDICINE NEUROLOGY



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
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The Royal Society of Medicine.

President—Sir WILLIAM HALE-WHITE, K.B.E., M.D.

DISCUSSION ON "THE ÆTIOLOGY AND TREATMENT OF OSTEO-ARTHRITIS AND RHEUMATOID ARTHRITIS."

Sir ARCHIBALD GARROD (Regius Professor of Medicine in the University of Oxford),

who opened the Discussion, spoke to the following effect:—

Of the branches of medicine that which deals with the more chronic diseases of the joints remains one of the most backward and obscure. The varied nomenclature in use in different lands, and by different observers in our own country, and the systems of cross classification which prevail, bear eloquent witness to the obscurity which still surrounds such maladies, and the backward condition of our knowledge of their pathogeny. For many years the morbid anatomy of one such disease was tacked on to the clinical features of another.

The lesions to which we apply the epithet osteo-arthritic, namely, erosion of cartilages, the formation of osteophytes around the articular surfaces, eburnation of the bony surfaces and the like, may result from any form of articular disease, provided that it be of sufficiently long standing. The joints are comparatively simple structures and the kinds of morbid change to which they are liable are not very numerous. The arthritis of bleeders, if it recurs frequently, produces the lesions of osteo-arthritis, coupled with fibrous adhesions between the articular surfaces. But such changes develop in elderly people in an insidious manner, apart from any antecedent acute or subacute trouble in the affected joints. The hip-joint disease of elderly people offers a familiar example, as also does the affection of the knee-joints of middle-aged women. Heberden's nodes upon the terminal joints of the fingers are also familiar osteo-arthritic lesions, and it is noteworthy that the joints attacked are not the same as in the condition commonly spoken of as rheumatoid arthritis. In the latter the interphalangeal joints of the middle row are specially involved, in osteo-arthritis the terminal joints and carpo-metacarpals of the thumbs. Osteo-arthritis tends to progress steadily and deformity results from bony outgrowths, and not from muscular contracture which is responsible for the grave deformities of the other malady. For my own part I cannot doubt the diseases in question are distinct morbid entities. When one looks back to various discussions of this subject which have taken place during the past thirty-five years, one realizes that ideas as to the natures

of these diseases have undergone profound changes. Doubtless many opinions will be expressed to-day which would have greatly surprised physicians working at the subject thirty-five years ago. We have acquired a considerable amount of more exact knowledge, but, unfortunately, we are not able to do much more for the relief of sufferers from chronic diseases of the joints. One of the advances I believe to be a wider recognition of the fact that more than one and perhaps several maladies had been confused together under the name of rheumatoid arthritis. For this name, which was introduced by my father, I have naturally a pious respect, but I am fully alive to its shortcomings. It was certainly an advance upon the term "rheumatic gout," which it superseded in the middle of the last century, but this in turn has lost its utility, and might be superseded by a better name if such could meet with general acceptance.

We have still much to learn concerning the pathogeny of osteo-arthritis. For many years, and in many writings, Sir Arbuthnot Lane insisted upon the part played by wear and tear of the joints in its causation. Though I believe that these influences do play a very important part in this connexion, I cannot doubt that there is also some underlying cause at work, which may be congenital or acquired. There is an interesting but out of the way piece of evidence bearing upon this point, which is worthy of attention, namely the peculiar liability of persons with alcaptonuria and ochronosis to develop osteo-arthritic lesions in later life. Alcaptonuria is a rare and highly hereditary inborn peculiarity of metabolism, in virtue of which an aromatic acid, homogentisic acid, is present in the blood and is excreted in the urine, which blackens on standing. It is almost certain that all alcaptonurics develop ochronosis if they live long enough. That is to say their cartilages become blackened and undergo some structural change. The osteo-arthritis is a still later event, and may affect all the alcaptonuric members of a family of which the remaining members exhibit no articular lesions. Moreover the spinal lesions, which are conspicuous, impart a special stamp to the clinical picture, and we cannot doubt that the association of the anomaly and of the osteo-arthritis is not accidental.

Another question upon which it is to be hoped that light will be thrown in the course of this discussion, is whether there be any one specific disease to which the name rheumatoid arthritis may be applied, or whether the condition so called is rather a syndrome which, like malignant endocarditis, may originate in infections by several kinds of bacteria. That the malady is an infective one is now generally accepted, but our knowledge of the organisms at work in its production is still very imperfect. The synovial fluid is usually sterile, even in the acute stage, and little success has attended cultures from the synovial membranes.

Cases of rheumatoid arthritis present very uniform clinical pictures: they resemble each other in the mode of onset of the articular lesions and in their distribution, in their persistence and in the liability to remissions and relapses, and in the deformities which they bring in their train. The characters of the arthritis are not so uniform. In some cases the periarticular structures suffer most and in others there is effusion of jelly-like fluid, and in others, again, the cavity is filled with villous outgrowths of synovial membrane.

One of the most striking features of the malady is the absence of any associated visceral lesions, in spite of the severe and long lasting affection of joints.

In some cases there is an obvious septic focus around the teeth or

elsewhere, and treatment of that focus may be followed by arrest of the disease and recovery more or less complete. On the other hand I think it will be agreed that in many cases, and perhaps in the majority, the most careful scrutiny fails to reveal any such focus.

Such experience as I have had in this matter has led me to the belief that among the cases classed under the name of rheumatoid arthritis the majority are examples of a specific infective disease. But there are other maladies which have a great superficial resemblance to that under consideration, and two of these call for special mention.

During the war some of us had opportunities of seeing many cases of bacillary dysentery in an acute stage, and at or near the places at which it was acquired. In not a few cases of that disease there develops a wide-spread periarthritis and arthritis which closely resembles rheumatoid arthritis in its clinical features. There is a similar affection of small and large joints alike, similar fusiform swellings of the joints of the fingers, and muscular atrophy. In a number of cases there is in addition an acute conjunctivitis, and irido-cyclitis also, as was pointed out by Kiep and Maxwell. But in all the cases of dysenteric arthritis which I had the opportunity of watching recovery followed after the dysenteric infection had died out, and serious permanent damage to a joint is certainly uncommon.

The course of rheumatoid arthritis, on the other hand, suggests that the infective agent remains active over long periods, and that by the time that it dies out irreparable damage has been done. In connexion with treatment of rheumatoid arthritis it is important to distinguish between the active stage, which may persist for many months or for years, and the later stage in which we see the after-effects, contracture of muscles, fixation of joints, and the bizarre deformities which form such characteristic sequels of that malady.

The arthropathies which accompany psoriasis also bear a close resemblance to those of rheumatoid arthritis. This form of arthritis has not received the attention which it deserves, but I believe it to constitute a distinct clinical entity. It is apt to develop in close association with an attack of psoriasis, and may cripple the patient completely, but when the psoriasis yields to treatment, especially by arsenic, there may follow a complete or almost complete recovery, so that the patient may resume an active life. However relapses usually occur and successive attacks tend to leave the joints permanently damaged in the end.

Of treatment I propose to speak very briefly, hoping that other speakers, whose experience has been greater during the past few years, will enter more fully into this aspect of the subject. My own experience of the action of drugs in the diseases under discussion has not been encouraging. Of the drugs which have been recommended for the treatment of rheumatoid arthritis guaiacol carbonate has appeared to me the best, but it needs to be given continuously for months or for a year, if any effect is to be obtained. It is to be hoped that we shall have reports of the use of vaccines from some who have large experience of their use. I cannot say that I have seen any case of rheumatoid arthritis in which conspicuous improvement resulted from their administration, but believe that when a septic focus is found a vaccine should be given from which an autogenous vaccine can be prepared. Undoubtedly an attempt to get rid of any definite focus of infection is an essential part of treatment, and recovery may undoubtedly follow such treatment. It is obviously most important that such treatment should be carried out before irreparable damage to the joints has resulted.

Treatment by protein shock, which has been applied by Dr. A. E. Gow and others, undoubtedly gives relief in some cases, but it would not seem that the effects are permanent.

Watering-place treatment is obviously not desirable in the acute and sub-acute stages, but may be of much service when the infection has died out, whereas the deformities are not yet fully developed. Rest is a most important item in treatment, and it is unfortunate, from the patient's standpoint, that in the early stages he is often able to drag himself about, instead of taking to his bed.

I have thought that in the early stages of osteo-arthritis iodide of iron and arsenic have some power of arresting the morbid process. But most patients have to adjust their lives to their disabilities, and get along as well as they can. Treatment at watering places by baths, douches, electricity and massage, is often of much service to the sufferers from osteo-arthritis, even to those who have malum coxæ senile. A judicious course of such treatment often relieves stiffness and diminishes pain, and its effects may persist for a year or more.

In conclusion I would express the hope that in the course of this discussion we shall have the advantage of hearing opinions as to the reality of the distinction drawn between osteo- and rheumatoid arthritis; as to whether or no rheumatoid arthritis is a definite morbid entity, or is produced by a variety of infective agents, and as to the value of modern bacteriological treatments in either condition or both.

Dr. Maurice Cassidy.

It is with considerable diffidence that I take part in this discussion, for it is no easy task to follow Sir Archibald Garrod, and I am aware that the few remarks that I have to make may be regarded as being agnostic or even reactionary. I shall begin by expressing my profound conviction that the importance of infective conditions in the ætiology of these diseases is at the present time grossly over-rated. Particularly is this the case with rheumatoid arthritis. I regard true rheumatoid arthritis as a clinical entity, a somewhat uncommon disease. and one almost entirely confined to women, whom it attacks in early life. In my experience it is uncommon to find any septic focus, however carefully one hunts for it, in a genuine case of rheumatoid arthritis. As a rule these unfortunate patients have perfect teeth, there is no evidence of intestinal toxæmia, the urine is sterile, and the respiratory tract, tonsils, ears, and genitalia appear to be healthy. The fluid aspirated from these joints is invariably sterile, and the synovial membrane itself is, I believe, similarly sterile. Some years ago Mr. Percy Sargent and I examined, microscopically and bacteriologically, small fragments of synovial membrane removed during life from rheumatoid joints. In only one out of fifteen cases was an organism (a streptococcus) recovered, though cultures were made from the minced synovial membrane, aërobically and anaërobically, in all the usual media. I always suspected that our one positive result was due to a skin contamination.

Not only do I feel that there is no evidence in favour of the prevalent view that rheumatoid arthritis is due to septic absorption, usually streptococcal, from some local focus, commonly dental, but—I say this with bated breath—I confess to being a little sceptical as to whether this disease is infective at all. To my mind there is little in its clinical aspect, apart from the occasional pyrexia, to suggest an infective process. It may run a steadily progressive course, with remissions and exacerbations, of thirty or even fifty years, with

never a visceral complication. Pleurisy, pericarditis and endocarditis occur only very rarely, and then probably accidentally. The only really common complication is fibrositis, and I am satisfied that not only is there a definite tendency to heredity in rheumatoid arthritis, but also that the relatives of these patients are specially liable to fibrositis. I often wonder whether both conditions are not metabolic rather than infective, and wish that the biochemists could be prevailed upon to tear themselves away, if only for a few months, from tests of renal function and blood-sugar curves, and devote themselves to re-investigating, on modern lines, the metabolism of gout, rheumatoid arthritis, and fibrositis.

I must repeat that all that I have said refers to genuine rheumatoid arthritis, the disease as described, for instance, in Allbutt's "System of Medicine"[1] sixteen years ago by the distinguished opener of this discussion. I freely admit that there is a large group of cases of chronic infective arthritis which may resemble, sometimes very closely, rheumatoid arthritis, at some particular stage of the disease, but rarely, in my opinion, throughout its course. In this group, an infective focus is often readily demonstrable, and when this focus is adequately dealt with gratifying therapeutical results not infrequently ensue.

But it has been my misfortune to see a considerable number of cases of true rheumatoid arthritis; sooner or later, usually sooner, these patients undergo extraction of all their teeth; almost invariably they submit to a long course of inoculations, often of several years' duration, the vaccines being prepared in turn from the teeth, the fæces, and the urine. Never, in my experience, is the relentless course of the disease arrested by these measures.

What I have said of rheumatoid arthritis holds good also for many cases of osteo-arthritis. Without doubt some cases of osteo-arthritis are infective, and this has been demonstrated both experimentally and therapeutically. But certainly there are other causes of osteo-arthritis than infection. We all know that a joint which has been the site of repeated attacks of acute gout may eventually become osteo-arthritic. I believe that some osteo-arthritic joints are primarily gouty, even though they have never been attacked by typical acute gout. A year ago, I showed, at a meeting of the Clinical Section of this Society, two sisters, who exhibited numerous subcutaneous nodules along the course of the extensor tendons of the hands and feet.[2] These nodules had been appearing slowly during the previous five years; some of them were excised by my colleague, Mr. Max Page, and were found to contain an abundance of uric acid. Now both of these sisters are developing, under observation, osteo-arthritic changes in the knees and other joints, and I find it difficult to believe that this joint condition is not of gouty origin, though in neither case has there been anything remotely resembling acute gout.

We are all familiar with the acute onset of osteo-arthritis of the knees, with or without the simultaneous appearance of Heberden's nodes in the fingers, in many women at the time of the climacteric. Surely this condition must be metabolic rather than infective! Yet these patients, like every arthritic at the present time, are doomed to suffer wholesale extraction of teeth, and probably also inoculations.

[1] Allbutt and Rolleston, "System of Medicine," 1907, iii, pp. 3-30.
[2] *Proceedings*, 1921-22, xv (Clin. Sect.), p. 8.

Some of you may consider these remarks altogether heretical and absurd, and others may think that I have laboured my point unnecessarily. I am fully alive to the dangers of oral and other chronic septic intoxications, and I recognize that perhaps the majority of all cases of chronic polyarthritis are infective. But I submit that there are other common, important, and easily recognized types, and I consider that it is a retrograde and altogether pernicious step to lump all these diseases together under the horrible title "non-specific infective arthritis," as has been done in a recent and, in other respects, admirable text-book of medicine.

We are a profession of extraordinary crazes; not many years ago, every patient suffering from arthritis, and indeed from most other diseases, was drinking sour milk. Now we pull his teeth out. We are apt to smile at the custom of our fathers to bleed almost every patient they came across; I sometimes wonder whether their sons are very much wiser.

Dr. F. J. Poynton.

My position on the occasion of this discussion is very different to that of some twenty years ago, when Dr. Paine and I were enabled to demonstrate the experimental production of an osteo-arthritis. This we had produced intravenously with a streptococcus isolated from the knee-joint in a case of osteo-arthritis in which the patient had died from misadventure by carbolic acid poisoning. That record was a contribution of some stimulating value, and our later results experimentally obtained with the rheumatic organism also helped to elucidate a fact, the credibility of which I think we must all attest, namely, that different types of arthritis may result from the same infection when that infection differs in virulence. In those days practically nothing was known in this country about experimental arthritis, and the above-mentioned results represent some of the brilliant work done by Dr. Paine at the beginning of this century—work which has been without doubt insufficiently recognized by his profession in this country.

Now, I have no such suggestive contribution to make, and on more than one occasion I have shirked participation in these discussions because I have not felt sure, and do not now feel sure, as to what I am talking about. I commence discussing a disease distinct from other diseases, in which an intractable non-suppurative arthritis is a prominent feature, and find myself drifting into the consideration of a severe form of arthritis which may result from various causes. It appears to me that, just as a particular streptococcus may cause acute rheumatism, so very possibly some staphylococcus may be an important cause of rheumatoid arthritis. Then I remember that the streptococcus of rheumatism, if cultivated in various solid media, may become indistinguishable morphologically from a staphylococcus, and I wonder whether in human diseases there is any essential line to be drawn between the streptococci and staphylococci. This is an unsatisfactory position for one taking part in such a discussion.

There is one other fact I would recall, which has a bearing upon the subject under discussion: we produced by *intravenous* injection a monarticular arthritis which was in character an osteo-arthritis. This does not take us far, but in the consideration of osteo-arthritis of the hip-joint it is of some importance to remember that it is an established experimental fact that a solitary arthritis may result from an infection in the blood. Mr. Timbrell Fisher has

laid stress upon this from the clinical standpoint in a recent valuable contribution to the study of osteo-arthritis. Interested as I am in the causation of these diseases, the various changes in the joints that result, though of the greatest practical importance, have never appealed to me as more than incidents, connected with the prime cause, which are dependent upon the age of the joints, the virulence of the infection, the resistance of the patients, the anatomical surroundings of the structures involved, the climatic surroundings and intercurrent injuries. In other words, rightly or wrongly, I have always thought that any infection which attacks the joints may attack large or small joints, and damage any part of these joints in every sort of way under the circumstances already touched upon. But is there some special disease affecting the whole constitution in which the arthritis is really specific, as are the rest of the symptoms? That is one problem about which I am hoping to gain knowledge upon this occasion.

However we may look upon these diseases, there can be no doubt that the view that there is an infective element has gained much ground, and the streptococcus is the infective cause that has attracted most attention. Seldom now do we read any communication bearing on the subject which does not dwell upon "the local focus." It seems also that I do not now see the terrible cases that I used to come across; possibly this may be due to a gradual lapse in my opportunities, but I have for many years acted as a consultant upon these diseases for a large benevolent association, and here again my experience has been the same. I hope the impression made upon others coincides with my own, and that impression if correct, I believe is due to the recognition of the infective focus.

This brings me to what seems to be the most important problem in the subject at the present time; it forms part of the great general problem of infection in the human tissues.

I think the idea of a local focus, a fundamental conception I admit, carries with it a dangerous simplicity. The idea of some accessible focus discharging continuously or at intervals infective agents into the system—a focus the destruction of which will terminate the disease—is most attractive, the more so because it may also be sometimes correct; but the problem as I see it in rheumatoid arthritis is much more complex, and involves the entire question of the behaviour of infections within the body: of the methods of resistance of the body to infection, and the influence of outside factors such as diet, climate, occupation, nerve strain, &c., upon this resistance.

We have only to read a recent communication by Sir William Willcox upon infection of the teeth and gums[1] to realize the far-reaching possibilities of the local focus. This communication particularly interested me, because it brought me face to face with a problem in the treatment of rheumatoid arthritis which I regard as pressing. Sir William attaches the greatest importance to the so-called "apical abscesses"[2] disclosed by radiography. Although such an abscess may have produced no painful symptoms and possibly, at the most, loosening of the affected tooth, I have asked myself whether such a condition may produce a rheumatoid arthritis—and whether the removal of the tooth will check it? The answers that occur to me I have found conflicting. No one would advise keeping in an unhealthy tooth, or still less allow a generally septic condition of the teeth to remain; but that is not the particular point. We are confronted

[1] Discussion on "Dental Sepsis as an Ætiological Factor in Disease of other Organs." *Proceedings*, 1923, xvi (Sect. Odont.), pp. 7-17.

[2] *Ibid.*, p. 11.

with this fact, that after unhealthy teeth have been removed rheumatoid arthritis may continue with obstinacy for years, even to crippling the patient. Either, then, the teeth were not the cause, or if they were, the infection has remained active in the tissues and formed internal foci—and that, if true, introduces a great difficulty. I need not delay with the numerous possible accessible local foci in the body, but in every case the same question arises—are they the cause, and, if they are, will removal produce a cure?

May I give one extreme example? A delicate lady of rheumatic stock after devotedly nursing her mother through a long and fatal illness, some six months later, after suffering from a sore throat or "influenza" or a dental abscess, mysteriously develops a virulent rheumatoid arthritis. Is it not possible that then the local focus, even if it exists, sinks into mere insignificance in comparison with the infective processes in her system devoid of resistance? What must happen if we trust to the crude conception of a local focus and that perhaps an indefinite one? Teeth are extracted, doubtful tonsils enucleated, nasal passages explored, the colon irrigated, and yet all the time none of us can claim to know the life-history of infection within the human body. Yet each active attack on the local foci brings with it a strain on the enfeebled constitution which makes it a serious adventure. We see, in recent writings particularly, this aspect of infection gaining attention in comments upon leucopenia as a sign of deficient response, and we must all feel that leucopenia is but one element in this difficult problem.

When there is an obvious and harmful focus from which a micro-organism is isolated in pure or almost pure culture, particularly if this micro-organism is also found in the blood—the latter being an unusual event—then it appears to me that a vaccine is indicated. On the other hand modern methods lead me to realize that though many cases of rheumatoid arthritis still do not seem to present any local focus, there is no case of rheumatoid arthritis from which a micrococcus cannot be isolated. A growth is always possible from material derived from the tonsils or nasal passages, from fæces, or even from urine. If there are no symptoms however pointing to the local focus from which the growth is obtained, can a vaccine from that focus have any effect upon a case of rheumatoid arthritis? This is a point of practical importance, for we are told that such a course of treatment may require two years, which is a serious undertaking for the patient. From my experience, I am very doubtful as to the value of these vaccines, although well aware that often enough the patient may insist upon such a treatment, and that good results are reported.

I have little experience of protein shock, and my application of it dates from the pre-war period, when it was made with intravenous injections of phylacogen. I have abandoned these, for I could not see how a shock could cure such a stubborn disease. Nevertheless one must not be biased by preconceived opinions, and I admit that the results reported in a recent paper by R. J. Perkins and Dr. Bruce, who worked with a coli vaccine, are not to be lightly dismissed.

The view that rheumatoid arthritis is due to an infection from the bowel and that the local focus is often situated there, meets, I suppose with as much approval as any view; it is probably the most popular. Some may push the history a step further back and say that the gums are the first focus, then, as a secondary result, the alimentary canal, and that after the gums have been put into order the alimentary canal may now become the source of infection. I am not convinced. It appears to me that the colitis we see in rheumatoid arthritis may just as well be of the same origin and nature as the arthritis and

not the cause. Upon this point I shall listen with interest to others for it has important bearing upon treatment. I recognize the value of high colonic douches where there is obvious colitis, but when there is no particular indication and the patient is asthenic, a routine treatment of this kind on the off-chance of destroying the local focus only produces a lower pitch of vitality.

Intestinal disinfectants are of course less trying. I shall not be thought, I hope, so foolish as to deny that constipation or dyspepsia, or mucous colitis and unhealthy motions ought to be treated, but I do not yet feel sure that they are the cause of the rheumatoid arthritis though the correction of these faults may assist the cure by improving the patient's strength. I suppose there is no greater divergence of opinion upon any question in rheumatoid arthritis than upon this one of alimentary toxæmia.

I need hardly say that I have used collosol preparations, collosol iodine and collosol sulphur. I have even used them alternately with the shame-faced feeling that I am returning to the methods of the ancient alchemists. My favourite preparation, however, for anæmic asthenic cases still remains—I almost blush to say it—the time-honoured syrup of the iodide of iron, and my experience of the collosol preparations has left no permanent impression on my mind and the recent paper by Dr. A. J. Clark[1] upon them is not encouraging reading.

One feature in the clinical history of rheumatoid arthritis that has always attracted me has been the importance of the part played by the nervous system. Many of the worst cases I have met with have been antedated by some great nervous strain due to grief, anxiety and mental exhaustion. There has been a great tendency for all the worst symptoms of this disease to develop rapidly after such a history and the illness to follow upon some apparently slight cause, generally, I think, an infection, which may produce no obvious local focus. The treatment of the nervous system is an important factor in the case. The courageous, resolute patient wins through when nerve-worn patients go from bad to worse, and in the case of the latter the application of any drastic measures should not be undertaken without deliberation.

Another line of treatment—that of organo-therapy—is particularly employed in those cases in which there are severe vaso-motor symptoms, and that large group in which the disease commences at the menopause. So far as I can see the correction of a lack of balance in the internal secretions, as much as is possible in the present state of this line of therapy, is reasonable, and likely to improve the patient's resistance—but it is only one factor in a very complicated problem and its virtue is difficult to appraise.

The question of diet, which I think has been mainly resolved on the general rule that a generous diet is advisable, has received fresh impetus from the researches of Pemberton and his followers. It has always been my custom to restrict carbohydrates, particularly in stout patients unable to take exercise, but there still seems to remain for settlement the difficult problem as to whether we can influence infection more accurately by some closer study of the dietetic problem.

Local measures we know to be legion. All have their uses and limitations and few are worthless. There is a curious group of cases with much neuritis; the patients are always worse in the summer, and in their treatment I find radiant heat—and I take this as a type—not advisable. Dr. Campbell's[2] recent

[1] *Brit. Med. Journ.*, 1923, i, p. 273.
[2] *Brit. Med. Journ.*, 1923, i, p. 409.

onslaught on ionization upset me, for I have often thought good resulted from this treatment in cases of painful hypertrophic arthritis, but I cannot help suspecting that much in his paper was correct, although the final solution of the question he raises must be a matter for experts to determine.

One word about osteo-arthritis—my experience coincides with others as to the importance of injury in its production. The Rugby International, the even-time sprinter, the lady who tries to keep up with her husband in long walks, the fiery colonel who lives to slay partridges and red deer—we have seen them all—pathetic sights—hobbling about in pain and weakness. They will not rest at the first warnings, and the calliper or other splint is worse for them than the disease; but for my part I can see no better line of treatment in the early stage for them than that which one would use for a tubercular joint. The next best is the relative rest at a spa.

To sum up: I believe our treatment of rheumatoid arthritis is advancing and that the view that it is infective has done good. I believe we are at present obstructed by our lack of knowledge of the behaviour of infections in the human tissues, and I think this tends to make our treatment too liable to be influenced by imperfect truths or perhaps more correctly, half truths and to make us forget our great ally—the healing power of nature, and that trite saying "treat your patient."

Dr. Arthur F. Hurst.

I believe that the disease so clearly defined by Sir Archibald Garrod and known as rheumatoid arthritis is a common and definite specific disease. I should like to add a few features to the clinical picture he has drawn. I believe that our President, Sir William Hale-White, was the first to draw attention to the constancy during the acute phases of sweating of the palms of the hands and soles of the feet, the rest of the skin remaining dry, and also to the slight but constant increase in the rate of the pulse even when the temperature is normal. Eve, of Hull, and Coates and Gordon, of Bath, have described the constant enlargement of the epitrochlear glands, corresponding with the general enlargement of the lymphatic glands in Still's disease, which appears to be nothing else than rheumatoid arthritis occurring in children.

(1) *Focal Infection, with special reference to Intestinal Infection resulting from Achlorhydria.*

The prognosis of rheumatoid arthritis is far better than some authorities would have us believe, certainly better than if Dr. Cassidy's opinion were accepted that infective foci have nothing to do with the disease. Though in some cases septic foci may be very difficult to discover, I am convinced that they are always present.

The teeth are generally the seat of the primary infection, and it is important to remember that the X-rays may reveal the presence of apical infection in teeth which look quite healthy. For such a crippling disease as rheumatoid arthritis dental treatment must be radical. The tonsils also form a very important focus of infection; flat tonsils, which at first sight appear healthy, may be severely infected. Here, again, radical treatment is essential; nothing short of enucleation is worth doing.

In those cases in which the disease continues to advance after all oral, pharyngeal and nasal septic foci have been eradicated, the intestines have generally become secondarily infected. In this connexion the comparative

frequency of achlorhydria in rheumatoid arthritis is noteworthy. It is present in about 4 per cent. of normal individuals, but Faber found it in fifteen out of sixty-five cases of "chronic polyarthritis" and Kalmeter in twenty-seven out of fifty cases, making together 36·5 per cent. Coates and Gordon, in a series of twenty cases of definite rheumatoid arthritis investigated at the Royal Mineral Water Hospital in Bath, found achlorhydria in fourteen and hypochlorhydria in three, and Woodwark and Mackenzie Wallis found absent or diminished gastric acidity in nine out of ten cases. By the more accurate fractional method we found achlorhydria three times in fifteen consecutive private cases; definite hypochlorhydria was present in five and a low normal curve in three out of the remaining twelve cases. In individuals with deficient gastric secretion the intestine readily becomes infected from the mouth and pharynx; this secondary infection requires treatment by the administration of large doses of dilute hydrochloric acid, sour milk, and a vaccine prepared from the primary foci, and, if possible, from pathogenic organisms isolated from the duodenal contents obtained by an Einhorn tube.

(2) *The Importance of Rest.*

During the active stages of the disease complete rest for every painful and tender joint greatly accelerates recovery, and fixation in a good position prevents deformity of fingers, wrists, knees, and other joints. The irksome limitation of activity for weeks or months is worth while, as it may save the patient from becoming permanently crippled and perhaps bed-ridden at a later stage.

(3) *The Psychological Factor.*

The active disease always dies out eventually, sometimes only after many years if the foci of infection have not been treated. But with their complete eradication and with rest its course is comparatively rapid. When no pain and no tenderness are present rest is no longer required. But patients who have been long confined to bed on account of the pain involved in moving their limbs rarely discover for themselves when the time for activity has arrived. I believe that there are thousands of people bed-ridden in their homes, in infirmaries and in hospitals for incurables, who are really capable of taking up their beds and walking if they only knew it. When the active disease has died out, the fixity of the joints is very rarely due to bony ankylosis, and comparatively rarely to firm adhesions. It may be partly due to contractures, which ought never to have been allowed to develop and which can generally be overcome by orthopædic measures. But in most cases it is mainly due to the prolonged inactivity having fixed the idea of incapacity in the patient's mind. His muscles are weak from disease, but they are not paralysed; his joints are stiff, but they are not ankylosed. Yet he makes no attempt to move them. The incapacity is in fact largely hysterical, being caused by the suggestion of incapacity developing in the patient's own mind and often also in the minds of those in attendance upon him. Explanation, persuasion and re-education, with the aid of manipulation and massage, which are, however, of quite secondary importance, often result in the recovery of the use of the limbs in a few weeks or even a few days. About three years ago a man came under our observation, who had been completely bed-ridden for three and a half years, with his knees fixed in a flexed position, and with inability to use his deformed hands. There was no longer any active disease, and as a result of combined orthopædic and psychotherapeutic treatment he

rapidly improved. When I showed him here before the Clinical Section [1] about six months later, there was no longer any limitation of movement in his knees, which looked perfectly normal, and he could walk miles. His hands still showed the typical deformity, but he could use them for all ordinary purposes.

If the treatment of rheumatoid arthritis by rest and eradication of foci of infection in the early acute stages were universally carried out, and the late cases, in which there is really no longer any active disease, were all vigorously treated by a combination of orthopædics and psychotherapy, the disease would soon cease to be a reproach to the medical profession and the most common of all causes of permanent total incapacity.

Dr. T. S. P. STRANGEWAYS (Cambridge)

demonstrated pathological specimens to show that there were at least six types of arthritis at present included under the name "rheumatoid arthritis." In order to avoid new names he had provisionally classified these types as follows: (1) capsular, (2) dry, (3) adhesive, (4) rarefying, (5) villous, (6) infective.

In the *Capsular type* the joint changes were practically confined to the capsule of the affected joint. The synovial membrane was not much altered. The capsule itself showed some degree of thickening and fibrosis. The articular cartilage, as a rule, appeared normal, but in the later stages of the disease it might show atrophy over areas of pressure. The bones of the articulation were normal. This type might be found in both sexes, but was more common in the female sex. The patient complained of loss of strength and a feeling of slackness. The joints were swollen, and on movement were stiff and painful, and often there was a leather-like creaking. Apparent recovery, or great improvement in the symptoms, might take place within a few months of the onset, but a relapse nearly always followed within a longer or shorter period. Although complete recovery might occur, the patient, as a rule, gradually became more and more helpless and crippled.

In the joints of the *Dry type* the characteristic change was the absorption of the synovial fluid which in the later stage of the disease was completely absent. The capsule became atrophied, fibrous, and tightly contracted, gripping the bones of the articulation firmly, and thus causing the articular surfaces to to lie in close contact with each other. The synovial membrane was dry and had the appearance of parchment. The cartilage was atrophied from pressure. The bones of the articulation were not altered. This type was seen more commonly in the female sex, and in its early stages it was sometimes diagnosed as chronic rheumatism or fibrositis. There was no swelling of the joints, but as the disease progressed movement became more and more difficult and eventually impossible, and this often led to a mistaken diagnosis of "bony ankylosis." In spite of the close contact, true adhesions between the joint surfaces did not take place, but if the joint was opened the articular surfaces would be found stuck together, so that some force was required to separate them.

The *Adhesive type* began with inflammatory changes in the synovial membrane and capsule. These changes were associated with the formation of a vascular granulation tissue which spread over the cartilage and became adherent to it, thus causing vascularization of the perichondrium and of the deeper layers of the cartilage. This was followed by the erosion and replacement of the

[1] *Proceedings*, 1922, xv (Clin. Sect.), p. 20.

cartilage by a vascular inflammatory connective tissue which later invaded parts of the calcified cartilage and the bone underlying it. As this inflammatory process spread, vascular adhesions were formed in the joints between adjacent articular surfaces. This acute condition of the joint might persist for months or years, but eventually tended to subside. The inflamed capsule and synovial membrane, together with the newly formed connective tissue and adhesions, gradually became less vascular and eventually were replaced by firm fibrous scar tissue in which development of new bone might occur, leading to bony ankylosis. This type appeared to be confined to the female sex. The patients were thin and wasted, and several joints were usually attacked. They complained of intense pain on movement, but apart from the joint conditions were not in bad health. After several years the disease usually became quiescent, but the patient remained permanently crippled owing to the joint changes.

In the *Atrophic type* the bones in the affected joint were greatly rarefied, and eventually the cancellous tissue was practically replaced by pure fat, so that the bone cut readily with a knife. Accumulation of fat was also present in the muscles and other tissues of the limb, which in extreme cases became so friable and fatty that on forcible extension they broke like a carrot. The capsule, synovial membrane, and articular cartilage were atrophied. In the smaller joints atrophy of the bones might be extreme, especially in the joints of the hands when they had been used, in spite of the joint changes. The carpi were fused together, and after maceration had a lace-work appearance resembling that of white coral. In the early stages inflammatory changes took place in the capsule, and as these subsided bony ankylosis of the larger joints often occurred. In the late stages of the disease the atrophy of the limbs, especially of the hands, was a marked feature. This type appeared to be confined to the female sex, and the patients were usually well nourished and cheerful. There might be a history of exposure, such as sleeping in a damp bed or sitting in a cold train when wet through.

The *Villous type* was seen in both male and female patients, who complained of the joints feeling weak and somewhat painful on movement. There was considerable swelling of the affected joints, which felt soft and full of fluid. Although some increase of synovial fluid might be present the swelling was chiefly due to the presence of a number of larger or smaller pedunculated villi, which might be so numerous that the whole of the synovial membrane was covered with them. On opening the joint these villi appeared as soft, velvety, pinkish outgrowths, and in some cases, especially where the pedicle was long, they were deeply congested. It was in this type that "joint mice" or melon-seed bodies were found lying in the joint cavity. As a rule only the larger joints were involved and the condition seemed to be due to formation of warty growths of vascular polypi on the synovial membrane. The cartilage was not much altered, but might show signs of atrophy. The bones of the articulation were normal.

The *Infective type* included a group of cases which were usually diagnosed as rheumatoid arthritis, but in which the arthritis was due to some definite micro-organism. A careful inquiry into the history, and a study of the cases, would occasionally make it possible to determine the type of micro-organism to which the arthritis was due, but more often it was impossible to arrive at a definite diagnosis. Examples of this type were those obscure forms of chronic arthritis due to the gonococcus, pneumococcus, or various types of dysenteric bacilli, and most observers would include streptococci and staphylococci; but he was doubtful if these two latter organisms were so common a cause of

chronic arthritis as was usually thought. Clinically the condition might resemble the adhesive type, but a careful inquiry into the history of onset and progress of the disease, together with a study of the clinical symptoms, usually made it possible to give a provisional diagnosis of infection. This type would, of course, drop out of the above classification when methods of diagnosis improved.

Dr. J. A. GLOVER (Ministry of Health)

said that as he had been working at the records of the recent Ministry of Health preliminary inquiry into the incidence of rheumatic diseases amongst the insured, he proposed to demonstrate a few estimates on the ætiological side of the two diseases under discussion that evening, which might be of interest. To prevent any misunderstanding he explained that the inquiry was not one into rheumatoid or osteo-arthritis specially, but had arisen in the following way. In 1921 by direction of the chief medical officer a committee of the medical staff of the Ministry had been formed, with Sir George Buchanan as chairman, to look into the huge amount of mortality ascribed to "rheumatism" known to exist amongst the insured and to see if, and in what direction, the Ministry could help with regard to investigation already in progress or contemplated. After a review of previous work it had been thought that as a preliminary inquiry it would be useful to estimate the respective incidence of the various forms of so-called "rheumatic" diseases in relation to the age and sex, occupation and environment of insured persons and the relative economic importance of these several diseases by their production of sick absence. The committee had been highly fortunate in being able to enlist as a team of voluntary observers a number of insurance practitioners in various parts of the country, and so to keep a large sample population under observation throughout the year 1922. Fifty practitioners had fulfilled for the whole year the arduous task of recording every case of rheumatic disease occurring in an insured person in their practices on a uniform plan. The amount and quality of the work done by some of these observers was marvellous, and the high general average of the work done and the keenness displayed remarkable. By their unselfish and public spirited labours, these gentlemen had rendered it possible to keep a sample population of 90,000 insured persons under review for a year, a population which, though it was less than one-hundredth ($\frac{1}{140}$) of the whole, was yet large enough and varied enough to form a reasonable sample of the 13,500,000 insured persons in England and Wales.

The experiment was an interesting one, being the first team inquiry on these lines, and the inquiry had one great advantage over previous collective inquiries (such as that of the British Medical Association in 1888 into rheumatic fever) in that the population at risk was pretty accurately known.

It had also been decided as a separate inquiry to ask the regional medical officers of the Ministry to record on the same uniform plan every case of rheumatic disease referred to them during the same year 1922.

To secure uniformity of method in recording—a matter of peculiar difficulty—the committee, after careful consideration had adopted a simple classification of the various diseases under nine headings (two of which were rheumatoid arthritis and osteo-arthritis respectively), a record card, and a memorandum of instructions, of which instructions the most important was the list of exclusions, i.e., those forms of arthritis the origin of which was undoubted, for example, gonorrhœal arthritis. The nine categories of the

classification were comprehended in three groups—the acute group, the non-articular manifestation group, and the group of chronic joint change. The latter group of course included the diseases under discussion that evening, and in order that it might be possible for them to follow the few results he (Dr. Glover) was able to give, it was desirable that they should see the exact specifications used, as set out in the following table:—

Group C	Chronic joint changes	
6	Rheumatoid arthritis (infective peri-arthritis)	Usually with an acute *initial attack of pyrexia* of considerable length: *many smaller joints* usually affected, especially first and second phalangeal joints, *spindle-shaped joints*, the temporo-maxillary joint is often affected; changes mostly *peri-articular* often accompanied by much fibrositis; lesions often bilaterally symmetrical; nutrition of patient almost invariably bad; patients usually females
7	Osteo-arthritis, including malum coxæ senilis (See note below as to spondylitis)	Usually afebrile; gradual onset, often at first confined to one large joint, *fewer and larger* joints affected, especially knees and hips; *grating; lipping; eburnation; osteophytes;* lesions often asymmetrical; patient usually well nourished
8	Gout—(*a*) Acute (*b*) Chronic	Usual text-book description Polyarthritis with a definite history of repeated attacks of acute gout or the presence of tophi
9	Chronic joint changes unclassifiable	Chronic joint changes which cannot be allocated to any of the above (see also note on sub-classification)

The number of cases reported respectively in the two inquiries was as follows:—

	Males		Females	
	Regional medical officer series	Practitioner series	Regional medical officer series	Practitioner series
Rheumatoid-arthritis ...	364	83	805	97
Osteo-arthritis	640	149	623	71
Gout	88	150	23	3
Unclassifiable	28	50	43	20
Total ...	1,120	432	1,494	191

Dr. Glover then showed tables indicating the age grouping of the insured population at risk and comparative age grouping of the patients observed suffering from chronic joint changes.

The attack rates for every age group above sixteen for the diseases in question were as follows:—

Attack Rates per 1,000 in each Age Group. Insured Persons. (Practitioners' Inquiry.)

Age groups		16-24	25-34	35-44	45-54	55-64	+ 65
Rheumatoid arthritis	Males	0·33	0·34	1·06	3·43	3·75	6·47
	Females	0·67	1·91	2·73	7·60	18·24	49·43
Osteo-arthritis	Males	0·13	0·34	1·71	2·32	9·56	35·20
	Females	0·07	0·56	1·47	7·60	16·72	60·84
Gout ...	Males	—	0·13	1·38	6·08	9·56	17·96

He also showed tables indicating the attack rates in the four English divisions which seemed to show that there was some excess of osteo-arthritis, particularly in males in the north-western division, an excess of gout also in the males in the south-western and south-eastern divisions, and an excess of

rheumatoid arthritis in the south-eastern division, the latter especially in females.

Dr. Glover then discussed the question of dental sepsis and gave diagrams illustrating the findings on this point with regard to both sexes. The following table showed the percentage of patients above the age of 25 with good teeth, and clean mouths, with the four diseases, compared with control series, patients suffering from diseases other than rheumatism. Referring to malum coxæ senilis, Dr. Glover called attention to the much greater incidence that this disease had upon the left rather than upon the right hip.

PERCENTAGE OF PATIENTS OVER 25 WITH GOOD TEETH AND CLEAN MOUTHS.

Disease	Males		Females	
	Practitioner series	R.M.O. series	Practitioner series	R.M.O. series
Rheumatoid arthritis	8 per cent.	11 per cent.	15 per cent.	8 per cent.
Osteo-arthritis	15 ,,	9 ,,	16·6 ,,	3 ,,
Gout	14 ,,	21 ,,	(only three cases)	9 ,,
Unclassifiable: chronic, joint, change	12 ,,	8 ,,	(only twenty cases)	2 ,,

Control Series.

Dr. Harper's series; referred patients with other diseases; males	17 per cent.	
Dr. Panton's 104 cases of pernicious anæmia; both sexes	20 per cent.	*Lancet*, February, 1923.
Dr. Panton's series of 100 in-patients: London Hospital	14 per cent.	
Bellevue Hospital	19 per cent.	
(Series of 250 consecutive cases of pneumonia; Lambert.)		

Dr. W. J. MIDELTON (Bournemouth)

regretted that no speaker, so far in the discussion, had mentioned treatment of these conditions by continuous counter-irritation. He had now been carrying this out twenty-two years, and he regarded it as the most efficacious form of treatment yet known. Dr. P. W. Latham had first called his attention to it; he (Dr. Latham) showed three cases he had successfully treated at Addenbrooke's Hospital. One method, introduced by Dr. W. B. Rule, consisted in multiple acupuncture, followed by the application of croton oil, cantharides and almond oil. He showed on the screen photographs of several cases in which he carried out the treatment.

Case I.—The disease in this case had not been diagnosed when he saw it; he (Dr. Midelton) provisionally diagnosed pneumococcal arthritis. Patient had been using rigid support, and had been moping at home, very much depressed. After this treatment she was able to discard the support, and join in social intercourse with other people.

Case II.—The patient was supposed to have only two months to live, owing to heart disease. After the application of acupuncture and continuous counter-irritation, he was able to make a meal of steak and onions and to walk five miles.

Case III.—Patient had suffered from traumatic arthritis, the result of a strained ankle sustained forty years previously. The toes were chronically inflamed and quite rigid; but within six weeks of the commencement of the treatment the toes became movable, the ankle well-defined, the muscles strong, and the feet useful.

Case IV.—A trained nurse, who had sprained her ankle thirty years before he (Dr.

Midelton) saw her. She had arthritis, with rigid toes, but within six weeks of receiving the treatment she had a useful foot.

Case V.—The patient had suffered from lupus erythematosus complicating arthritis, but her condition had greatly improved after blistering of the spine.

Case VI.—Enormous swelling of wrists. Patient had suffered from generalized arthritis with generalized wasting of muscles. Almost helpless when first seen but under treatment by the multiple acupuncture method he had improved so much that he could walk about unaided, the muscles had increased in size and strength, and from being very depressed he became cheerful. His illness had lasted many years, and no previous treatment had benefited him materially.

Case VII.—Photograph of a patient suffering from Still's disease, for which many forms of treatment had been tried without benefit. Two months from the time of beginning continuous counter-irritation her hair had grown thickly; her wrists had decreased in size sufficiently to enable her to feed herself, and her mental state had also improved. Ten months later her medical attendant (Dr. Rees, of Swanage) reported arrest of the disease.

Case VIII.—That of a trained nurse who had suffered many years from arthritis before coming under his (Dr. Midelton's) care. She had been extensively treated by other doctors. He (Dr. Midelton) treated her by means of blisters applied in the region of the lumbar and cervical enlargements of the spinal cord, the raw surface being kept open for fourteen days. The blistering was carried out in two stages. First the cervical application, then a rest for seven days, followed by the lumbar application. She had gone through this course several times at intervals between of from three to six months, and also from time to time she had been treated by means of the acupuncture method, &c. She had made such a good recovery that some years later when examined in London by Dr. Batty Shaw and other physicians the idea of her having had serious arthritis was ridiculed.

The blistering method was first described by Dr. J. K. Mitchell, the father of Weir Mitchell, one hundred years ago, and Dr. P. W. Latham revived it. In none of these cases was any other treatment worth mentioning employed.

Dr. G. L. Kerr Pringle (Harrogate)

said he disliked the term "rheumatoid arthritis" which was apt to be used as a "scrapheap" for undiagnosed articular affections: and further, patients regarded it as an incurable disease.

He thought that four different types were included under the heading: (1) infective; (2) chronic fibrous rheumatism, or periarticular arthritis, a condition recognized by Jaccoud; a type which followed on some cases of acute and subacute rheumatism, gonorrhœal rheumatism, and was very frequently associated with intestinal toxæmia and sepsis; (3) an atrophic type occurring in only about 6 per cent. of cases coming under this heading, associated with nerve irritation, peripheral and central neural involvement, such as anomalies of sweat secretion, glossiness and ulceration of skin and nails; (4) a villous form attacking the larger joints in middle life, frequently associated with septic foci, static injuries and endocrine changes. This variety might sometimes lead to the osteo-arthritic variety. In all these forms of arthritis it was advisable to cut down the carbohydrate intake, substituting for this fresh fruit, green vegetables and red meat.

Dr. Lydiard Wilson

drew attention to a form of thermal treatment for arthritis, muscular rheumatism, gout and neuritis, which was quite new to London. It had previously been used only in Bath; it had been devised by Dr. Percy Wilde, who had

observed that patients suffering from these conditions had a subnormal temperature which was the cause of uric acid being held up in their tissues. If the temperature of these patients could be raised and natural sweating ensue, the acid was liberated freely in their sweat. This could be accomplished by Dr. Wilde's thermal couch, which raised the patient's temperature by 2° or 3° F., causing rapid and profuse sweating but without exhaustion or risk of chill. Turkish and radiant heat baths produced sweating by dry heat at high temperatures, but did *not* raise the patient's temperature; and these were often followed by exhaustion and consequent risk of chill.

Dr. Wilde's couch consisted of an ordinary iron six-foot-six bed with an apparatus underneath the wire mattress which generated steam at a given temperature. Over this bed there was a metal canopy reaching to the foot but not covering the head. Inside the canopy there were special electric lamps which could be regulated at will and used in conjunction with the steam in cases which required it. The temperature inside this couch was only 102° to 105° F., yet its effect was to make the patient sweat rapidly and naturally because the cutaneous nerves and sweat glands were stimulated instead of exhausted by the temperature being raised two or three degrees.

The couch could be used with safety in the case of patients with heart disease, and there was no record of a cold or a chill having been contracted as a result of taking the bath.

He (Dr. Wilson) had tried the bath on thirty of his own patients with surprisingly good results. The reaction of the sweat from the hands, feet, chest, forehead, and other parts was tested, and the patient's temperature was taken before and after the bath and recorded on special charts. It was of interest to note that the sweat from an inflamed joint or a painful fibrositic area was especially acid, and remained acid so long as the inflammation persisted, normal sweat being alkaline.

In the case of rheumatic patients the bath should be given two or three times a week and continued until the sweat was no longer acid. Usually twelve baths were sufficient.

The couch had been installed, and was open daily to inspection, at 18, Manchester Square, in charge of a sister who had given this treatment in Bath for many years.

The Royal Society of Medicine.
President—Sir WILLIAM HALE-WHITE, K.B.E., M.D.

OCCASIONAL LECTURE.

Circulation and Assimilation in Plants.[1]

By Professor Sir JAGADIS CHUNDER BOSE, C.S.I., C.I.E., F.R.S., M.A., D.Sc., LL.D.

(Director Bose Institute, Calcutta).

ON two previous occasions I have been able to give, before this Society, demonstrations of various life reactions in plants. The imperceptible growth of the plant was magnified fifty million times; a moving spot of light indicated the rate of growth. Application of drugs produced very characteristic effects; stimulants caused great enhancement of the rate of growth, or awakened it when dormant. Under the action of poison a violent death-spasm was seen to occur corresponding to the death-throe of the animal. These and other reactions were demonstrated by the automatic record made by the plant itself; they showed that the fundamental mechanism in the life processes is the same in the plant and in the animal. From the establishment of this generalization, it follows that the intricate problems of animal life are likely to be solved by the study of corresponding problems in the life of the plant.

Far more important than external manifestations are the activities in the interior of the organism which are beyond our scrutiny. New instruments of great sensitiveness have therefore to be invented to bring the invisible within the range of the visible. I shall describe a new method and appliance I have devised, which enabled me to probe into the interior of the organism. The internal activities, hitherto unsuspected, thus became fully revealed. I shall describe other experiments which solved the great mystery of the ascent of sap in trees, which baffled inquiry for more than two hundred years. I will also exhibit my new apparatus by which the rate of carbon assimilation in plants becomes automatically recorded, and by which investigation on this subject has been greatly extended. The shortness of time available will allow only brief accounts being given of two very extensive lines of investigation. The subject will be found fully dealt with in my "Physiology of the Ascent of Sap" already published by Messrs. Longmans, and the "Physiology of Photosynthesis" shortly to be published by them.

THE ASCENT OF SAP.

The movement of sap inside the tree is invisible, and no accurate means have hitherto been available for the measurement of the normal rate of ascent of sap, and how that rate is affected by changes of the environment. I have overcome the difficulty by devising three different types of apparatus by which the normal rate and its modification are determined with the highest accuracy.

[1] Substance of an Address delivered before the Royal Society of Medicine, December 6, 1923.

Strasburger, from his experiments, imagined that poisoning did not affect the ascent of sap; hence he inferred that the movement of sap cannot be due to the physiological action of living cells. This view, which found general acceptance, had the most disastrous effect on investigation of this intricate problem. My results completely disprove Strasburger's conclusion. The records obtained by my apparatus show that a complete arrest of ascent takes place under the action of poison. A simple, yet most conclusive, experiment is to take two similar drooping stems; of these the cut end of one is placed in water, that of the other in a poisonous solution of formaldehyde. In the first case the drooping stem soon becomes turgid and fully re-erected by the ascent of sap, but there is no recovery in the poisoned stem, which droops still more and soon becomes a huddled mass of dying tissue. This offers a conclusive proof that the movement of sap is essentially due to the cellular activity in living plants. Further experiments prove that a definite active tissue extends throughout the length of the tree, the cellular pulsations of which in regular sequence cause, by their pumping action, the propulsion of sap. In the animal the circulation is maintained by the pumping action of the throbbing heart. Since the mechanism for the movement of sap is essentially similar, the tree may be regarded as possessing an elongated "heart."

The effects of various agents on the "heart beat" of the tree and that of the animal, exhibit most astonishing similarities, of which I shall mention only a few instances. In the case of the animal it is well-known that the frequency of heart-beat is quickened under a rise, and slowed down under a fall of temperature, till at a sufficiently low temperature the rhythmic activity becomes arrested. Under certain abnormal conditions when the beating of the animal heart is arrested, it can be renewed by adequate stimulation. Certain stimulants cause a great enhancement of the pulsatory activity, while poisons arrest it. Results in every way parallel to these are observed in the cellular pulsation of the tree. Rise of temperature, increasing as it does the pulsatory activity, enhances the rate of ascensional movement of the sap. Lowering of temperature, on the other hand, causes a depression of the rate, till, at a critical temperature, the ascent becomes arrested. A slight rise of temperature above the critical point, however, renews the ascent. Under a condition of sub-tonicity the cellular activity and the resulting movement of sap become arrested. Stimulus is now found to revive the activity and renew the ascent. Very striking results are produced by the action of anæsthetics, which in small doses act as a stimulant. A small dose of ether is thus found to enhance the cellular pulsation and cause a great increase in the rate of ascent. Chloroform causes a preliminary enhancement, followed by arrest caused by the toxic effect of a large dose.

Diurnal periodicity in intravascular pressure.—The cellular activity undergoes a periodic variation, in response to changes in the environmental condition during twenty-four hours. This causes a very interesting diurnal periodicity of the pressure exerted by the sap, as indicated by a self-recording manometer attached to the tree. It would no doubt be interesting to find out whether there is a diurnal periodicity of blood-pressure in the animal.

The "Milking" of the Palm Tree.

The water pumped up by the root causes intra-vascular pressure, and profuse exudation may thus take place when a hole is drilled into the tree. But the Indian date palm grows in a dry or even arid soil, and necessity compels the tree to exploit the scanty or precarious supply of water. The sap

is held very tenaciously in the trunk, and a hole drilled into it causes no exudation of sap. Yet, after certain special treatment, the tree yields quantities of sap containing sugar, as large as 20 litres per day. No explanation has hitherto been offered for this copious exudation in the absence of internal pressure to urge it. My experiments show that a dormant and inactive tissue is roused to intense activity under repeated irritation. When a slanting cut is made in the date palm at the upper end of the trunk, it does not cause any yield of sap. But when the wound is made for several days in succession, the cumulative irritation is followed by copious exudation of the sap. This drastic and harsh treatment cannot but cause injury; it is within bounds of possibility to devise other modes of irritation which are less injurious to the tree. In certain other varieties of palm the sugar-containing juice is yielded by the inflorescence, and the coercion employed here is as curious as it is interesting. The Malays inflict repeated blows for several days with a wooden hammer to the base of the flower stalk; after this an incision made at the tip of the spadix is followed by copious exudation. In Bengal the practice is perhaps a little more humane; the long spadix is held tightly between the fingers and kneaded downwards. This potential milking process is continued for about a week, after which the yield of sap becomes abundant. The two processes just described may be aptly described as "butting" and "milking," from the analogy of the action of the calf to make the cow yield her milk.

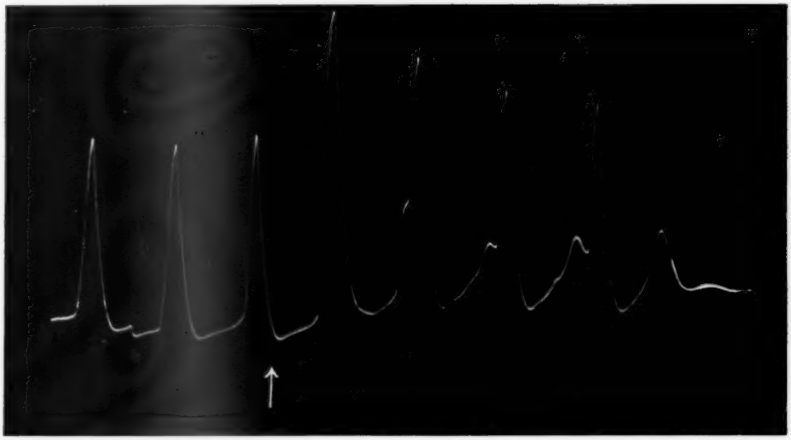


FIG. 1.—Electric record of cellular pulsations in the interior of the plant. Application of chloroform at arrow is seen to cause a preliminary enhancement followed by permanent arrest due to toxic dose.

THE ELECTRIC PROBE FOR RECORD OF THE INTERNAL PULSATION.

The experiments already described prove that the movement of sap is maintained by the pulsating activity of certain cells. The next problem was the localization of the pulsating layer, and to obtain an actual record of the individual pulsation and watch its responsive variations under drugs and other agents. For this we have to get access to the smallest unit of life, the

individual cell or the "life atom," a congregation of which constitutes the living organism. But the pulsatory movement of a cell is ultra-microscopic and its detection may well appear to be beyond the range of possibility. This has, however, been accomplished by my electric probe in circuit with a recording galvanometer. The probe is gradually introduced across the tree, its tip thus coming in contact with successive layers of cells. The record thus obtained is thrown on the screen, from which it will be seen that the galvanometer remained quiescent till the probe came in contact with the active layer, the throbbing pulsations of which gave rise to corresponding electric pulsations recorded by the galvanometer. In dicotyledonous plants it is the inner layer of cortex which functions as the organ for the propulsion of sap. Moreover, any agent which quickens or arrests the heart-beat of the animal is also found to enhance or inhibit the electric heart-beat of the tree. The automatic records given by the tree are thrown on the screen; under the action of a stimulant we find the cellular pulsations become very greatly enhanced. The effect of a poisonous dose is soon seen in the permanent abolition of the throbbing activity (fig. 1). We thus find that the tree which appears so insensitive and inactive is not so in reality, but that notwithstanding its placid exterior intense and ceaseless pulsations are taking place within it—pulsations which are modified in response to changes in the environment. The fluctuations of the life-activity in the interior of the tree are thus revealed by the waxing and waning of its pulse-records. We are now in a position fully to realize the essential similarity of physiological mechanism in the maintenance of circulation in the plant and in the animal.

Assimilation in Plants.

It is through these incessant internal activities that the tree is enabled to raise large quantities of water to a height sometimes as great as 450 ft., as in the giant eucalyptus. The energy for doing this work resides in the breakdown of complex chemical substances in internal combustion or respiration. Energy must therefore be stored in meeting this loss; green leaves function in storing the energy of sunlight, CO_2, which is the gaseous food of the plant, being built up by photosynthesis into carbohydrate. The phenomenon of CO_2 assimilation in plants is of great thoretical interest, as an example of the simplest type of assimilation. In normal photosynthesis a certain volume of CO_2 is absorbed and an equal volume of oxygen evolved. Photosynthetic activity may therefore be measured from the rate of absorption of CO_2 or of evolution of oxygen. The method that has been generally employed is the absorption of CO_2, necessitating complicated chemical analysis, which is therefore a very prolonged and laborious process. It is not a very sensitive or a highly accurate method. The evolution of oxygen by water plants is a more sensitive indicator of photosynthesis, but numerous sources of error had hitherto stood in the way of its employment for quantitative measurements. These difficulties have been completely removed by the new method which I devised, by which the evolution of equal volumes of pure oxygen becomes automatically recorded (fig. 2). The photosynthetic recorder is seen at work; the record of successive evolution of equal volume of oxygen is obtained by the electric device which actuates the electro-magnetic writer to inscribe successive dots on a revolving drum (fig. 3). It also gives independent audible signals. This method being automatic all personal errors of observation are completely eliminated. It is also so extremely sensitive that it is possible to measure photosynthetic deposit of

carbohydrate as minute as a millionth of a gram. The extreme sensitiveness and accuracy of this new method has led to the discovery of several important phenomena which otherwise would have been impossible. Stimulation is



FIG. 2.—The vessel containing the water-plant, *Hydrilla*. S, stop-cock; B, bubbler; O, oil-valve. Photosynthetic evolution of oxygen lifts the oil-valve, successive bubbles of the bubbler representing evolution of equal volumes of gas.

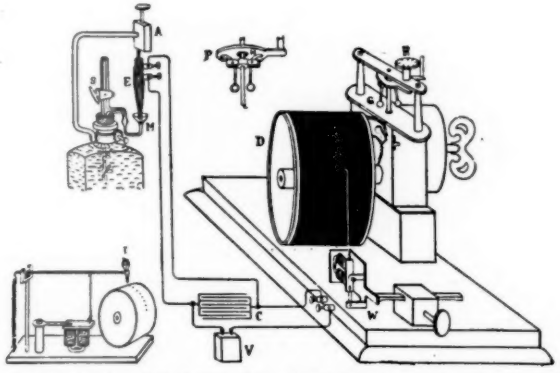


FIG. 3.—The photosynthetic recorder. E, the electric pencil for completing contact through drop of mercury, M; A, adjusting screw; V, voltaic cell; C, condenser; D, revolving drum; W, electromagnetic writer; G, governor (shown separately at P, with pair of hinged levers, H). I, ink recorder.

found to produce characteristic modification in the assimilation. A moderate stimulation enhances this power, but stronger stimulus inhibits it, the period of inhibition depending on the strength of stimulus. Still more interesting are

the actions of different chemical agents, the effects of which are strikingly modified by the strength of the dose.

Derangement of normal assimilation.—In normal cases the various co-efficients for the activity of assimilation in a particular season of the year are fairly constant. The respiratory quotient is in normal cases very nearly equal to 1. But a sudden derangement occurred in the physiological condition of the plant during the passage of heat wave in Bengal in April last. The respiratory quotient was then found to be very much lowered and less than unity. Further examination showed that the juice of the normal plant was practically neutral, but under the excessively high temperature in April the plant becomes markedly acid, this physiological derangement being probably associated with the abnormal variation in the respiratory quotient.

Effect of Infinitesimal Traces of Chemical Substance on Assimilation.

During the course of another investigation I discovered the effect of minutest traces of certain chemical substances in inducing an extraordinary enhancement of assimilation by plants. The dilution employed was one part in a billion (billion in French measure is equal to 1,000 millions). With certain substances a dilution of one part in two billions produced an increase of activity of more than 200 per cent. The activity declined when the strength of the solution was raised above a critical dose. Dilute extract of thyroid gland, in a dilution of one part in a billion produced a maximum increase of activity of about 70 per cent. The noticeable fact in the action of thyroid extract is that no diminution of activity below normal took place for a considerable range in the dilution. The effect of traces of iodine was more or less similar. At first sight it is inconceivable that infinitesimal traces of certain chemical substances should have such a potent influence on life-activity. There is, however, no doubt about the reality of the phenomenon. The immediate and concrete demonstration of the effect of minute traces of chemicals on assimilation of plants is of special interest, since it enables us to understand the effects of ultra-measurable quantities of vitamin on general assimilation and of hormones on physiological reaction.

The plant is a multicellular organ and hence necessity arises for inter-communication and interaction between more or less distant organs; this is accomplished in two different ways: the first is exemplified by the hydraulic convection of liquids carrying chemical substances in solution such as occurs in the circulation of sap. The second mode of intercommunication is the conduction of excitatory change along certain tissues in the plant which function as nerves.

The ultimate result of investigations such as these and others which I have been able to complete, is the establishment of the important generalization of the unity of physiological mechanism in all life. For we find in the plant and in the animal, similar contractile movement under stimulus, similar reaction under particular drugs, similar cell-to-cell propagation of pulsatory movement, similar circulation of fluid by pumping action, similar nervous mechanism for the transmission of excitation, and similar reflex movements at the distant effector. The simpler type of plant organization offers a unique advantage in investigation, the pursuit of which will no doubt lead to the solution of many perplexing problems of animal life.

Treatment of Rhino-laryngological Tuberculosis by Finsen Light Baths, and Results.[1]

By Dr. Ove Strandberg.

(Introduced by Sir StClair Thomson, M.D.)

(From the Finsen Medical Light Institute (Ear, Nose and Throat Department), Copenhagen.)

Before beginning I have to thank you for the honour you have shown me in inviting me to deliver a lecture before your Society.

The treatment of rhino-laryngological tuberculosis by means of the Finsen light is not entirely new; for, as early as nine years ago, I published the first few results obtained after treatment of tuberculosis of the larynx by the Finsen light; since then my number of cases has greatly increased and the treatment has been thoroughly tested.

The Finsen bath consists mainly of an electric carbon arc-light which is supplied in two sizes, a small one and one somewhat larger.

The small one is used for the treatment of more serious cases, who cannot bear to sit up, as for instance, patients with tuberculosis of the larynx or patients recently operated for tuberculosis of the ear.

As you see it here, it consists of a system of three electric carbon arc lamps, which are connected either in parallel or in series, the latter however only when using a current of 220 volts which will then require a series resistance of 55 volts. The lamps are suspended from the ceiling and have check-rods in a perpendicular plane to prevent the lamps from rotary movement (*see* figs. 1 and 2, p. 29).

The lamps must be capable of a perpendicular movement of one meter so that in the end-positions the distance of the light arc from the floor is 75 cm. and 1·75, but these measurements are naturally subject to alteration according to the height of the patient's couch. Two patients, each on his own couch, can be treated simultaneously by this type of Finsen bath (*see* fig. 3, p. 30).

The couch must be as close to the electric light as possible. The lamps use 20 amperes and 55 volts each. The distance between the light arcs is 550 mm.

Here you see the large Finsen light-bath, where six to eight patients can be treated at the same time, sitting round two large electric carbon arc-lamps, each of 75 amperes and 55 volts (*see* fig. 4, p. 30).

The distance between the light arcs of the two lamps is 600 mm. The carbon-electrodes of the lamps are situated perpendicular to each other, the positive being fitted uppermost, because if direct current is used a crater will be formed in the positive electrode, and from this crater the greater part of the effective rays is radiated.

With alternating current, on the contrary, the light-cone is emitted from the upper and the lower carbon-electrode alternately, and, hence, so much effective chemical light is lost that alternating current cannot be used, without first being converted into direct current.

[1] With regard to the history of the Finsen Bath see Strandberg, *Acta Laryngologica*, vol. iv, fasc. 3, 1922.

No glass or other covering round the light should be used, as the chemical rays would then be absorbed. The lamps must burn with a clear, steady light, and the carbon-electrodes must not be allowed to glow or to soot up.

It is evident that the large lamps generate a larger amount of chemical light than the small ones, but this difference is neutralized by the fact that the patients cannot sit as close to the large lamps as they can lie near to the small ones, because the latter give off less heat than the large ones and it is of importance that it should be possible to place the patients closer to the light of the small lamps, because the intensity of the light decreases with the square of the distance.

On account of the strong light the faces and the eyes of the patients are protected by a paper shade, but otherwise they are completely naked.

They must be placed as near to the light as they can bear the heat, and they must turn themselves round occasionally in order that the light may be equally distributed all over the body.

When using the large lamps of 75 amperes, a room with a cubic capacity of 90 to 100 m^3 and with a floor area of 5 by 4·8 metres will be necessary. With the three small lamps, a cubic capacity of 75 to 80 m^3 and a floor area of 5·5 by 3·5 metres will be sufficient.

Patients with tuberculosis of the larynx, or those who are seriously ill or feverish, lie in the Finsen light-bath the first few days from ten to fifteen minutes, slowly increasing up to the full dose of 2½ hours every alternate day. When they are able to sit up, they may begin with the large lamps, taking a shorter bath the first few times, but they may of course continue with the small lamps, if more convenient.

Patients with other illnesses, for instance lupus vulgaris, where the state of health in general is good, can well begin with thirty to forty-five minutes with the large lamps. After the Finsen bath a tepid shower-bath is always given and the out-patients go home to their work.

In order to be able to treat as many patients as possible we only give a Finsen bath every alternate day, but otherwise I think it is of little importance whether you give a bath every day or only every other day.

After the first baths you will find on the body an erythema of varying strength, ranging from an intense blush to the formation of vesicles. In a short time the patients will be brown pigmented, as if sunburnt, and from then onwards will feel no further inconvenience from the Finsen bath; on the contrary, they will find it delightful in every way.

It might be expected that patients who walk, during the cold season of the year, straight from a Finsen bath to their work would be more disposed to colds, but my observations would seem to show the contrary.

Contra-indications are shown only in certain heart and kidney troubles, i.e., provided that the latter are not of tuberculous origin. But I am not afraid to treat out-patients also, and even when they have a temperature of 100·4° or 100·8° before taking the bath.

I have shown, further, that the Finsen bath, even if uncombined with any other treatment, is an effective cure for rhino-laryngological lupus vulgaris and tuberculosis of the larynx; but if you combine the Finsen bath with some other treatment, many of the patients will get well more quickly. Therefore, when treating lupus vulgaris, I always combine the Finsen bath with surgical treatment or with electrocoagulation simultaneously. I also do this in many cases of tuberculosis of the larynx.

Heiberg and I have demonstrated—also by histological examination—

that the Finsen baths are a thoroughly effective cure for tuberculosis of the nasal mucous membrane, even when uncombined with any other treatment. We have examined the inferior turbinals during various phases of the treatment, in a number of cases, and have shown that this treatment produced a definite cure.

The question of exactly why the Finsen light-bath has this beneficial effect has not yet been solved.

I should like now to mention the results of this treatment when used in lupus vulgaris of the mucous membrane, of the nose, mouth, throat and larynx, and in order to do so I will consider the patients who came to my department as new patients during the years 1913 to 1921.

379 Patients treated for Lupus Vulgari by Light Baths.

Affections of the nose	Mouth	Throat	Larynx	Various regions simultaneously	W.R. —	Stethoscopy +
358	90	92	38	123	361	50

30 Cases lapsed out of 379—Remainder 349.

Unchanged	Aggravated	Died	Improved	Still under treatment	Cured
1	2	5	9	28	304 = 87·1 per cent.

Periods of Observation for 304 Cured of Lupus Vulgaris.

0-3 months	3-6 months	6-12 months	12-18 months	18-24 months	2-3 years	3-4 years	4-5 years	5-6 years	8-9 years	9-10 years
66	26	47	39	33	44	19	18	8	3	1

Recurrence: 25.

Lapsed cases	Still under treatment	Re-cured
1	3	21

Periods of Observation of 21 Re-cured Cases.

0-3 months	3-6 months	6-12 months	12-18 months	18-24 months	2-3 years
7	1	4	2	1	6

I have treated tuberculosis of the middle ear and the temporal bone with excellent results too, but in view of the brief time I have at my disposal, I will not touch upon them here, but content myself with mentioning the results obtained in tuberculosis proper of the larynx. By this term I do not imply the ordinary laryngitis, with some reddening, which is so often found and frequently disappears spontaneously in patients with tuberculosis of the lungs who cough much, but I mean the infiltrative or ulcerative affection of the larynx which occurs among patients with tuberculosis of the lungs.

Here we have in all 100 patients, all with aggravated affections of the larynx.

100 Patients with Tuberculosis of Larynx.

Before Treatment.

Hoarseness	Pain and difficulty in swallowing	Tubercle bacilli in sputum	Tuberculosis of the lungs	Hæmoptysis
97	87	80	100	8

After Treatment

The voice		Pain and difficulty in swallowing		Hæmoptysis during treatment
Clear	78	Cured	32	2; both having had hæmoptysis before treatment
Improved	15	Improved	3	
Unchanged	4	Unchanged	2	

100 Patients with Tuberculosis of Larynx.

77 out-patients; 23 in-patients.

Cured in larynx	Still under treatment	Lapsed cases			
53	10 all improved	37	Improved	19	
			Unchanged	6	
			Aggravated	6	Improved 3, Unchanged 1, Aggravated 2
			Died	6	

Periods of Observation of 53 Patients cured of Tuberculosis of Larynx.

Light baths, 34 cases. Light baths + surgical treatment, 19.

0-3 months	3-6 months	6-12 months	12-18 months	18-24 months	2-3 years	3-4 years	4-5 years
22	5	8	2	4	7	3	2

Recurrence, 3.

Still under treatment, 2. Re-cured and without recurrence in one year, 1.

As you see, ninety-seven were hoarse and many of them completely aphonic before treatment, yet seventy-eight patients regained their voice full and clear; in fifteen cases the voice was improved, while in four patients only did the voice remain unchanged even after treatment.

Thirty-seven patients had pain and difficulty in swallowing before treatment, while after treatment thirty-two were entirely free from these troubles, three were improved and only two patients remained unchanged.

I may add that pain and difficulty in swallowing frequently passes off very soon.

It is noticeable that the only two patients who showed any hæmoptysis had this symptom also before treatment. None of the other patients, however, exhibited any hæmoptysis at all during treatment.

With regard to the seventy-seven patients who were treated as out-patients it must be pointed out that none of them could obtain any kind of sanatorium treatment, and as they lived in poor—and frequently, because of their illness, very poor—homes, they had no money to spend on any kind of special food, and the majority were obliged to work, as best they could, in order to earn sufficient money to support their homes, and this even during the first period of treatment, when they were often very ill indeed.

When they had been treated for some time the majority of the patients were able to work much as before their illness.

The ten patients, who are still under treatment, have all improved, and out of the thirty-seven lapsed patients nineteen had improved, in some cases so much that they were able either to be admitted to a sanatorium or they felt so well that they would not spend more time on treatment.

The words "unchanged" or "aggravated" mean *materially* unchanged or aggravated, while, as you see from the table, nearly all the patients were relieved of their pain and difficulty in swallowing.

During the treatment twenty-eight out of fifty-three increased in weight, some patients only a pound or two, but others as much as 30 lb.

The number of treatments with the light bath averaged about eighty-six.

Fifty-three per cent. of cures of tuberculosis of the larynx is a very large percentage, larger than that of any other treatment up to date. But this treatment has later been thoroughly tested in other hospitals also; in the

Oresunds Hospital for Tuberculosis at Copenhagen Blegvad cured some 30 per cent. of the 110 in-patients.

In conclusion I show some diagrammatic drawings of the laryngeal cases which I have treated.

As you see, the affections of the larynx which I have treated were often in a very advanced stage. No doubt the percentage of cures might be greatly increased in patients with less advanced lesions and treated at an earlier stage.

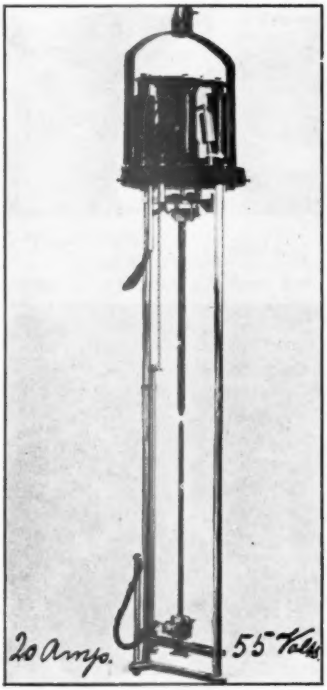


FIG. 1.—Small size.

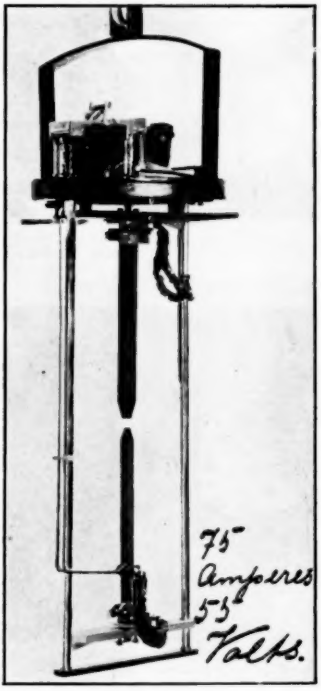


FIG 2.—Large size.

Electric carbon arc-lights used in treatment of tuberculosis of the larynx.



FIG. 3.—Small Finsen light-bath.

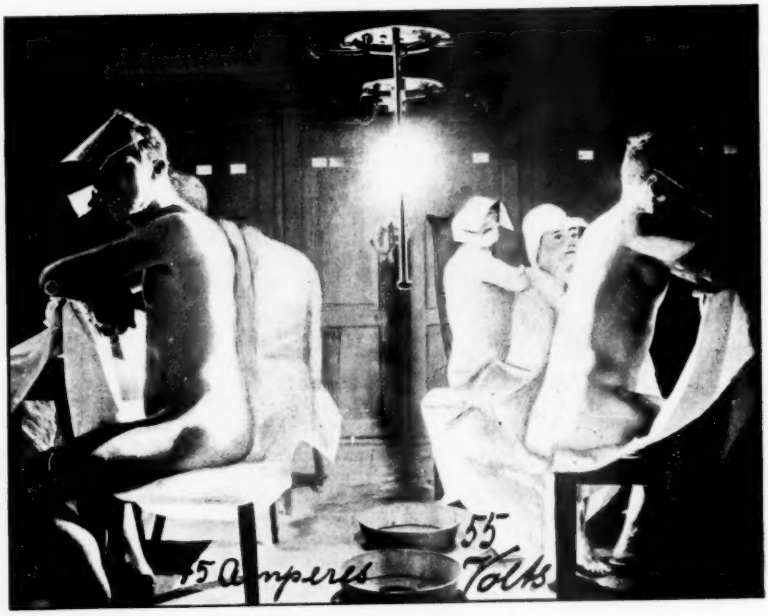


FIG. 4.—Large Finsen light-bath.

SPECIAL DISCUSSION ON "CHRONIC ABDOMINAL PAIN IN NERVOUS WOMEN."

Dr. ROBERT HUTCHISON.

I UNDERSTAND that those who were responsible for arranging this discussion had some difficulty in finding a suitable title for it. Not because we are not all quite familiar with the sort of cases to be discussed—we are indeed only too familiar with them—but because they do not exemplify any definite disease but constitute rather what is generally known as a "clinical syndrome," for which there is, as yet, no definite name. They are cases which I recently ventured to describe as those of the "Chronic Abdomen," and I see that an American writer (Dr. H. Cabot[1]) has lately spoken of them as "Those Painful Women," for they are apparently as common a phenomenon in America as they are here.

As the title finally decided upon for the discussion implies, the patient is a woman who complains of constantly-recurring abdominal aches and pains of one sort or another, very often referred especially to the right iliac fossa. She suffers also from dyspepsia, is profoundly constipated, and often passes mucus in the motions. Her menstrual functions are often deranged. In addition there are remote symptoms of various sorts—headache, "rheumatic" pains, tiredness, nervousness and insomnia.

These are the chief physical symptoms, but the mental aspect of the case is often even more pronounced. The patient is depressed, introspective, extraordinarily self-centred and peevish; is always dwelling upon and magnifying her symptoms and has an insatiable craving for sympathy. She is a connoisseur in doctors, specialists and "treatments"; she has often acquired the operation habit, and although any new therapeutic plan usually relieves her for a time the relief is always very short-lived.

On examination, the patient is found to be undernourished, often of a bad colour, and exhibits a greater or less degree of visceroptosis. The stomach is usually atonic and the gastric secretion rather subacid. There is delay in the passage of the contents through the intestine, especially in the pelvic colon. Displacements of the uterus are common, but the other organs usually present no signs of disease.

A patient of the kind we are considering usually belongs to the upper classes: she is rarely met with in hospital practice. She is generally unmarried, or if married, childless, and there is often in her immediate environment some relative who ministers to her constant demand for sympathy. She will have seen many doctors, has probably had several operations, and will have undergone a great many more or less elaborate and expensive treatments without permanent relief to her symptoms.

Such, in broadest outline, are the cases we are met to discuss, and I think it will be agreed that they present a real problem, both pathological and therapeutic.

[1] *Med. Clinics of N. America*, March, 1923.

In discussing the problem I think it would be well for us to confine ourselves to certain definite lines of inquiry, and I would suggest the following:—

(1) To what extent are the abdominal pains in these women due to an organic cause, such as dragging upon nerves, kinks, and so forth? The results of surgical treatment should help us here. If the pain can be definitely and *permanently* relieved by operation, it is presumptive evidence that its cause was organic.

(2) If not always, or not entirely, of organic origin, what is the cause of the abdominal sensations? Are they due to a low threshold for pain in these patients, so that slight discomforts become magnified in consciousness? And, if so, how is this brought about? Or are they hysterical, a form of "convenient arrangement," as Adler has called it, the result of a subconscious desire for sympathy? The psychologists should be able to throw light on this aspect of the subject.

(3) What is the relation of the physical to the mental state in these patients? Is the mental side of the case the result of the physical, or is it primary? A discussion of this point raises the whole question of the relation of the viscera and the sympathetic systems to the emotions. For example, is it the depressed emotional state in these patients that causes atony of the stomach and intestines, or does visceroptosis and atony lead to emotional depression? If the latter, why do many women with visceroptosis fail to exhibit any mental symptoms? Here again we want the help of the psychologists. The part played by the patient's environment in the development of the mental symptoms has also to be considered. Do these symptoms only appear in an atmosphere of idleness and aimlessness, or where there is an excess of sympathy on the part of relatives? It would be profitable to inquire also to what extent, if any, the artificial menopause which many of these patients have suffered is responsible for some of the mental symptoms. Gynæcological experience would be of value on this point. Or has the mental trauma inflicted by repeated abdominal operations anything to do with them? Dr. Cabot, to whose paper I have already referred, is inclined to think that it has. My own experience, however, would be that identical mental symptoms are often exhibited by women with visceroptosis and mucous colitis who have never been subjected to operation.

(4) As regards treatment, I think we shall all be agreed as to the general medical lines to be pursued, especially the importance of recognizing these cases in their initial stage, of subjecting them to a suitable fattening cure, with the provision afterwards of an efficient abdominal support, of avoiding drastic aperients, and so on. As regards drug treatment, everyone will admit the utility of the bromides, but I should like to raise the question as to whether anything can be done for these patients by the use of endocrine preparations. On this point I must confess myself highly sceptical, but I know there are others who hold a different opinion.

When we turn to surgical treatment there is much more room for debate. I think there will be unanimity as to the undesirableness of all piecemeal operations and the removal of organs which are not demonstrably diseased. Short-circuiting devices will also in all probability be condemned. What needs to be discussed, however, is the degree of *permanent* benefit to be obtained from the fixation of organs, and especially from colopexy. On this point I have myself an entirely open mind. It can only be settled by a careful "follow-up" of a large number of cases for a considerable length of time, and

it would be of great help if the surgeons could provide us with trustworthy statistics of this sort.

Finally there is the question of psychotherapy. We want to know from the psychotherapists to what extent they find these patients benefited by psychotherapeutic treatment, and what form of it is best suited to their needs.

These are the lines on which I think this discussion may most profitably proceed. The problem of abdominal pain in nervous women is a very real one, and up to the present it cannot be said that as a profession we have been very successful in grappling with it. Perhaps by focussing upon it the collective experience of members of different sections of this Society we may come to understand it better and to have more satisfaction in its future treatment.

Dr. J. S. Fairbairn.

I, too, am a little disappointed with the title; the word "nervous" rather grates upon me. The wording should have suggested a mental rather than a nervous element. Also it begs the question; and the surgeons and "pexyists" will take no part in the discussion because they will not consider that this is the type of patient for which their operations are designed. I therefore regret that the transatlantic term or a "slang" term has not been adopted.

Many of these abdominal women are seen in gynæcological practice—which is not surprising when it is remembered that the word "hysteria" is derived from the Greek word for womb—and my experience of them does not differ from that of Dr. Hutchison, except in a few minor points. Besides spinsters or childless married women, others are seen with small or large families and some who could not be said to be comfortably off, with similar symptoms to those described by him. In addition to a general abdominal enteroptosis, there is, in women who have borne children, also some degree of pelvic floor weakness, but with subjective sensations of prolapse out of all proportion to the degree of prolapse actually present. Laxity of the abdominal and pelvic floor muscles, though common, is not invariably present. Constipation and mucous colitis are also frequent. These patients also suffer from over-doctoring and especially from having fancy operations and fancy treatments practised upon them.

I agree as to the extraordinary difficulty of trying to form a just perspective of the physical and psychical elements: such patients can be regarded from almost as many aspects as there are healers of the body and perhaps no two observers will look upon them from the same angle. The mechanical school will find an explanation in some usual or unusual position of the uterus or other organ; the chemical school will look for and find an auto-intoxication or some disturbance of metabolism; the Freudian psychologist will discover some repression or conflict, usually sexual.

The vision of the mechanical school is confined to the anatomical position of organs and is blind to physiology. That school is the curse of gynæcology and is represented by those who do constant fixation operations and hysteropexies; nowadays, however, most gynæcologists admit the large part played by the mental factor in making women over-sensitive to bodily sensations of a disagreeable kind. Spinsters and childless married women, owing to our social system, have been denied the exercise of the sexual or reproductive function and that in itself must have far-reaching effects, especially if there is no adequate complemental interest to take its place.

Another important factor in the gynæcological type is seen especially in

another class, the women with children but with little help in dealing with them. It seems as if the long-continued impressions of the same kind without sufficient relief or distraction by other impressions of a totally different kind are especially provocative of fatigue. In hospital my diagnosis of these cases is "tired mother." For example, the tired mother of four or five young children, with little or no help in the house, has few holidays, and those few are but a change from her own house to rooms at the seaside, where escape from the children is even more difficult than at home. If, in addition, she has anxiety and sleepless nights, nursing sick children or a sick husband, all the factors, mental and physical, are present for the production of chronic fatigue of the higher centres.

Another type is that of the woman who, after bearing her first child, is described by her distracted husband as "never the same woman since her baby was born." She is of the "disgruntled" order, has abdomino-pelvic pains of varied kinds, and is incapable of exertion and particularly of doing anything she does not want to do, because it always aggravates her troubles. That bears out what Dr. Hutchison has said, that a lowered threshold for disagreeable impressions is a characteristic of these women. The one-child mother seems to divide her anxieties between herself and her ewe lamb. To such I frequently advise a "hair of the dog that bit her," in the shape of another baby, but generally the advice is so badly received that the patients are not seen by me again, so that I cannot say that I have been successful.

Dr. Hutchison has raised the point as to the place of the mechanical disturbances of the abdominal and pelvic viscera in producing the symptoms found in abdominal women. In gynæcology, displacements of the womb have been given an undue importance. Professor Clifford Allbutt made fun of the stress laid on them by the gynæcologists of fifty years ago, when he talked about the womb being impaled upon a stem or perched on a prop. His words may equally be applied to the "pexy-merchants" of to-day, for though their methods have changed, their mentality is the same; they merely stitch the unhappy viscus into the abdominal wall, or sling it up by the round ligaments instead of using the pessary of their predecessors.

As there is no pelvic examination of normal women corresponding to the general medical overhaul for insurance or admission to the services, the gynæcologist has no statistical evidence of the proportion of normal women in whom the uterus lies backwards, and hence its ætiological moment cannot be estimated accurately. The displacements have all been given up except the backward one, which, however, has inherited the wealth of the rest of the family in the way of symptoms. What can be said, however, is that many women have the uterus lying back and yet have no symptoms; that many women with the symptom-complex ascribed to backward displacement by the mechanistic school have the uterus in the normal position, and that naturally there are cases also in which retroversion and the symptom-complex are found together. It will be generally accepted nowadays that the position of the uterus, *quâ* position, matters very little. When there is weakness and laxity of the pelvic floor, the uterus drops back, and therefore it must be looked upon as part of the same condition as pelvic floor laxity. The mechanistic school has a primitive idea of the cause of pain and disability which can appeal to the meanest intellect, but it will satisfy only those who look on the human body as a mass-production engine, and overlook all the patients' circumstances and those conditions of stress and strain that are the most frequent cause of wear in the human machine.

What can be done for these people to restore them to efficiency? I agree with Dr. Hutchison that the most important therapeutic measure is a radical change in their environment; they need reassurance and relief from the exacting duties and conditions which bear heavily on them, and they want fresh impressions. In the earlier stages of the war there was an extraordinary disappearance of these women from the gynæcologist's consulting room; many who had made a habit of coming once or twice a year for reassurance and to discuss the changes in their bodily sensations did not come. Since the war the numbers have increased again.

The chief point to be settled is how the confidence of these people in their own bodies can be restored. I am certain that that is the principal means by which the surgeon is successful when he operates. The operation constitutes a powerful suggestion, the patient gets a complete change in her surroundings and a fresh start. If operator and operatee are of the right psychology, and "hit it off," a good effect is produced on both. But with some of these "pexy"-operations, the lady is better for a year or eighteen months, and then she gets sad and depressed because her old trouble has returned, and off she goes to her surgeon, convinced that her organs have become untied, and dreading lest a further operation is required. She is assured that all is right, but that she is now neurotic, and that she must go back and not think about her bodily sensations. The effect of that visit lasts for a time. The dose must be frequently repeated, because the effect does not last very long.

Exercises and efforts to restore the general physical condition are necessary because the most prominent feature in these cases is laxity in the pelvic and abdominal muscles with intestinal stasis. There is no royal road or dramatic cure, but much may be done by those prepared to give the necessary time and individual attention required.

Dr. James Collier.

The earlier neurologists attempted to divide the cases which we are discussing into the following four groups:—

(1) Those in which the condition is without any other basis than the hysterical state.

(2) Those in which the condition includes a delusion of the insane type.

(3) Those in which the natural physical state is one of continual vigilance, apprehension and fear and attention as regards bodily health; and

(4) Those in which there is a primary and real bodily cause for the trouble.

In this classification it will be obvious to you that the three last groups overlap or may be commingled, for an insane delusion may have an organic basis in local disease, and organic disease may be immeasurably more incapacitating when occurring in the subject of vigilant apprehension. The hysterical group is quite distinct and apart from the other groups. The majority of the cases in my opinion fall into the last group in which a real physical cause exists.

The hysterical cases are distinguished by the presence of the definite stigmata of hysteria and, however long lasting, they are curable, and when successfully cured they do not, in my experience, relapse. I give you an example:—

A girl, aged 20, is at the present time under my care at the National Hospital. She was admitted, bedridden for three and a half years with left hemiplegia, and very dull mentally. The hemiplegia was hysterical. During the process of curing the hemiplegia, she complained of severe abdominal pain, which persisted and was followed by frequent

vomiting for a couple of weeks. Then hæmatemesis began and was at times copious. Every meal was vomited and severe general wasting and marked acidosis occurred and the knee-jerks and other deep reflexes disappeared. The condition seemed so desparate that this patient was prepared for abdominal section, and my opinion was then asked for. I directed that there should be no operation but that the patient should be watched night and day. It was soon discovered that this patient lived much of her time with two fingers down her throat with which she scratched her lower pharyngeal veins. She was at once faced with the discovery and told that her hands would be tied and that she would be forcibly fed every time she was sick. The discovery of the fraud produced no resentment in the mind of this patient, in fact it seemed to relieve her. There was no more sickness. She made a rapid and complete recovery, was overwhelming in her gratitude and only remains in hospital to enjoy the Christmas festivities, before taking up a regular employment.

The most interesting point about this case was the loss of the deep reflexes from the bodily dyscrasia resulting from starvation. I have seen one patient die from a similar condition and another from the air-swallowing and distension type of abdominal hysteria, and I have seen others like the case I have related in which prompt and confident measures resulted in recovery from a seemingly hopeless condition.

The cases with insane delusions are not very common, and perhaps do not greatly concern this discussion, but I would like to mention one case which, though it occurred in a man and has no reference to the abdomen, illustrates two points which I consider of great importance. First, the all-pervading effect in some of these subjects of what would be in normal subjects merely slight uncomfortable sensations; and, secondly, that I would extend the symptom-complex which we are discussing to include the regions of the pharyngeal supply of the ninth and tenth nerves and the intracranial distribution of the fifth nerve.

An inmate of an asylum for some twenty years attracted the attention of my late colleague, Mr. Clinton Dent, because he was continually tapping the back of his occiput with his right hand, afterwards shaking his head rapidly and saying "I shall get it loose presently." Mr. Dent was informed that this patient's delusion was that something had become stuck in the back of his head and that for over twenty years he had devoted himself exclusively to the task of shaking it free. Not long afterwards the man died of pneumonia; Mr. Dent was present at the autopsy, when a lesion which blended bone, meninges and brain together was found exactly localized to the spot where the patient tapped his head. His presumed delusion was a reality, the discomfort of which drove him mad.

The group of cases in which habitual self-vigilance and anxiety without adequate cause is the chief condition present are easy to distinguish. Hereditary and familial tendency is often strong, and an easily excitable temperament is always present. The subjects are usually careful livers, even valetudinarians, but sometimes one meets with alcoholism, which is adopted as the only relief from the anxiety. They may be very able and successful hard-workers. Their especial characteristic is, I think, the ease with which their symptoms can be removed by the assurance of any dominant personality for whom they have liking and confidence. Such removal of symptoms is never anything but temporary.

The last group in the classification which I have outlined, or combinations of the last two groups, will cover the majority of the cases which we have in our minds this afternoon. These, both from their symptoms and signs, impel the conclusion that there is something wrong with the splanchnic region of the anatomy. I am sure we shall all agree with Dr. Hutchison that any gross abnormalities of the abdominal viscera which have been found in these cases

are entirely inadequate to explain the symptoms of this malady, and that such gross abnormalities do indeed commonly exist where no such symptoms are present. Yet the malady which we are discussing not infrequently does originate in definite abdominal illness. I have in my mind at the moment the case of a man of fine physique who had never known what it was to be ill and who had led the most vigorous of lives. He had a sudden attack of colitis of the mucous variety, from which it took him two years to recover. Years later he told me that the one sore spot on his memory was the disgusting condition of mental depravity, whining selfishness and love of sympathy, all so foreign to his nature, which had accompanied that illness and which, despite every effort, he had no power to suppress or moderate. I am sure, too, that this malady sometimes proceeds from an exhaustion condition, both inherent and acquired. This will explain the usual effect of rest in benefiting the condition and the temporary good which operations may do in that they necessitate rest. But I have seen cases in which a life of abdominal misery, by reason of this malady, has not frustrated the necessity of hard daily toil and childbearing, and when old age has been reached and the rest from necessity and toil attained, these patients have one and all arrived at a condition of abdominal comfort and mental happiness which was very striking. Dr. Hutchison touches upon a point of fundmental importance when he speaks of the relation of the vegetative nervous system to the emotions, and on this subject I will say a few words.

It is, I submit, universally accepted that all consciousness of well-being, all feelings of happiness, joy and elation, of fear, anger and sorrow, are derived from sensations in the area of the splanchnic viscera, nose, eyes and possibly, meninges. And that these sensations are produced by definite bodily reactions, which can in some instances be perceived and registered by electrical methods. If the bodily reaction which depends upon the functional integrity of the organs in which it occurs does not take place, no feeling rises into consciousness, no emotion occurs. The importance of these emotions as the interests and stimuli of our daily lives is incalculable. Their importance in aiding a healthy action of our vegetative organs is, I doubt not, equally important.

In melancholia, it is believed that these reactions fail completely as the result of some mysterious metabolic dyscrasia, with the result of misery, inaction or complete restlessness and mindlessness.

The ancient Egyptians had some knowledge of these matters, and their soul, though a run-about sort of thing, yet particularly loved to dwell in the splanchnic organs, for which they were particularly careful to provide immortality, without which the soul could hardly be expected to pay the body even an occasional visit. Plato, who derived much of his learning from the Egyptians, theorized that the soul was a trinity with abdominal, thoracic and naso-cranial aspects, each commanding the "nutritional force," the "vital force" and the "mental force," respectively. Which, indeed, correspond with the three regions from which we know the emotions spring.

Hippocrates and Galen derived their ideas from Plato, and they described our malady under the term "hypochondriasis," and introduced the terms "hysteria" and "melancholia." Their ideas that these diseases were due to perversion of the splanchnic organs and disturbance of the soul concerned with these viscera, was generally accepted with some elaborations from the Paracelsian and Helmontian schools up till the time that Burton wrote the "Anatomy of Melancholy," with the divisions of the windy melancholy into the "hepatic," "splenetic" and "meseraick" varieties. In Burton's lifetime the

famous Willis, of the "circle," attacked the splanchnic theory and claimed our malady as a disease of the brain, but he so far conformed to the old ideas as to attribute its ultimate causation to impurities of the splenic blood. The most complete and effective attack upon the old ideas was made by Falret, in 1822, who, in a monograph entitled "Hypochondriasis and Suicide," pointed out the frequency of heredity and of stress of intellectual labour, moral and emotional upsets as factors in the production of the disease, and claimed it for the brain as a variety of insanity. And this view prevailed until recent times, when, with the development of specialists, these specialists have professed and attempted to cure the malady by the methods of their respective arts. And surely there is truth in most of these theories and claims, much truth in the ancient theories, least for the surgeons and for those who would attribute this malady to a fault of the mind.

Assuming that our feelings of well-being, of bodily and mental satisfaction, of rest and comfort in general, are abdominal in site and are dependent for their production upon physical processes and reactions occurring in the splanchnic organs, what will result if these reactions fail wholly or in part, or if they are disordered as the common bodily sensibility may be disordered in disease, with the production of pains, cramps, numbness, dysæsthesia, &c.? It has been suggested that a total failure of such reactions results in melancholia. I would suggest that partial failure, and especially disorder in the normal production of these reactions does produce the almost ceaseless abdominal unrest and discomfort, the pain, cramps, disordered action and secondary anatomical changes which are met with in this disease. The mental effect of these is immensely more important than that of common bodily disorders of sensation, in that the former are disorders of the integral mechanism by which well-being, satisfaction and peace in consciousness are maintained.

The cause of such disorder is hypothetical and still to be sought, but I would suggest that a metabolic dyscrasia with disorder of activation of the many elements concerned, is likely to be responsible, rather than any local abdominal condition, intoxication or derangement of the nervous system. The malady has many factors, both hereditary, inherent and acquired, one of which surely is acute gastro-intestinal illness.

I am certain that I have seen well-marked examples of this malady recover permanently and perfectly and more as the result of time and circumstances than of any particular line of treatment. But these have been a few among the many.

Dr. Crichton Miller.

Palliative treatment always means there is an uncertainty about the ætiology. One mistake there is a tendency to make is to regard these cases as belonging to a single group. Unless the cases are thought of as producing a fairly similar syndrome from many different factors, there will not be much advance in their treatment.

Leaving out of consideration all the cases in which an anatomical lesion is ætiological, I would refer to two types, which may be termed: (1) the physically mal-adjusted, and (2) the psychically mal-adjusted.

In regard to the first, there is the amyotonic group, that in which atonicity is the predominant factor. In this, every variety of visceroptosis occurs, and while these tend to react well to rest, massage, and fattening treatment, they show a tendency to relapse as soon as they resume normal life and posture.

They provide many opportunities for surgeons to practise the "pexy" operations, and I doubt whether there is any logical justification for any "pexy" except "pan-pexy;" any justification for anchoring one organ, simply because it happens to be the first to have expressed its disapproval of its position, seems difficult to find. I agree with Dr. Fairbairn as to the merely palliative nature of all anchoring operations. The aim should be to deal with this atonicity. The work of Vine and Groves is very interesting. It seems to show that this atonicity depends largely on an inadequate calcium function; and if the calcium metabolism can be therapeutically dealt with, not only by the administration of calcium, but at the same time by parathyroid administration, there is a possibility of being able to reach these rather mysterious and extraordinarily common factors of physical atonicity.

Next, there are the vago-tonics, people in whom an inadequate adrenal function produces a chronically low blood-pressure, inadequate thyroid action, &c. These are the people who complain of chronic nausea, epigastric sinking, and so on. Such are the patients for whom, theoretically, adrenal therapy is indicated. I hope some of those present have found much encouragement from the use of adrenal treatment. I have been greatly discouraged by its results, and this I gather is the experience of Dr. Hutchison as well. But whatever may be said of the cases in which the renal function is inadequate, the sympathetico-tonics can be reached. They are those in whom the increased adrenal function produces over-action of the thyroid, with general tension, and failure of relaxation, with a somewhat high blood-pressure. In that group there is a somewhat different class of symptoms. I consider that the only help for those in this group comes from mental treatment. They practically all—all, I think—have repressions, conflicts, &c., which are constantly keeping up the mechanism of the old biological "fight or flight" mechanism. It is one of the groups in which psycho-therapeutic treatment is justified. There is a tendency on the part of some doctors to talk of such pain as "all imagination." That is not a very helpful conception from the therapeutic point of view, because an imaginary disease is a disease of the imagination, and though it needs a different treatment from that required by physical conditions, it is quite as much entitled to treatment as they are. Others go further and say that the pain is auto-suggested, and that is true. It is a duty to find out why any particular woman, or man either, should enjoy auto-suggested pain. Here we come to the important thing on which analytical psychology has thrown light. There were cases in the war with which many were familiar, in which a trauma gradually passed into a neurosis: a fractured arm would heal up, but afterwards there was a mysterious pain. Occasionally it was possible to believe that a nerve trunk was involved in the callus, or a similar happening. Occasionally it had to be concluded that the man had become a malingerer. Still, in most of the cases neither of those explanations fitted the case. The original trauma had offered a defence from a situation from which the individual demanded a defence and it had become too valuable a defence, and so it had to be kept up. I think there is a very large group in the cases now under discussion in whom that is so: a casual gastritis, or dysmenorrhœa, or a pain with an organic basis, occurs in such a way as to offer a valuable defence. And the question should be asked: A valuable defence against what? There are many things from which a patient—a woman in particular—may demand to be defended; parturition, marriage, sexual intercourse, all sorts of things which might at first not appear obvious or likely to the physician. And the more one deals in mental analysis, the more one realizes the necessity for

coming down on to a plane of minute investigation of the patient's reactions to life, to environment and destiny, to be able to find out what is the actual factor in her environment or her destiny which is determining a continuation, as a defence symptom, of some pain which would otherwise have passed off in the natural course.

Dr. E. I. Spriggs (Ruthin, North Wales).

On looking through my cases it appears that out of 988 women patients admitted at Duff House, Banff, and at Ruthin Castle, in the last ten years, eighty-two are recorded as having been nervous and having complained of abdominal pain, roughly 8 per cent. Thirty-three of these had undergone one or more abdominal operations.

From the eighty-two cases I have selected fifty-four, in whom both nervousness and pain were specially prominent and in whom a cause for the pain was not clinically obvious; and it is upon a review of the fifty-four cases, about 5 per cent. of the whole number, that I propose to found my remarks.

We are often uncertain of the intensity of pain felt by others, sometimes even of its existence. For the purpose of this discussion I excluded occasional, fleeting and indefinite pains and pains complained of at one time and forgotten at another. Nearly all of the patients were for some weeks under the observation of doctors and nurses, the impression received at a first interview being often modified later. If, therefore, I attach more weight to the symptom of pain than some earlier speakers have done it may be because the opportunity we have of watching our cases has enabled me to omit from this series some of the less convincing complainers.

It must also be remembered that these patients had all undergone a selecting process before coming to our hospital, many of them having been investigated radiologically and surgically.

My general conclusion was in my mind before I went through the records of these fifty-four selected cases. It has been much strengthened by their re-perusal. It is that, even in very nervous, neurotic, or neurasthenic women, if the complaint of pain is definite, consistent and persistent, if, after watching the patient, I believe in the pain, then there is cause for it which sooner or later is found.

A diagnosis of neurasthenia as an explanation of pain is generally wrong and always dangerous. It is inexcusable until after careful and thorough investigation and observation; and even then should be only provisional.

Nervousness of course may be, and often is, a result of pain. A niggling pain beneath the midriff, if it lasts long enough, will make most people nervous; and the nervousness will not get better until the pain is relieved. A certain degree of apprehension is probably an advantage to the patient. It may pay to be nervous. I remember a strong-minded young woman who flatly refused any treatment for her pyloric ulcer which would involve either lying in bed or operation. Years later it perforated and she went in danger of her life.

There is another diagnosis often made in these cases which is almost as likely to prove wrong, as an explanation of pain, as is that of neurasthenia. I speak of auto-intoxication, or toxic poisoning. It is not really a diagnosis because it does not go back far enough. In a large proportion of cases with pain, in which the illness is named auto-intoxication, a definite mechanical or inflammatory cause is found.

Many of these fifty-four patients had been regarded as neurasthenic for

years, and in a few I concurred in that diagnosis, sometimes apparently correctly, but once or twice to my later regret; and in no less than thirty-two out of the fifty-four some definite irrefutable condition was found sooner or later to explain the pain. These thirty-two do not include the patients with visceroptosis and constipation, who may suffer much from pain, and of whom I will speak in a few minutes.

Gastric and duodenal ulcer, jejunal ulcer, chronic appendicitis and gall-stones account for some of these cases, the biliary tract furnishing, I think, the most unexpected surprises. For example, a nervous woman had been ill for years, on and off, diagnoses having been made of ovaritis, loose kidneys, colitis and neurasthenia. The kidneys had been stitched up, the appendix, tonsils and three teeth removed. We failed to diagnose a stone in the cystic duct, which was excised six weeks later.

Other conditions found were renal calculus, early œsophageal stricture, inflamed duodenal diverticulum, intermittent volvulus, painful blind pouches from intestinal anastomoses, disorders after gastro-jejunostomy, unsuspected bronchiectasis, pancreatitis, some gynæcological disorders, and, of course, various kinds of painful adhesions.

Two cases of volvulus were especially instructive, both in young women called neurasthenic. One had intermittent severe constipation and was cured by removal of a large sigmoid loop which became twisted. In the case of the other an ileocolic anastomosis was made and the patient died of volvulus some years later.

We have proved more than once how painful the contractions of a blind end of an anastomosed piece of gut may be and how it may become enlarged with time. There are, no doubt, other abdominal abnormalities, as yet unrecognized, which give rise to pain.

Pain due to gynæcological causes may puzzle the general physician, but in this series I have generally erred rather in the direction of ascribing pains wrongly to such causes, for example, to an inflamed ovary, when it has been due to something quite different. I think the complaints of those patients who are in possession of the womb and ovaries are on the whole easier to diagnose and certainly easier to treat than they are in those who have lost those organs, at all events in women below the climacteric age.

I now come to the considerable class in whom adhesions are present in the abdomen. There are six definite cases in this series, and many others of lesser degree. Immobility of a movable organ is not enough upon which to diagnose adhesions radiologically; it should be shown that the adherent parts pull upon one another or move together or cause some deformity. Some can be dealt with definitely and successfully, such as adhesions of stomach or bowel to a scar, bands and the like. When adhesions are more general, and of long standing, massage sometimes helps a great deal and sometimes does not. In one such patient mild aperients with oil enemas led to great relief. These cases are difficult to improve by operation, and it is wrong to ask the surgeon to reopen the abdomen unless there is definite evidence of interference with function at a particular spot. When in doubt, if there is no urgency, wait for a time, as improvement, though gradual, may be definite in the course of a few months or a year.

In a number of cases the only abnormalities recognized were general loss of tone, constipation, sometimes with resulting colitis and visceroptosis, of varying degree. Such patients may suffer much pain, either colicky from irregular distension and contraction of the bowel, or dragging pain, or epigastric

pain after food. I do not think that the dropping of the stomach and bowels is the cause of the lack of tone and the constipation, but that the constipation and the dropping are the result of the poor nervous and muscular tone, either inherited, or, more often, acquired by living unphysiologically. If the low position of viscera is not the cause we should not expect an artificial fixation to cure the condition; and, in my experience, it does not. In the first place these operations are frequently ineffective. We have found the slung stomach in the pelvis, the stitched up colic flexures back again where they were, or nearly so. In the second place I believe there is no advantage to be gained, but the reverse, from fixing an organ which is naturally movable as most of the colon is. What is important is not so much where the stomach and colon lie, but whether they can do their work and pass on their contents in a normal time.

That the pain, nervousness and auto-intoxication are commonly due rather to the lack of function or irregular function of the bowel than to its dropped position is shown in two ways. First, by the result of treatment. When these patients get well, by the means I shall mention shortly, their stomachs and bowels are still low, though with increase of fat and of muscular tone, and as a result, in suitable cases, of lavage, the position of the lower border of the stomach may improve greatly, and that of the bowel to some extent. Secondly, the same symptoms occur in patients showing general poor tone and anæmia without much ptosis, and yield to the same treatment. For example:—

A pale, thin spinster was highly neurasthenic—both ovaries had been removed. The blood-pressure was 100 mm. The gastric juice was subacid, the bowel hypertonic, with great delay. In addition to her constipation and pains any over-exertion brought on headache. With general treatment and relief of constipation, weight began to increase, and in a month she was looking and feeling a different person.

It is remarkable what can be done for these sufferers from constipation, auto-intoxication, gastrocoloptosis, anæmia, and neurosis, if time enough be given. The first step in the cure is the elimination, so far as possible, of the suspicion of more serious causes of pain: this alone gives the patient help and freedom from anxiety. Then come rest, fresh air, an anti-constipation diet, the inculcation of a proper habit, gastric or intestinal lavage, as the case may be, massage, abdominal exercises, sometimes the faradic current, suitable medicine, but not chemical aperients, and in due course increasing outdoor exercise. The first step towards recovery is the relief of constipation without drugs, other than paraffin. When this is accomplished, and it nearly always can be, a sense of health and the improvement of nutrition soon follows. For example, a highly nervous woman at the climacteric complained of pain and habitual constipation. With relief of the latter the pain and discomfort vanished, and the mental state became altogether quieter. Here is another typical case:—

A childless married woman alternated between exaltation and depression. She had had an ovary and the appendix removed, and the kidneys stitched up. The gastric juice was subacid, the stomach and colon were dropped, with great delay in the passage of their contents. Treatment led to gradual recovery both physical and nervous.

Time must be allowed for treatment in these cases. The condition is often the result of gradual deterioration for years, and will not get well in two or

three weeks. A young woman with visceroptosis and constipation recovered with ten weeks' treatment, putting on 1 st. 6 lb. in weight. An older woman was suffering from neuritis of the sciatic and other nerves, heart attacks, collapse, constipation, and remitting colitis. She was neurasthenic; any little thing would upset her. As the bowels became regular her nervous state improved. In three months she had no heart attacks, no collapse, the abdominal pain was seldom heard of, and the anæmia, which is common in such cases, was improved.

It is sometimes said that many people are not able to give the time or bear the expense of such investigation, and particularly of such prolonged treatment. To this it may be rejoined that other and ineffective methods of dealing with such cases cost more in the end. I see patients of this class continually who spend many times as much time and money on visits to foreign resorts, voyages for their health, special treatments in vogue at the moment, and operations of various kinds as it would take for them to be treated by their medical advisers at home or in a hospital continuously, perhaps monotonously, but effectively.

There are some cases of the constipation and visceroptosis class in which the patients do not yield to treatment. With them the bowel has lost its power of recovery. These cases are rare, and no one should be regarded as incurable by medical means until adequate treatment, as described above, has received a thorough trial and has failed. If, however, the bowel cannot be got to pass on its contents, as shown by X-rays after prolonged treatment, the right course, in my opinion, is the excision of the inactive part of the large intestine. Excluding cases of volvulus and of intestinal abnormalities resulting from operations, there were two such patients in this series. One of them was found to have the muscle of the ascending colon largely replaced by fibrous tissues: she received much benefit from removal of the ascending and most of the transverse colon. The other patient, who was the subject of great visceroptosis and a constipation which proved intractable, unfortunately died from pulmonary embolism after colectomy.

I must say a word about another group of which there are five examples in this series, namely, women whose nervous and physical state is, in the opinion of the physician, the direct result of psychical overstrain. Such strains occur in human life at all times; they are especially frequent in such years as those we have been passing through. Some of these patients threaten to pass under the care of the alienist; but it is remarkable how much good can be done by rest and by treatment of the physical disorders of the abdominal viscera.

For example, the frail wife of a wealthy and busy public man was unhinged by some years of devoted service to him and the State. She complained of almost continual abdominal pain. The only abdominal disorder found was general sluggishness, and with three months' treatment she recovered in all respects.

A small woman who had lived through the civil war in Ireland complained of extreme nervousness, bad dreams, and abdominal pains. There was severe constipation, and recovery followed suitable treatment and rest.

Among these patients there were several about whom I felt that the physical condition was not adequate to explain the intensity of the pain of which complaint was made. But I can find only one in the series whose abdomen, on a medical and surgical investigation, could be called entirely healthy.

In conclusion, it is suggested that when a nervous woman complains of abdominal pain, the case should, in the first instance, be inquired into, and investigated as completely as possible. No patient should be branded as primarily psychopathic or mental until physical causes of ill-health have been

eliminated. If some definite disease which is not curable by medical means be found the surgeon's help should be obtained at once, as a continuance of pain which cannot be otherwise relieved adds to the neurosis. It is wrong, however, to operate first and investigate after. Ineffective operations make these people worse. It must be remembered also that the inspection of the abdomen at an operation gives much less information about the position and function of the stomach and bowels than is given by a good X-ray examination. I hold strongly that operation should not be advised for constipation and visceroptosis unless the result of adequate and prolonged treatment has shown that the bowel is not capable of passing on its contents in a reasonably normal time.

Dr. Millais Culpin.

From the point of view of the psychopathologist there are two factors in the symptoms under discussion: the bodily changes induced by emotion, as demonstrated by the experiments of Izod Bennett and Venables, and the end subserved by the symptoms.

The bodily effects of acute emotion are commonly recognized: the dry mouth, the intestinal discomfort, the diuresis, the sweating skin, the interference with muscular tone as shown by fine or coarse tremors. The nervous woman is a woman suffering from a continued unpleasing emotion, and in investigating these patients we need to ask about their fears, their worries, their doubts and emotional inhibitions, and we should then find clinical pictures with which the psychopathologist is already quite familiar, either alone or combined with physical symptoms.

The stance of the nervous child has been described by Dr. Hector Cameron, and a similar stance is familiar in the nervous adult. Its marks are the drooping shoulders, sunken chest, lordosis, and protuberant abdomen. Just as the other nervous stance—scoliosis—if uncorrected leads to muscular, ligamentous and bony changes, so this stance leads to the visceroptosis of the adult, with secondary changes in the shape of bands, kinks and sagging viscera. It is probable that the attitude with the resulting visceroptosis may occur primarily in later life, but whether it does or not we can see here the relation between a mental state and some chronic abdominal and pelvic troubles that are supposed to produce pain in nervous women.

We may admit that a common aim is that of obtaining sympathy. But why do intellegent people follow a course which, if we consider it as a result of deliberate volition, is scarcely sane? It is sometimes because they are really sick people and in need of sympathy which they cannot otherwise obtain. A nurse, for example, complained of abdominal pain, for which her teeth were extracted and an exploratory operation was carried out. For the "nervous breakdown" which followed she received three months' Weir Mitchell treatment, and only when given the opportunity of telling her true story did she admit to an obsessional fear of finding her patients dead, to nightmares of corpses and persistent haunting thoughts about dead people. These symptoms had been present throughout but never investigated, and after a few therapeutic conversations she said, "When I had that operation I half knew it ought not to be done." Perhaps one may say without offence that for the mental symptoms she had little hope of help or understanding from friends, relatives or physician, and she was practically forced to seek refuge in the physical symptoms which her own emotions provided. She still has her obsessions, and their exacerbation is accompanied by digestive upset, but it has been necessary to warn her that

there are really conditions for which abdominal operations are necessary, lest she might have an acute appendicitis and drive away the surgeon.

An intelligent lady with mucous colitis, which was emotionally determined by complicated factors, said during treatment, "I used to think it was a physical disease; but looking back I see it was only an excuse."

We should note the therapeutic value of the admission of such motives. They would inevitably be repudiated if prematurely offered as explanations. Contrary to general belief, these patients are often perfectly reasonable people when their true symptoms are considered as disabilities calling for medical treatment.

But there is present in most psychoneurotics a tendency to hide their symptoms under a physical camouflage. I do not venture to say how much this is due to our attitude towards physical disease as something, in contrast to "nervous" disease, for which the patient is in no way responsible, but I would recommend a study of "Erewhon" to bring home to us our attitude towards minor mental disturbances.

Not only the trained but the untrained subject may repudiate anxiety or obsessional symptoms, and the trained subject will combine in a remarkable way the various physical diagnoses under which she has suffered. A consultation with a neurotic patient who has undergone examination in an American team-work institution is most depressing for the psychotherapist.

It is not easy to grasp the conception that the patient may not be aware of the presence of anxiety and that its bodily symptoms may be all that enter consciousness. She may know, however, that anxiety is present, but be unaware of any relation it may have to the symptoms. In the case of the nurse just described, there was no awareness that the obsession, or the anxiety arising from it, bore any relation to the abdominal trouble; but the attention given to her physical condition helped her in a certain degree to bear in silence the more distressing mental trouble.

The absence of psychoneurotic symptoms may be important in making a diagnosis. Persistent abdominal pain in a woman, in whom efficient examination does not reveal such symptoms, may need an exploratory operation, which can be undertaken in the full assurance that it is not a step on the path to chronic invalidism. There are cases in which an organic disease exists in a psychoneurotic patient, but, if she learns that to diagnose symptoms as "nervous" does not convey a stigma or mean telling her that "there is nothing the matter," the patient will co-operate in sifting the physical from the mental or emotional symptoms.

One may sum up with three propositions: (1) A continued emotional state induces changes in the abdominal viscera. These involve secretion, motility and position, and to such changes the word *neurosis* may be properly applied. (2) The symptoms thus produced may be used purposively by the patient, and the purposes are of infinite variety. The one against which we, as clinicians, should be on guard is the concealment of anxiety or obsessional symptoms, which it is our duty to elucidate. (3) Even if we do not cure the mental trouble we may succeed in pushing the symptoms back to the plane to which they belong, and thus remove the patient from the category of the "chronic abdomen."

Dr. J. A. Hadfield.

My experience is confined to cases commonly termed hysterical, most of my patients being those who are declared by physicians and gnyæcologists

to have "nothing wrong with them," or who after operation or medical treatment have been declared "cured," but continued to suffer from chronic abdominal pain.

(1) It is now generally admitted that hysterical pains are *psychogenic* in character. They are frequently called "imaginary" pains, but pains due to the imagination are not necessarily imaginary. The term "imaginary pains" implies that the pain does not exist—a characteristic of the malingerer. But a hysterical pain is a "real" pain, in the sense that it causes acute suffering to the patient even though there may be no peripheral organic cause to give rise to the sensation. Pain, as such, is a psychic phenomenon, a product of the imagination. It may originate as a sensation from a wound or diseased condition of the organism, or it may be produced centrally as a hysterical pain, but in either case it is itself a psychic phenomenon and is as real to the patient in the one case as in the other. Hysterical pains are of this nature. It has been said of such a hysterical woman that "if she thinks she is ill when she is not ill, she must be very ill indeed."

(2) Nevertheless these conditions as I have met them, are nearly always found to have originated in an actual organic illness, though they are no longer due to this: they *originate* as an organic sensation, they are *reproduced* as a mental image independent of peripheral stimulation. We must, therefore, distinguish between an organic *origin* and an organic *basis*. Hysterical pains usually originate in an organic disease, but persist as neurotic pains without any organic basis.

Essentially, there is no difference between hysterical pains in the abdomen and hysterical pains elsewhere, since they are regarded as centrally caused. But there are special reasons why such pains are localized in the abdomen, for the abdomen contains the organs of reproduction, the disturbance of whose functions are a fruitful source of hysteria.

(3) The *sexual ætiology of hysteria* has long been recognized in its very name, the "womb." Experience does not lead me to believe that all hysteria is sexual in origin, but I believe that the majority of hysterical conditions which have to do with the sexual organs are related to disturbances of the sexual functions. One has only to think of the emotional disturbances women have to face in relation to these organs: unfulfilled desires for sex experiences and marriage, desires for children, fear of begetting children, fear of cancer, &c., to realize how fruitful of hysteria disturbances of the reproductive functions may be.

(4) It has further been recognized, quite apart from the "new psychology," that such hysterical conditions are due to the *suppression* of sex desires. This is borne out by the advice frequently suggested by the practitioner as a cure for neurotic women, "all she needs is to get married." This, in the speaker's opinion, is usually bad advice—more hysterias are precipitated by marriage than are ever cured by it—but it proves that the medical profession generally believes in the causal relation between sex suppression and hysteria.

(5) But until recent years it has not been realized that such sexual desires may be completely unrecognized by the patient, being repressed and repugnant to the patient. Amongst the commonest emotional causes of hysterical abdominal pains is a *latent craving for sympathy*. But it is useless to point out what one believes to be the latent cause of the condition. A hysteria may be due to a craving for sympathy, and the patient may be not only entirely unaware of this but may indignantly repudiate any such suggestion. Dr. Hutchison has described wealthy patients of this kind who frequently have at hand those

ready to sympathize. An entirely different and more "genuine" class is made up of those who neither openly crave for, nor expect, sympathy for their pains, but retire to their rooms alone to hide the fact of their suffering. Nevertheless their hysterical pains are frequently found on analysis of its origin to be due to such a craving, latent and repressed. It is futile in such cases to charge them with a craving for sympathy of which they are utterly unaware. Another typical emotional cause of hysterical pain is jealousy, e.g., the condition of a girl with severe pain at the menstrual period was found to have been due to an "affair" with a married man, whose refusal to leave his wife for her made her furiously jealous, and this not only suppressed the sex desire but completely disorganized her menses. Other common causes are *sex traumata* in earlier days; *sadism*, such pains being used as a means of tyrannizing over the household; *masochism*, the instinctive craving and morbid pleasure which some women have to be overmastered by pain, a feature of neurotic life far commoner than is generally realized, or even the patient realizes. [Dr. Hadfield here exhibited "unconscious" automatic drawings of tortures done by such a patient in a hypnoidal condition demonstrating such masochistic cravings.]

As regards treatment, more than one speaker has advocated change of environment for such patients with a view to change in their sensation and their interests. The psychotherapist approves of such an aim, but seeks to alter the mental attitude not by changing environment, but by treating the mind directly by psychological means, and so altering the attitude of mind from a morbid to a healthy one, whether by suggestion or by analysis.

Dr. T. Stacey Wilson (Birmingham).

Although I should much like to comment freely upon the extremely interesting facts and theories which have been brought before us in connexion with abdominal pain in nervous women, I think I had better confine myself to the small contribution which I think I can make towards the better understanding of some part of this subject.

I would throw out the suggestion that if in all cases where there is abdominal pain the colon were carefully palpated in the two situations where this can be easily done (namely in the right and left iliac fossæ) valuable facts would often be obtained, and the diagnosis and treatment of these difficult cases would sometimes be made more easy. The reason for this is that in a number of these cases the colon will be found to be abnormally hard and also tender, and when this is so it is highly probable that abnormal muscular activity of the colon is playing some part, either in the causation of the pain or of the nervousness which tends to enhance the pain; and that treatment of the colon by means of an intestinal antiseptic together with hyoscyamus or some other sedative-containing atropine will probably prove of great service.

The reason for the association of pain and tenderness with hardness of the colon becomes evident when the nature of that hardening is understood. To Sir Charles Sherrington's researches and deductions we owe what knowledge we have of the physiological process which underlies this hardening of the colon. He has demonstrated that a muscular fibre, in addition to its well-known contractile functions, is also subject to another type of activity —tonic in character, but not contractile. In this form of activity the proto-

plasm undergoes a change of condition which is best described as an elastic fixation, for, as the result of a nerve impulse, the fibre may become fixed in any degree of extension or shortening, and when thus fixed is extensible and compressible, returning to its original form as soon as the distorting force ceases, just as if it were simply a piece of elastic.

A very good illustration of the way this type of activity affects the colon is sometimes obtainable during operations upon the abdomen. Mr. W. F. Haslam described to me an experience of this nature while operating upon a patient at the Birmingham General Hospital, in March, 1909, and an account of what happened was written out for me by his house surgeon.

> During an operation a loop of colon which was lying flaccid outside the abdomen suddenly hardened, becoming like a thick-walled rubber drainage tube ¾ in. in diameter, and rose up in an arch some 8 in. long, and 2 in. clear of the abdominal wall at its centre. The muscular bands were visible all the time, both the circular and longitudinal. When forced down it would spring up again just as if it were elastic. When flattened by compression it would at once return to its cylindrical form, when released. Therefore, while these periods of abnormal activity lasted the loop of colon closely resembled a thick-walled piece of rubber tubing. These periods of activity would only last a few seconds and then the gut would lie flaccid upon the abdominal wall until another spasm once more raised it up into an arch.

It is clear that if a considerable number of the fibres of the circular coat were undergoing this elastic fixation much resistance might be offered to peristaltic or tonic contraction, and pain might well result from the conflict between these two opposing types of muscular activity.

It is a fact which is easy of clinical verification that whenever there is a considerable amount of hardening of the colon (due to an abnormal amount of this elastic fixation), pain or some other sign of nervous disturbance is certain to occur.

Relationship to Nervousness.

The presence of this recognizable elastic fixation of the colon muscles bears a double relationship to nervousness. In the first place it is a true reflex just as contractile muscular activity may be. It is due to the action of a stimulant which develops in the intestine, but the amount of abnormal activity naturally depends not only upon the amount of this stimulant, but also upon the excitability of the nervous part of the reflex arc.

The more excitable the nervous system the greater the response to a given amount of stimulant. Therefore the more nervous the patients the more liable are they to abnormal elastic fixation of the colon muscles, and to the pain which it occasions. For this reason colon pains such as those of which we are speaking do sometimes only occur at the menstrual periods, and may easily be mistaken for true dysmenorrhœa. Conversely, the improvement in nerve tone which results from a good holiday may prove curative even for a severe attack of pain of this nature.

The second and more important relationship of this abnormal muscular activity to nervousness is not easily dealt with in a few words. The colon is not supplied with sensory nerves, and its *normal* muscular activities give rise only to various *reflexes* concerned with digestion *and not to sensations*. Abnormal muscular activity will therefore give rise to abnormally great reflex changes of the normal type, and will only produce pain if the afferent impulses from the muscles find their way through the fibres which connect

the sympathetic nerves of the colon with the sensory tracts of the cord or brain.

One of the best known of the reflex results of digestive activity is the mental inertia and physical lassitude which one sometimes experiences after a good meal. Mental inertia and physical lassitude are very frequent symptoms of the abnormal muscular activity of which we are speaking, and their frequent recurrence during the working hours of the day, in the absence of abdominal pain, may not only be mistaken for true neurasthenia, but by increasing the mental concentration necessary for the accomplishment of brain work, or physical exertion, may be a potent cause of true neurasthenia.

Again, is there not a well-known saying to this effect—"All that lies between man and happiness is often nothing more than a plate of roast beef"? and is there not a distinct relationship between the sense of well-being or happiness and the reception by the brain of afferent messages which tell of satisfactory activities in the digestive organs?

When, however, abnormal activities are occurring the abnormally powerful messages will cause unpleasant instead of pleasant disturbances of the brain-cells, and unhappiness and mental depression or mental misery of various kinds result. It is an easily verifiable clinical fact that mental disturbances tending to nervousness of various kinds and even to mental depression or melancholia, may not infrequently be found to be true symptoms of the colon hardness of which we are speaking, and to be curable by measures which cure the colon condition. I would therefore throw out the suggestion that study of the colon along these lines will show that disordered motility in it is frequently a potent factor, not only in the causation of abdominal pain, but also of the nervousness which so tends to enhance the severity of pain from whatever source. The proof of this will be found to lie in the fact that relief of the colon condition will prove the most efficient way of dealing with such cases.

Dr. Robert Hutchison (in reply).

There is not much which calls for an answer. It has been a very interesting discussion and, as I expected, it has revealed a considerable divergence of view. Roughly, one may say there are two schools of thought among those present: those who are represented by Dr. Spriggs, who would attribute at all events a very large part of these patients' symptoms to genuine organic disease of some kind, including visceroptosis, and those who regard them as mainly of psychical origin. I cannot help feeling a good deal of sympathy with Dr. Spriggs' view. One has to recognize that in the class of case discussed on the present occasion the majority are people who do suffer from visceroptosis. Why should visceroptosis be constantly associated with the malady unless it be in some way a causal condition? I take it that people without visceroptosis suffer from mental conditions as much as do those with it. I am afraid that some of those who have spoken from the psychical standpoint have not had a sufficiently large clinical experience of the class of patients dealt with in the discussion; but the psychotherapeutists have brought forward evidence to show that pains which might arise in an organic fashion are, for psychical purposes, perpetuated and stereotyped in one way or another.

The conclusion is that, from the point of view of treatment, it must be recognized that the mental side needs to be dealt with as much as the physical.

I regret that no surgeon has taken part in the discussion. One wants to know whether all fixation of organs is totally discredited. I do not feel sure

that it is discredited. I have an open mind about colopexy. I have not seen many cases, but I have heard from persons, whose veracity I do not doubt, that they have cured such people by colopexy, and that the good results of the treatment have lasted for years. If that be true, it shows that dragging on, and displacement of, organs is very important.

Dr. FAIRBAIRN (in reply).

Although colopexy has possibly not had a fair trial, it will probably disappear from surgical practice in course of time, as nephropexy has already done. Hysteropexy, too, is largely being abandoned.

The Royal Society of Medicine.

President—Sir WILLIAM HALE-WHITE, K.B.E., M.D.

SPECIAL DISCUSSION ON THE GRADING OF THE POPULATION FROM THE POINT OF VIEW OF PHYSICAL FITNESS.

Air-Commodore DAVID MUNRO, C.B., C.I.E., R.A.F.

I AM extremely diffident of expressing any opinions on this subject as I am only too conscious of my superficial grasp of it. The subject is complex and far-reaching. It is mixed up with fundamental social, economic and political problems—as any subject which touches the health of our population must be. Some of these problems also have been subjected to close study by the best brains in our profession.

The late war gave us a rude shock in that we discovered the physical state of our manhood to be below what we—what the average man at any rate—thought it to be.

A well-known politician, in a passion of platform rhetoric during the war, declared: "We are a C3 people." This pronouncement became a catch phrase, and though in fact it is untrue for certain classes of the population, for other classes it is pretty well agreed to be true. Thus, while miners and agricultural labourers were found to have a high index of physical fitness, as judged by Keith's standard, those engaged in sedentary industrial occupations were found to have a low index.

The report issued by the Ministry of National Service upon the physical examination of men of military age by National Service Medical Boards is the most comprehensive survey that has yet been attempted on the health and physique of a large portion of the population. Records of some 2½ million examinations carried out on men between the ages of eighteen to forty-five were analysed. To quote the words of the report:—

> "As the result of this analysis the conclusions come to were that of every nine men of military age in Great Britain on the average *three* were perfectly fit and healthy; *two* were upon a definitely infirm plane of health and strength whether from some disability or from some failure in development; *three* were incapable of undergoing more than a very moderate degree of physical exertion and could almost—in view of their age—with justice be described as physical wrecks, and the remaining man was a chronic invalid with a precarious hold on life."

As matters have existed in the past only the stress of a great national struggle with the necessity for the fullest use of man-power could have brought into existence, and caused to function, the machinery for conducting such a large number of medical examinations, and for analysing their results, and you may well be thinking—"What practical possibility is there in peace time of obtaining

records to show the state of physical fitness of any large section of the population and, if obtained, to what practical use could they be put?"

I shall endeavour this evening to give you a sketch—it can be no more than that—of (*a*) the value of such records, if obtained; (*b*) the amount of information necessary to be recorded; (*c*) the possible method of obtaining and recording it.

(*A*) The Value of Records of Physical Fitness.

(1) It is obvious that the picture presented would indicate the directions in which efforts should be made to improve health generally, and as such the figures would be of great value to the Ministry of Health.

(2) To the physical culture teachers these figures would be of value in giving them a picture of the different classes, also in recording progress obtained in training. Men at different stages of physical and mental fitness require different methods of training.

In this connexion my attention was first really drawn to the subject by an excellent paper read last year before the War Section of this Society by Colonel Campbell—officer in charge of Physical Training in the Army—in which he advocated the ideal of a uniform standard of physical culture for all countries.[1]

(3) Figures of this kind, giving information of the *fit* classes of the population, would undoubtedly be useful in the future in the consideration of eugenic problems. One cannot forecast what influence Malthusian and neo-Malthusian doctrines may have on future policies.

(4) Information of value to industrial psychologists would be obtained, and, I think, the interests of the public safeguarded as I shall suggest further on, against the errors in human judgment, which are apt to result in accidents and loss of life—I refer to those sorts of accidents, the avoidance of which is intimately dependent on the sound physical and mental health of *one* man, e.g., a railway signalman; a factory machine operator; a motor-car driver or civilian air pilot—those employments into which the element of public safety enters.

I have purposely avoided mentioning the use of these figures in national emergencies such as the late war, as this question is, I think, outside the scope of this Society to discuss.

(*B*) The Amount of Information Necessary to be Recorded.

Physical fitness is a quality which is not capable of precise measurement. There are, it is true, certain qualities capable of exact measurement such as height, weight, and girth of chest. If these qualities *only* were taken into account it would not be difficult to obtain figures and to work out a mean, and, by giving percentage values to measurements above and below this mean, to assess exactly the physical fitness of the population judged by these standards alone, and to classify them accordingly.

Sir Arthur Keith's standard, as I understand it, was based on such measurements—in fact based on height measurements. Quoting Sir Arthur Keith's own words as contained in the "Report of the Ministry of National Service," he says: "We have every reason from past inquiries to suppose that physical fitness—the quality which separates men into classes, will be distributed exactly as are certain qualities capable of exact measurement—such as stature," and his "index of fitness" was based on the percentage of men who came up

[1] *Proceedings*, 1922-23, xvi (War Section), pp. 31-43.

to the average as regards stature. But while physical qualities do lend themselves to exact measurement physical disabilities unfortunately do not. To refer again to words in the Report already quoted :—

"To give an exact percentage value to every known disability would present insuperable difficulties in practice. The health of individuals and of each of their component structures and organs shades off gradually, and often almost insensibly, into disease, which in its turn exhibits varying degrees of intensity which are not sharply defined from each other, and react differently on different individuals. The medical interpretation of objective clinical phenomena does not, and cannot, lend itself to expression in figures."

The borderland of disease is still largely an unexplored territory. What, then, is it necessary to do in the way of getting data about physical fitness? Are we to depend on a few measurements of certain definite physical qualities such as height, weight, &c., or could we try to institute a clinical examination of every system to discover disabilities? The latter is plainly impossible, for whatever records are desired to be obtained they must—if they are to be of large numbers of the population—be of the simplest character. Otherwise it would be impossible to set up the machinery for obtaining and recording these. They must also, as I have just said, be capable of simple and numerical expression—of which the results of clinical examinations are not.

My only excuse for presuming to open the discussion this evening is that I believe that we have—in certain of the tests in use in the Royal Air Force for assessment of the physical fitness of air pilots—tests, simple in application, the results of which are capable of numerical expression, and which at the same time give extensive information as to the state of health of the individual tested. According to our periodical results of these tests, supplemented by a record of observed physical disabilities, my feeling is that individuals *might* be classified from a health point of view. Before proceeding to describe these tests, however, I had better try to define what I am talking about when I use the words "physical fitness."

The tests I am about to describe, for instance, do not give results indicating in the subjects any special aptitude for special employments, and therefore from the point of view of the industrial psychologist, whilst they would be valuable in picking out the man who was physically and mentally fit for arduous work, and could do day and night shifts, they would not pick out the man likely to become a skilled manipulator. For these, accessory tests—such as reaction-time tests—would need be employed. I should prefer to use the words "physical and mental endurance" rather than "physical fitness"—I mean that state of health which makes a man able to perform an act of physical exertion not once or for a short time, but repeatedly over a long period of time without undue physical or nervous stress. This is a state of health which, as we know in the Services, requires personal care and preparation with a view to that end. Even then every man has his breaking point, and the fit man is the man whose breaking point takes longest to supervene. These particular tests were originated after examination of men who had manifested such endurance, viz., star-turn pilots.

The tests which I would suggest are two in number, viz., (1) the "endurance" or mercury U-tube test, and (2) the response of pulse to exercise, or exercise tolerance test.

(1) *The Endurance Test* is performed as follows: With the nose clipped, the subject is asked to empty the lungs completely, inhale fully, blow the mercury in this tube (here shown) to 40 mm. and maintain it there without breathing for as long as possible, the pulse being counted in periods of five seconds during the performance of the test.

The average time of holding up the mercury in a large number of cases tested is between 50 and 60 seconds. The pulse-rate should remain steady or rise gradually according to the time the breath is held. A marked rise is unsatisfactory, and a still more unsatisfactory sign of cardio-motor instability is a marked rise during the second or third period of five seconds, followed by a dying away to normal or below normal.

The test is recorded numerically in two ways:—

(1) The number of seconds during which the mercury column is sustained to 40 mm.

(2) The number of pulse-beats in each five-second period. What information does this test give? We believe that it affords information as to the stability of the centres controlling respiration and circulation directly; and, indirectly, of the stability of the nervous system generally. We believe also that it has a psychological value in that it indicates resolution to "carry on" under difficulties and the power of endurance to fatigue. Men vary greatly as to the length of time they will "hang on," though obviously in respiratory and circulatory distress. From this point of view it is a mental as well as a physical test. For the detailed physiological explanation of the phenomena of this test I must refer you to its inventor, Group-Captain Martin Flack, but as I understand it the factors involved are:—

(i) The state of oxygenation of the subject's blood, e.g., no man's blood is 100 per cent. oxygenated; the fit man's may be 98 per cent., the less fit man's 96 per cent. and so on. I have seen a very interesting record of one of these tests, the 40 mm. mercury test: 8, 8, 12, 14, 15, 4, 4, 2. That was one of the worst types. This man, it turned out, was working over a gas stove, and 20 per cent. carbon monoxide was in his blood. This practically proved that the test depends largely on the state of oxygenation of the patient's blood.

(ii) The state of his respiratory efficiency. Good respiratory apparatus enables a fit man adequately to empty out his lungs and fill them up again, and therefore to start the test with a lower percentage of CO_2 in his lungs and a higher percentage of oxygen than an unfit man. (It has been proved by Flack's experiments in re-breathing in a closed bag that unfit people cannot endure so low a partial pressure of oxygen in the alveolar air as fit people.)

(iii) The tone of his blood-vessels. The abdominal and other muscular effort necessary to sustain the column of mercury causes back-pressure effects on the venous system, resulting in an increased flow of blood to the right side of the heart, a consequent rise of blood-pressure and an increase in pulse-rate. In the unfit man with lax abdominal muscles, subject to splanchnic pooling, the obvious result is an abnormal rise of blood-pressure and pulse-rate. If carried further still the stimulation of the depressor-nerve endings in the aortic arch by the raising of blood-pressure causes a reflex slowing of the heart (which explains the dying-away pulse in the most unfit cases).

Whatever be the actual physiological machinery functioning in this test, empirically it has proved of great value to us in the R.A.F. as an indication of a man's capability for physical endurance. So far we have only used this test for testing flying personnel, but I anticipate its general use in recruiting centres for all personnel at no very distant date. I also anticipate that a U-tube manometer will in the future be as much a part of the equipment of a general practitioner as is a stethoscope.

(2) *The Response of Pulse to Exercise Test.*—This we perform as follows: The pulse-rate is taken with the subject sitting down and the total number of beats in sixty seconds recorded. If the pulse is unsteady it is counted in periods

of five seconds until the minimum constant rate is obtained. The subject is then directed to stand up, and the number of beats recorded in the first five seconds multiplied by twelve is recorded as the rate per minute for the standing-up pulse. With the subject still standing the examiner now records the lowest steady rate to which the pulse falls. Regulated exercise is then carried out by the subject placing one foot on a chair and standing alternately on the floor and on the chair five times in fifteen seconds, the speed being regulated by the examiner raising and lowering the subject's wrist in every three seconds. At the end of the fifteen seconds the pulse is counted as described above in periods of five seconds—the time which it requires to fall to its steady rate being recorded in addition to the rate per minute in the first five seconds after exercise. A typical numerical record of this test in a fit man would be as follows:—

(*a*) Sitting 72, standing 84-72, after exercise 96.

(*b*) Time of return to normal standing rate, 20 seconds. The reason that we made this test five times up and down only on the chair was because at the time we were catering for the examination of several hundred men a day.

What information does this test give? A pulse which rises rapidly on standing and takes some time, e.g., 1-2 minutes, to return to its lowest steady rate probably indicates poor splanchnic vasomotor control—in such cases the pulse maintains its quicker rate during standing. If the time of return to normal after exercise exceeds 30 seconds we regard it as suggestive of cardio-vascular insufficiency or of nervous instability. Further, this test tends to differentiate between true and false tachycardias and to throw light on various disordered actions of the heart, of which one heard so much during the war. Possibly, also, it would assist in assessing the seriousness or otherwise of valvular lesions as expressed in terms of "bruits." I have heard that these tests have been employed in America by insurance companies, who find a definite correlation between them and the expectation of life.

To sum up—these two tests are:—

(1) Simple in application.

(2) The results can be expressed numerically so that they are capable of being used for health classification purposes.

(3) They can also be easily codified for statistical purposes and thus made ready for dealing with large numbers by a statistical machine such as the Hollorith.

(4) Apart from gross disabilities and special qualifications such as vision, they give a good indication of the general neuro-muscular and constitutional tone of an individual at the time of examination. I can state this all the more confidently in that the standards of fitness in the R.A.F. were set by the examination of individuals who had successfully withstood considerable physical and nervous strain in contradistinction to those who had broken down.

(5) The psychological element in the endurance test is, in my opinion, of real value as a measure of the mental "make up." The Report of the Shell Shock Committee brought out very clearly the lack of attention that had previously been paid to mental standards in the course of medical examinations.

(6) They would be valuable as a comparative test of fitness employed periodically, and I would suggest their use to safeguard the public in those occupations in which the element of public safety is involved, e.g., motor drivers, tram drivers, locomotive engine drivers, railway signalmen, factory machine operators, mercantile marine navigators, and civilian air pilots.

(7) Where employed up to date on a small scale they have given consistent results. In the R.A.F. each officer is medically examined once a year, as a

routine, and these tests applied; and it is noteworthy that examinations carried out by various medical officers in all parts of the world confirm the standard originally set by one medical officer working in a research capacity.

The chief disadvantage of the tests is that temporary toxæmias upset them —mild influenza or even a heavy cold is sufficient to cause remarkable deterioration in results—hence the value of successive records such as we employ. Also, of course, they give no information about specific disabilities and therefore cannot supersede the search for disease. Nevertheless, any signs of physical inefficiency brought out by these tests should stimulate a closer search for the signs of the disease, if any, which is causing such inefficiency.

To complete the picture, therefore, it would be necessary to supplement the records obtained from these two tests by noting and recording any gross physical disabilities and relevant details of past family histories such as would naturally be recorded by any capable medical man, the presence of which would completely alter the value of these tests. These could be easily codified for record. For statistical purposes, in preparing the Annual Health Report of the R.A.F., we give to each disease a code number. This is required for the Hollorith card, for, as you know, the Hollorith is really but a machine for arranging different combinations of numbers.

(*C*) The Possible Methods of Obtaining and Recording the Data Required.

In the Services the obtaining of information is easy. Having decided on the information you wish to obtain you have merely to order people to fill up forms containing it. This is known as "red tape." Nevertheless it results in the collection of information which is valuable, according as the forms have been well or badly filled up. If it is medical information that is asked for, the statistics, once they have been compiled from the forms which have been rendered, constitute the basis of future medical policy—though no one who fills up the forms ever believes this.

If the filling up of forms is resented in the Services, in civil life it is hardly even tolerated, and I feel that the mere suggestion by me this evening that in order to collect the required data it would be necessary to institute forms stamps me as a bureaucrat. It would, I fear, be quite impracticable to attempt to conduct any examinations annually on every man, woman and child of the population—not only from the point of view of the difficulty in setting up the agency but also from the point of view of expense. I imagine that the most that could be undertaken would be examination of employed labour between certain ages. Little advantage also would be obtained by endeavouring to extend the inquiry further, in view of the already mentioned extent to which the results would appear to be useful.

I hesitate even to express an opinion as to the agency that could be used for collecting the data. Whatever agency were employed the co-operation of the Medical Research Council would, I think, be essential. However it may be done, I feel that some time or other some system of health classification of the population will be established—though I do not presume to think that it will be on the lines tentatively suggested by me this evening.

Lastly, you may perhaps say that physical endurance, as I have termed it, is only necessary for a very small portion of our people. For instance, the managing director of a factory may be full of physical infirmities and yet be the guiding spirit of the factory. Nevertheless the factory would not run if his operatives were also infirm. Whilst it is true that bodily endurance is

not essential for efficient mental work—yet efficient mental work is in the end associated with good general health and resistance to disease; and physical energy and endurance must in the long run count for the welfare of the nation.

Sir ARTHUR KEITH, F.R.S.,

thanked Commodore Munro for raising a question which had such a wide range of interest and was, at the same time, of the utmost public importance. As long ago as 1903 General F. Maurice raised the question of physical degeneration in this country, and the Government of the time became so alarmed that it instituted a Commission of Enquiry. The report of that Commission recommended that an examination of the physical condition of the British people should be made forthwith. It was at this early date that he (the speaker) became interested in the matter as a member of a committee set up by the Anthropological Institute. An attempt was made by the Institute to get a survey made, and a machinery by which it could be done was proposed but with no success. Then the war came, and he became a member of the committee which advised the Ministry of National Service about the grading of recruits. The results of the labours of the latter committee were issued two years ago by the Ministry of Pensions. The first point about which it was necessary to get some information was the proportion of fit men which could be reasonably expected to be obtained from the mankind of our population. It might be thought on theoretical grounds, that we should obtain 100 per cent. of fit men from a normal population; but if one applied the data of Galton and Pearson to this problem, to say nothing of the results of experience, there could be no doubt that nature never managed to attain 100 per cent. of successes in any of her efforts in mass production. Therefore they had to work out in theory what percentage of fit men any normal population should produce. He (the speaker) had inferred that as fitness and stature were equally the result of an infinite number of factors, the law of variation which held true for stature would also be applicable to fitness. He had used stature, not as an index of fitness but as a character which would guide one to the law of variation which must hold for the distribution of fit men in the general population. As regards mental and bodily fitness he inferred that they must be subject to the same law of variation as was stature. Taking stature as a guide, it was concluded that in every thousand persons examined, 700 should be of grade 1 standard, about 200 of grade 2, seventy-five of grade 3, twenty-five of grade 4, if the variation of fitness worked out in exactly the same way as stature did. On looking into the grading of the better samples of the population, it was found to be so; from parts of Wales and of the North of England the recruiting medical officers found proportions approximating to the ideal. In other parts of the country very different results were obtained. When the proportions of the grades departed from the normal it might be owing to a bias or error on the part of the medical officers concerned, or as was usually the case, the unfit were in undue proportions.

In these days, when it was not a matter of compulsory service, but of being presented with recruits and of making a choice from among them, the question was as to how to recognize the efficient man when he made his appearance. In former times an assessment was made purely on physical appearance: a certain stature, a certain circumference of chest, a certain expansion of chest, a certain weight were considered essential. He admitted that all these criteria might be fallacious, and one might pass as fit a man who was not efficient. It was necessary, however, to use such criteria in

making the first rough selection. There were certain physical disabilities which, if present, did enable one to know that the candidate could not be efficient.

There always remained the question as to whether the man chosen was really an efficient recruit. It was in this respect that a great advance had been made by the Royal Air Force. The tests Captain Flack had proposed were, he thought, accurate, reliable and effective, and constituted a great advance on anything done before in any country. In this, he considered, one was witnessing an extension of the movement introduced by Sir James Mackenzie, a very common-sense movement, namely, that the chief test of heart, lungs, &c., was, how did they do their work? There might be other physiological tests which gave a clue to efficiency straight away.

He congratulated the Air Force on having introduced what he regarded as the ideal method of classifying men, namely, the physiological method.

Sir Duncan Rhind

said his remarks on this subject were more from the lay point of view, namely, in association with man power in the Ministry of National Service. He was responsible to some extent for the statistics on which the report was based. It had sometimes been said that the statistics were a result of the examinations during the last year of the war, and were not, therefore, an indication of the true average health of the nation. But one fact which emerged could be relied upon, namely, that there was a very serious percentage of men who were not fit for military or other service whatever. Those unfits were always with us; they were examined as they came of age, and he had had evidence that those figures were very closely correct; the percentage of unfits was much higher than it ought to have been, and it was so of youths at the age of 18.

From the point of view of man-power, it was a very difficult problem to produce examination by tests such had been detailed that evening, on which the fitness of the nation could be based. Still, he thought it most important that there should be tests by which the training and development, even of these youths, could be successfully traced and recorded.

Dr. William Fitzgerald

said that perhaps he could speak with some force on this question of grading according to fitness, seeing that during the war he was engaged solely in the examination of recruits in Liverpool, where a great many passed through his hands, perhaps 30,000. He also served on the much-maligned War Office Boards, and he could bear out what Sir Duncan Rhind said as to the percentage of unfit men being far higher than it should be. Even in 1916, when the examiners were left alone, they still found 30 per cent. only of the men fit for general service. In Liverpool he recognized the importance of some effort being made to estimate the fitness of the population; he also realized that to a certain extent they were dealing with a residual population, in the national service sense. He found the boys of 18 very disappointing, as large numbers were unfit for service. In a large measure that was because they were suffering from conditions which could easily have been remedied if the attempt had been made earlier in their lives—conditions affecting the feet, tonsils, ears and nose.

He agreed that the tests outlined by Commodore Munro were most valuable, but he did not think they could be held to replace a sound clinical

examination; they were useful as an adjunct to that, i.e., to the work of the medical boards. Commodore Munro said the tests might be applied in the case of organized labour, but from his (the speaker's) experience he did not think the men would stand it.

Though it was now several years since the war ceased, he did not know of any steps having been taken to remedy the state of things set out in the National Service report. But that was one of the first things which should be tackled: environment, and the prevention of the diseases from which the youth of the country suffered so largely. He spent Saturday afternoons and Sundays in Liverpool, visiting the slums and residences of the rejected, and found a bad condition of home life. If this was to be a physically fit nation, it was necessary to begin with the boys.

Group-Captain MARTIN FLACK, R.A.F.,

said that he might be considered to be trespassing on the opener's right of reply, but he would like to say that these tests were solely designed as adjuncts, and such was laid down in regard to them. The tests now described were to show the physiological condition of the subject of examination at the time, and as likely to indicate how he would stand physical endurance. Perhaps they were empirical, in that they were endurance tests. His work on them seemed to show, mainly, the efficiency of the controlling centres of respiration and circulation. But they showed other things also. In the 40 mm. mercury test, the response was altered if the alveolar content of the lung was changed by putting in more CO_2 and less oxygen. One then got a response to the test which coincided with the expression of opinion, that if the man, to begin with, had the power adequately to empty his lung, and then started with a minimal CO_2 tension and a maximum oxygen tension, he could go on satisfactorily without embarrassing the pulse in any way. The second point concerned the arterialization of the blood. If the man had been engaged in an occupation in which he oxygenated well he went along in the test quite satisfactorily. No man could oxygenate 100 per cent., though racehorses did. If the man were given oxygen to breathe before he performed the test, he could maintain the mercury for twice as long. The tests also gave an indication of nervous control. It seemed that if a man persistently circulated blood which was not well oxygenated, his whole nervous system tended to become hypersensitive. To show this the man was told to breathe in and out of a bag, the CO_2 being absorbed; if his blood was not well arterialized, he would quickly manifest signs of discomfort; he would be hyperpnœic, his pulse would give a bad response, when the ordinary man under similar conditions would show no signs of distress. With a five-litre bag the unfit man would give up in two minutes, the fit man would continue for five minutes, the latter enduring 10 per cent. oxygen without much distress, while the former was distinctly distressed with 13-14 per cent.

There was also the circulatory factor. Marked respiratory inefficiency tended to be associated with laxity of the whole of the trunk muscles; thus there tended to be a pooling in the venous reservoir, and when the man was called upon to make extra effort, there was a sudden overloading of the right side of the heart, the pressure of the blood through the lungs went up, there was a rise of pulse-rate and after a time a slowing of the same, due to stimulation of the depressor nerve. A fit man could hold up the mercury for sixty seconds, the pulse meanwhile not being distinctly increased in rate and the blood-pressure rising 30 to 40 mm. in the sixty seconds. The time for the man who showed he could not endure stress was about thirty seconds, and the pulse response was bad, so that, instead of maintaining a steady 72 to 84, it went

up to 144, or even as high as 156 in ten to fifteen seconds, and the blood-pressure, instead of rising slowly, rose rapidly by 100 mm. of mercury to above 200 mm. Hg. in thirty seconds. This pointed to a marked instability of the nervous controlling centres of respiration and circulation. And as those centres were responsible for adequate endurance, and in so far as those were the people who developed war neuroses, it was a sound conclusion that the people who were to be called upon to endure should, at some time before admission, manifest their potentiality of enduring, by displaying this stability of nervous control.

Sir THOMAS LEWIS

said he came to the discussion in order to be informed as to the state of current opinion.

He had been particularly interested in listening to the remarks on the value of tests of efficiency. If the purpose was to test the health of the community as a whole at the present or a future date, he was inclined to agree with Dr. Fitzgerald that it was not likely to be found practicable; very soon an objection would come from the examinees; they would resent the tests. But if it was a question of judging of the fitness of a body of men who were definitely chosen, such as recruits for National Service, tests were practicable, because they could be employed under compulsion, and they could be used in the case of men claiming pensions from the State. And in that direction there had been more experience since 1919, because, under the Ministry of Pensions, many men were examined from the standpoint of their physical efficiency.

He agreed that exercise tests were often essential in forming a judgment, but that they could not replace a clinical examination. But, equally, clinical examination could not always replace tolerance tests to exercise. It was not infrequent to find men who were suffering from obvious defects of the valves of the heart, such as considerable regurgitation of the aortic valve, and on testing they were found to have almost perfect tolerance of exercise. On the other side one found men who had no observable physical signs of disease of the heart or vascular system, and yet they were soon found to have a very low exercise tolerance.

Two particular tests had been brought before the meeting: the mercury test—of which he had no personal experience—and the chair exercise test. In speaking of tests of exercise tolerance, certain general principles should be remembered. He thought any test which was to prove a man capable of strenuous exercise must come somewhere near giving the man strenuous exercise. The chair test was a simple one, and there were many men who would pass through it, meanwhile showing normal reactions, but if those men were pressed further their reactions would be found to be anything but normal. A general scheme upon which one would work in examining recruits for the Army or in examining pensioners, would be to have a graded series of exercises calculated to produce in each individual signs of distress, and the tolerance would be assessed according to the amount of exertion needed to reach that distress. He wished to offer a word of warning about the chair test; at Pensions Boards there had been trouble repeatedly. Originally, some emphasis was laid on the pulse-rates; the exercise was given, and the pulse-rate before and after noted, and the rate at which the pulse-rate declined to its previous level was also noted. The tendency among officers at medical boards using this test was to rely almost entirely on the rate of the heart, and consequently this part of the examination sooner or later reached the level of an almost useless routine. For it was frequent to find men showing a normal pulse reaction to the chair test and yet having considerable respiratory distress; and

if he (the speaker) had to emphasize one symptom more than another, he would emphasize breathlessness. Sometimes the distress expressed itself in tremulousness, sometimes in giddiness. If one wanted to choose a body of men to go into the front line, the only crucial test would be to put them there and see if they came through! What was required was a short-cut substitute for this crucial test. He thought the tests employed would become more and more effective as they approached more and more closely to the actual conditions of service. If it was desired to know whether men could stand route-marching, he did not know how that could be ascertained except by taking them for route marches in full kit. Certain men obviously would not be able to do it and could be excluded on simple tests; but however tests were graded, mistakes would be made. There were many lessons learned in that connexion when soldiers were returned to the medical officers at Colchester. Every man who came as a patient was put through an exercise test or tests, often elaborate, and from these they were judged as to fitness to go on to the parade ground and be drilled by the sergeant-instructor. But the M.O.'s made mistakes, and they were making them even at the end of the war period, after some 10,000 cases had been put through, and that was because the tests were insufficient. They could not make them sufficiently strenuous, or lasting sufficiently long, because the factor of time came in, as it always did.

The exercise tests chosen were to be employed on a number of men indiscriminately; the form of test chosen might suit one man better than another. For instance, a blacksmith, accustomed to wield a hammer, would, in doing the chair exercise test, use more force than was necessary, and more than a man would who was accustomed to agile acts, and the blacksmith might not shine in the chair exercise test, though he could swing a hammer all day long without particular fatigue. That was one of the directions in which exercise tests to some extent broke down. For the reasons given he had repeatedly advocated, for army recruiting, tests which came as near as possible to the sort of exertion the man who was examined would have to perform. For them he would advocate, as a supplement to the ordinary clinical examination and test exercises, a probationary period, and men should be finally sorted out on the parade ground. As things were at present, a comparatively small number of men being required, the tests could be very stringent. But if the nation were again to be faced with a problem similar to that of 1914, he thought there would have to be some system providing a probationary period, because the stringency of the examinations in the first instance would, naturally, have to be reduced, on account of the time factor.

The need for a probationary period was there for another reason, the reason which had been referred to by Commodore Munro. It was, that men who were somewhat "off colour"—possibly through some infection or other indisposition at the time of the examination, might show up comparatively badly; thus there would be a chance of immediate rejection, whereas at a later examination in better circumstances the reactions might be relatively good.

Another point was the following: If the men were graded according to the exercise tolerance test, it would be found that there was a middle group of men who showed a relatively fair reaction, and yet one could not consider them to be men with a perfect exercise tolerance. If the group lower down were taken, it would have to be divided into two clear-cut categories; there would be men who would stay at that level, and there would be men who, under training, would pass into the category above it.

He wished, for his own information only, to ask Commodore Munro a question with regard to the mercury test. That test had, apparently, given

satisfaction to the Air Force, and it had been largely used by them. He (the speaker) wished to know whether it had been employed mainly on young men, or whether it had been used also in the case of men in their forties. It could not be expected that there would be the same circulatory and respiratory reactions in men in middle years as in the case of men in their early twenties. He would also like to know on what basis the statement was made that the test had given satisfaction: had any deliberate tests been made by which a number of men had been given the test and who, having failed in the test, had been passed on and had not been eliminated. And had the after-histories of such men been recorded? That would be information all would like to hear.

Sir ARTHUR KEITH,

in further comment, said he was glad to see Dr. Fitzgerald present, because of all the medical officers, he sent in the best and most instructive reports from recruiting centres. He (Sir Arthur Keith) also agreed with Sir Thomas Lewis that every recruit should pass through a probationary period before final acceptance. The necessity of such a step had become apparent to many during the late war.

Dr. G. H. HUNT

said that at Guy's Hospital tests had been carried out on about one hundred medical students classified according to their known athletic capabilities; one class consisted of good athletes, another of men leading a sedentary life. In employing the exercise-tolerance test, they used a principle similar to that just mentioned by Sir Thomas Lewis, i.e., they found the amount of exercise required to produce a given effect on the pulse-rate; to obtain this effect a greater amount of exercise was required in the case of the athletic men. There was much less difference in the time for which the members of the two groups could sustain the mercury column, and if, in assessing a man, too much reliance were placed on this, many mistakes would be made; for example, one first-class athlete could only sustain the mercury for thirty-seven seconds, whereas two men, manifestly unfit, held it for sixty-one and seventy-two seconds respectively.

They had also attempted to assess the men according to their vital capacity in the way described by Dreyer, but had found this method of very little value: the average values for the two classes of men differed but little, some of the second-class men would be judged by Dreyer's standard as well above the average, and the man whose vital capacity was nearly the worst of all of those examined was an International Rugby football player.

Another point referred to by Sir Thomas Lewis was the difference in circulatory response to effort at different ages. The ages of the subjects at Guy's varied from 17 to 34; a few investigations had been made on men considerably older, and the results so far obtained seemed to indicate that if a man aged 20 and a man aged 50 were both made equally short of breath, more alteration in the pulse-rate would be evident in the younger man.

Their results on men showed that the effect of exercise on the pulse-rate gave a good but not infallible guide to physical fitness, and investigations on about sixty healthy women, mostly aged between 17 and 30, led to the same conclusion.

Colonel SYLVESTER-BRADLEY

said the discussion had turned largely on matters of recruiting. For some time past he had been engaged in connexion with recruiting for the Army. They had adopted an exercise-tolerance test and used it for some time, but the conclusions were that it gave no indication of the amount of exertion a man could take. It was, however, useful as a test of nervous and cardiac stability.

Instead of asking a recruit to get up and down off a chair, they used a 12-in. stool. Sir Thomas Lewis mentioned that the crucial test was as to how much exercise a man could perform. At the depots that was what was sought to be obtained. The recruit was supposed to be able to march ten miles with his pack on leaving the depot, and the period of training at the depot was the probationary period for the Army recruit.

Mental tests had been mentioned; experiments with these were now being made in the Army to find out the recruit's mental capacity and his educability. This could not be done during recruiting, there was not sufficient time; the tests must be carried out at the depot.

With regard to the tests of bodily fitness, no mention had been made in this discussion of the possibility of certain diseases or disabilities having altered in frequency during late years. Recruiting statistics were of some value in that respect. The incidence of certain diseases for which recruits were rejected on enlistment had increased considerably since 1862, while others, in regard to which medical fashion could not have entered, had decreased. Among the latter were varicose veins. In 1862, it was a condition in about twenty per thousand of recruits, in 1912 eight per thousand, and in 1922 it was eleven per thousand. Before 1872 flat-foot was not regarded as a sufficient cause for rejection. Its frequency was then 6·6 per thousand, and it had gone on steadily increasing; in 1892 its occurrence per thousand was 8·9, in 1902 it was twelve, and in 1922 twenty per thousand. Disease of the middle ear had increased from 0·6 in 1882 to 15·5 in 1922.

Surgeon-Commander DIGBY BELL (R.N. School of Physical Training, Portsmouth)

said that he spoke as one who had had the opportunity of studying these questions of physical fitness for seventeen years or more.

Since 1906 and onwards special work had been done at the R.N. School of Physical Training with reference to the so-called "irregular heart of soldiers" and the effects of oxygen inhalation on athletes, &c., and the question of some accurate tests for standardizing physical fitness had been worked at repeatedly, but no satisfactory conclusion had been arrived at even yet. He came to the meeting in the hope that he would learn something of value, as the question was a national one of distinct importance.

The discussion had been interesting in every way, but he felt it had proved still further that the use of suitable apparatus and instruments for grading the population from the point of view of physical fitness was an almost impossible one owing to the variations of the personal equation, the inaccuracy of mechanical appliances for measuring pulse-rate, &c. It was his experience that no efficient result could be obtained by counting pulse-rates at different times owing to the nervous response in the majority of the candidates under examination completely upsetting the normal rhythm of their hearts and rendering statistics of no value whatever.

The discussion that day had emphasized "practical tests" of fitness, and this he felt sure was the only real and accurate way of grading the physical and mental endurance of large bodies of people. Such tests had been employed in Sweden for several years now, where the individuals were asked to do five feats of physical activity and endurance as a test, namely, run 100 metres in a certain time, run and walk certain long distances in average times, swim a certain definite test distance, and so on. Those who had passed these tests were allowed to wear a certain distinction button on their civil clothes or official uniforms to show they were "fit." The test had to be done every eight or ten years and the button became silver or gold according to the age of the

individual who was thus keeping himself fit. This became a matter of great pride to each individual and was a way of assuring some keenness in everyone —schoolboy or elderly man—to keep himself fit and active.

Sir WILLIAM HALE-WHITE (President)

thanked Air-Commodore Munro cordially for his contribution. Its subject was of great interest to him (the President) during the war, for the whole time the Appeal Recruiting Board sat, whose opinion on these matters was final, he was a member of that Board. What perhaps struck him most was the light-hearted way in which many recruiting officers treated the question of nervous instability. To take a particular case—that of epilepsy. He thought the Board was able to allow appeals more because the appellant was an epileptic than for any other reason. To show how that condition was overlooked in recruiting, he mentioned the case of a man who had had a series of epileptic fits and who had been passed into the Army as an A1 man twenty-four hours before he came into a military hospital—of which he (Sir William Hale-White) had been in charge on the medical side. This man said he had had the fits many times before! The harm which was done to the country by passing into the Army these people in various degrees of epilepsy and nervous instability was very great, for not only had the country to pay for their kit, but to pay pensions to them afterwards, because, of course, they were the class of people who broke down when they got to the Front.

Air-Commodore MUNRO (in reply)

said the discussion had taken a turn which he did not contemplate when he wrote the paper; it had turned largely on the subject of recruiting. He had been thinking of the problem as a whole, with a civil rather than a military mind. He had been wondering whether there could be established some system of finding out the health of the ordinary civil population; its results would, of course, be of great use in a time of national emergency, when it could be switched off to a military use.

In answer to Dr. Fitzgerald, he agreed that the men would object to these tests in the civil population, and, further, the examiners would object to them too. He only intended these tests to be supplementary to the ordinary clinical examination. Any machinery for the purpose, operated presumably by the Ministry of Health, to be worked by panel doctors, would have to be of the simplest character.

As to the tests themselves, "the proof of the pudding was in the eating," and the question to be answered was: Does the man do the job? But in the case of the Flying Service, the man could not be set to do the job first. That had been tried, and sometimes he did not do it. These tests were invented as a result of the examination of the men who had successfully done the job. We said to the Air Force executive authorities: "Give us your best men who have done the job, and we will find out what it was that enabled them to do the job."

Sir Thomas Lewis had asked whether the tests were mostly done on young men. The answer was that they were mostly done on young men, but they were also carried out in the case of every officer in the Air Force, once a year, and before they were allowed to be posted overseas. There were not yet available the statistics which he hoped would be produced. He could not say there was the marked deterioration in the older men up to 45 years, who had come up, which one might expect. The tests had not been applied over a sufficient period to justify him in giving statistics.

In regard to which of the two tests was the more valuable, these were only part of a general examination; it was a multiple examination as to fitness.

Medical
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The Royal Society of Medicine.

President—Sir WILLIAM HALE-WHITE, K.B.E., M.D.

SPECIAL DISCUSSION ON "THE POSSIBLE SUBSTITUTES FOR COCAINE."

Dr. H. H. DALE, F.R.S.

(ABSTRACT.)

IT may seem strange that the question of replacing cocaine by artificial substitutes has excited interest, even in extra-medical circles, which is not aroused in the case of other alkaloids, even when these are, like cocaine, dangerous drugs of addiction. We must admit, I think, that the case of cocaine is in some respects unique. There are other drugs of addiction; but cocaine appears to be the only one in which the therapeutic value depends upon an action of a different type from that which leads to the vicious habit. In therapeutics only its local actions have any serious value, whereas the action desired by the addict is one produced, after absorption, upon his central nervous system. So far from having a positive value in therapeutics, these effects of cocaine after absorption complicate its use in medicine and surgery by producing undesirable and even dangerous symptoms. Theoretically it is not impossible, or even improbable, that a substance might be produced having all the valuable properties of cocaine and none of its harmful or dangerous ones. Cocaine, in comparison with many medicinal alkaloids, is a relatively simple substance, and its structure readily lends itself to the formation of artificial derivatives, and of substances departing more or less widely from its constitution, but retaining the structural features which experiment has shown to be definitely associated with its essential anæsthetic action. Professor Gibson, who is to speak later, will doubtless have something to say concerning the chemical aspect of the problems involved. I take it to be my function rather to indicate some more general points on which discussion may be valuable, and concerning which practical evidence is required. If convincing evidence were forthcoming that a substance existed, having all those local effects of cocaine which assist the surgeon, a lower immediate, general toxicity, and no tendency to produce addiction, I suppose we may take for granted the adoption of such a substitute by the medical profession, and the rapid fall of cocaine into disuse. If we knew of such a substance, we should be anxious to bring it to the notice of our colleagues, and to advocate its adoption, if advocacy were needed. The purpose of our discussion is to consider the various substances put forward as substitutes for cocaine, and to discover how far any one of them, or all of them together, can thus fulfil its functions and avoid its dangers.

It will be convenient to have in mind, during our discussion, a list of the properties of cocaine itself; distinguishing those which are essential for all its applications, those which give it special value for particular purposes, and those which are wholly undesirable. I will number them for convenience of reference.

(1) We begin, of course, with its powerful and long-persistent local anæsthetic action, including therewith its power of penetrating the tissues, which enables it to produce anæsthesia of relatively deep structures, when it is superficially applied to the mucous membrane, whether of the conjunctiva or the respiratory passages.

(2) Next in order of importance we have its property of producing, when locally applied to a mucous membrane, a constriction of the small blood-vessels, producing ischæmia and deturgescence, which have great value for certain surgical purposes. With this we may mention its mydriatic action.

(3) Though not entirely free from irritant properties, cocaine shows little tendency to irritate or devitalize the tissues in an area into which it has been injected.

We come now to its undesirable properties.

(4) Cocaine is a somewhat unstable substance, so that its solutions cannot be sterilized by boiling without danger of hydrolysing it into benzoic acid and methyl-ecognine, with destruction of the local anæsthetic action.

(5) Cocaine is a dangerously toxic substance when absorbed into the general circulation. The danger of such absorption severely limits its use for producing anæsthesia by injection, and is by no means completely absent when the application is made to intact mucous membranes, except when small quantities of dilute solutions are used. The symptoms produced comprise a primary stimulation of the brain, with acceleration of cerebral processes, loss of sensation of fatigue, feeling of unwonted efficiency and general euphoria. This is succeeded by restlessness and garrulity, accompanied by accelerated respiration, quickening of the heart-beat, rise of the arterial blood-pressure and of the body temperature. If the toxic action progresses further, these stimulant actions are followed by depression; the blood pressure collapses, the heart beat becomes slow and feeble, the skin pale and cold, consciousness is lost, and death may ensue from paralysis of the respiratory centre.

(6) Lastly we have the fact that the primary, stimulant effect of a not immediately dangerous dose, when experienced by a subject whose self-control is weak, is apt to leave an urgent desire for its repetition, especially as a relief from the depression which follows the feeling of exalted vitality and general euphoria. I need say no more concerning the resultant addiction and its deplorable effects.

From the very long list of local anæsthetics which have been put forward as substitutes for cocaine, we may select for consideration only those which have had some measure of success. We may further omit the substances of low solubility, such as orthoform and anæsthesin, which are only suitable for use as analgesic dusting-powders. We are left with the eucaines, of which only β-eucaine has had practical success; tropacocaine, the only natural alkaloid which has had any measure of success in this direction; stovaine and the closely related alypine; and, lastly, novocaine and its higher homologue, butyn. Of substances having no chemical similarity of any kind to cocaine, it should be mentioned that quinine, in the form of the double salt with urea, has been used for anæsthetizing the deeper layers of the body-wall in infiltration anæsthesia. But, since cocaine itself would hardly be used in that manner now, we

cannot consider quinine as a serious rival. I imagine that the same is probably true of benzyl alcohol and phenyl ethyl alcohol. Some years ago there was quite a series of publications in American pharmacological and medical journals, advocating the use of these simple and relatively cheap substances as local anæsthetics, either by local application or for infiltration anæsthesia. At the time of the first publications, early in the war, I made a number of experiments in the laboratory, with very disappointing results. I supposed that my technique was somehow at fault; but the fact that, with substances available, which are so cheap, so stable and so free from danger as these, new local anæsthetics of the cocaine type are still being sought, even in America, makes me suspect some excess of enthusiasm in the earlier advocacy.

Confining our attention therefore to the substances having some, even if remote, similarity to cocaine in chemical structure and a general similarity in their type of action, we have to compare them with cocaine with respect to the desirable and undesirable properties which I mentioned above.

(1) In intensity and persistence of local anæsthetic action, it seems to be generally agreed that cocaine stands as yet unrivalled, except, perhaps, by the recently introduced butyn. Nor can the admittedly weaker and more evanescent action of such an established substitute as novocaine be wholly compensated by using it in stronger solution. Novocaine appears to be much more rapidly destroyed in contact with the tissues, so that even when a satisfactory superficial action is obtained it does not penetrate deeply. Tropacocaine has been said by some to be as active as cocaine with local application, but its effect is more evanescent. For butyn an equal anæsthetic efficiency with cocaine has been claimed by American observers, but this claim has not hitherto received consistent support in this country. Further evidence as to its efficiency is very desirable, and it may be hoped that this discussion will produce some.

(2) I have found no record of even a claim for any of the suggested substitutes, that it resembles cocaine in constricting the blood-vessels and producing shrinkage when applied to a mucous membrane. For some surgical purposes this action may be undesirable; there is no doubt that for others it is considered valuable, and even indispensable. It is important that a discussion like this should produce evidence as to whether any of the available substitutes has this action of cocaine, and, if not, whether the defect can be compensated by mixing the locally anæsthetic substance with a suitable proportion of adrenalin. Similarly, it would be of interest to hear, from our ophthalmological colleagues, what importance attaches to the mydriatic action, which cocaine possesses, but apparently none of the substitutes.

(3) Many of the suggested substitutes for cocaine have the defect that their anæsthetic action on peripheral sensory nerve-endings is complicated by a much more seriously harmful action on the surrounding tissue cells than that which cocaine produces. Such an effect is, of course, apt to be intensified when the action is localized by the presence of adrenalin. The data which I have seen suggest that this irritant and devitalizing action compromises the value of eucaine, tropacocaine, stovaine and, especially, alypine. Novocaine appears to be remarkably free from it, and one would expect the higher homologue, butyn, to show a favourable record in this respect.

(4) All the suggested substitutes appear to be more stable than cocaine to boiling in watery solution.

(5) Most of the substitutes have less general toxicity than cocaine. Alypine is probably an exception, and, being practically as toxic as cocaine on absorption, and much more irritant locally, it is not likely to have advantages

for many purposes. Eucaine, tropacocaine, stovaine and novocaine are all much less liable than cocaine to produce bad symptoms if absorbed into the general circulation. The position of butyn is yet uncertain, and it may be hoped that this discussion will make it clearer. Animal experiments seem to have indicated that it is even more toxic than cocaine itself; on the other hand it was claimed that, in human therapeutics, its use had been singularly free from bad symptoms resulting from accidental absorption.

Leaving for the moment the question of danger of the drug habit, we find that, of the available substitutes, several are much weaker in general toxicity and therefore safer than cocaine, and that some of these are sufficiently potent and penetrating in anæsthetic action. But those which have sufficient potency and penetration appear to have the drawback of irritating and devitalizing the tissues, whilst novocaine, which is ideal in this respect, is deficient in power, penetration and persistence of effect. None appears to have the vaso-constrictor action of cocaine or to dilate the pupil. It is obvious that, for some of the uses of local anæsthetics, one or another of these deficiencies may be immaterial; and that by choosing for each separate purpose the appropriate substance, cocaine can be, and to a large extent has been, replaced by safer substitutes. I suppose that for infiltration-anæsthesia and for nerve-blocking novocaine meets most needs. For producing analgesia by intrathecal injection stovaine and tropacocaine have been largely used, and I suppose that cocaine has been altogether abandoned for this purpose. It is obvious that each special branch of practice must speak for itself; and I would suggest that the aim of the discussion should not be merely to discover whether there is any one substance which can replace cocaine for all purposes, but whether for each of the different uses of cocaine there is any less dangerous and perfectly efficient substitute; in other words, whether there is any use of cocaine for which no less dangerous substitute exists, which can efficiently produce the desired effect, even when associated with adrenalin.

In speaking of "less dangerous" substitutes I have in mind not merely the danger of immediate toxicity, but the much more subtle danger of addiction. It is common knowledge that the problem created by the cocaine habit has led the Minister of Health to appoint a special committee to discuss this question, the reference to the Committee being "to investigate the comparative value, for therapeutic purposes for which cocaine is at present used, of various possible substitutes, and the evidence as to risk, if any, of such substitutes becoming drugs of addiction." As a member of that Committee, my duty is simply to ask for information, and not to express any opinion which could even seem to anticipate the Committee's conclusions.

On the question of possible addiction to the substitutes, I think I can state, without indiscretion, that inquiries which I have made myself, from those most likely to know, in countries where drug-addiction is widely prevalent, have failed to elicit evidence of a case of addiction to any of the local anæsthetics other than cocaine. Unfortunately the absence of recorded cases hitherto does not exclude the possibility, and it would be of great interest to know whether any of the substitutes has been found to produce a mental exaltation and euphoria comparable to that produced by cocaine, and providing the basis for its habit-forming property.

Another question of importance in this connexion is that of the extent to which the proper use of cocaine in medicine and surgery involves the danger of producing a drug-habit. This again involves the question as to what is a proper use of cocaine. Probably there are those present who can quote

cases in which the prescription of cocaine, for application by the patient himself, has resulted in the formation of a vicious habit. Such cases may be rare, but I gather that evidence of their existence can be produced. On the other hand, it would be of great interest to know whether a single application of cocaine by the medical man himself, to produce a local anæsthesia for operation or to facilitate examination, has ever been known to result in the acquisition of the habit by the patient. If not, the question necessarily arises whether cocaine itself is indispensable for any of the other purposes for which it can now be used and prescribed, or whether these other uses could be met by one or another of the substitutes. The practical question is, in brief, whether medical practice would in any direction be materially hampered if the use of cocaine, short of its complete abolition, were limited to direct application by the medical man himself, with prohibition of dispensing or prescription for the patient.

The title given to me for the discussion was "The Possible Substitutes for Cocaine." I have assumed that the Council intended us to limit our discussion to those which are hitherto available. The only practical question for us at present is whether any of these, or all of them together, so far fulfil the functions of cocaine that medicine and surgery could do without it. It is obvious that no negative answer, given to this question at the present time, could be final. The synthesis of cocaine itself has only recently been completed, and has resulted in the production of a series of isomeric cocaines. There is still a large field of synthesis unexplored, and at any time the ideal local anæsthetic may appear—potent, penetrating, astringent, and at the same time free from dangerous properties, whether of immediate toxicity or or of habit-formation. Our business is not, however, to speculate on the future, but to decide, on evidence, whether ideal substitutes, or reasonably efficient substitutes, are now available; and, if not, whether the use of cocaine itself can be so restricted as to minimize its dangers without real detriment to practice.

Professor W. E. Dixon, F.R.S.,

said that Dr. Dale, in his very lucid exposition, had really told the meeting the points upon which information was required, and in the few remarks he (the speaker) proposed to make, he would endeavour to pigeon-hole his facts into one or other of the positions which the opener had indicated.

It was remarkable that, up to the present time, there had been produced synthetically no alkaloid superior to those provided by Nature; the chemist could not give a better quinine than natural quinine, nor a better atropine than natural atropine, nor a better adrenalin than the natural adrenalin, and the list could be extended indefinitely. But, as Dr. Dale pointed out, there was more hope in the case of cocaine, because its main action was on the central nervous system; the local anæsthetic effect being, in a sense, an accident. It was a side effect, something which might well be developed along other lines in the chemical laboratory.

Montagazza spoke of the effects of chewing coca leaves as causing "unspeakable beatitude," and he used other phrases equally strong. He (Professor Dixon) had had injected into himself, on two occasions, $1\frac{1}{2}$ gr. of cocaine, and the symptoms had not been different from those of others who carried out the same experiment. He became a little tremulous, a little excited, there was some frontal headache, he was wakeful, but there was no desire to try the experiment a second time. The effect was much the same as that of injecting

4 or 5 gr. of caffein. It was Mariani who popularized coca in Europe; he brought it over from South America and grew it in his own conservatories. It was from him the views came as to the action of cocaine on the central nervous system. Mariani gave Pope Leo XIII some of the drug, and his Holiness presented to the giver a gold medal as an expression of his gratitude, as it assisted him in the practice of his asceticism.

A local anæsthetic was another name for a general protoplasmic poison. The simplest local anæsthetic known was distilled water, but it had no practical value as an anæsthetic, as its action was not sufficiently selective on nerve-fibres. The endeavour must be to find other substances which, while killing all tissues, yet destroyed or threw out of action nerve-fibres considerably sooner than they affected other tissues. It was for that reason β-eucaine, tropacocaine and stovaine were generally regarded as not satisfactory, because in the doses necessary to produce anæsthesia they produced some irritation on other tissues. The most powerful local anæsthetic known, a substance at least twenty times—probably thirty times—as powerful as cocaine, was eucuprinotoxin, for a solution 0·03 per cent. would completely anæsthetize the eye, yet it was useless, because not only did it destroy the function of nerve-fibres in those minute doses, but it also injured other tissues—it was not sufficiently selective.

These local anæsthetics, like eucuprinotoxin derivatives of quinine, acted by paralysing the nerve endings, and if the nerve was a mixed one, they always paralysed the sensory fibrils before the motor ones. It was always an axiom in medicine that injury to a mixed nerve structure paralysed first the structures dealing with pain, then those with other sensations, and the last to go were those of movement. The same law held in the case of local anæsthetics.

Dr. Dale mentioned the objections to cocaine: the general toxicity, the absence of sufficient selective action, its slightly irritant action, and the fact that it led to addiction. Dr. Dale also said that no case had yet occurred of addiction to other local anæsthetics, remarking that heroin had to be in use at least fifteen years before there was a single case of heroin addiction described. Yet heroin was an ideal drug of addiction. For instance, the morphine addict had a motion of his bowels about once a week, but the bowels of the heroin addict acted practically normally. He (the speaker) thought that before the war pure cocaine addiction hardly existed; it practically always followed on morphine; the addict commenced with alcohol, went to morphine, then to cocaine, and later rang the changes. It was in France during the war that cocaine addiction became so marked. Thence it passed to England. The samples which were sent to him during the war, and which were largely used by the *demi-mondane* of Paris, were invariably meant to be 1 gr. powders, and apparently they were used either by snuffing, or to put into beer or other drink. The object was to produce exhilaration, and to make the man regardless of consequences. Soldiers returning from the Front, often in a highly emotional state, found alcohol and gay company insufficient to cause them to lose their unpleasant memories, and they found that cocaine produced what they desired. But when the debauch was ended, they had no desire for more cocaine. They could not be called addicts. A man might on occasion become drunk and yet not be a drunkard. Since the war, however, cocaine snuffing had become much more rife. In Germany it did not start at all during the war; it existed in Berlin now, but entirely as a post-war phenomenon. Professor Hahn said he did not think there were more than 6,000 cocaine

addicts in Berlin, and that outside that city they scarcely existed. He believed it was the same in Paris and London; in and around the big cities there was cocaine snuffing, which was a hideous vice, but not necessarily one of addiction.

What were the characters of an addiction drug? It would be agreed that an addiction drug must show one of two characteristics. First, it must depress one's higher faculties of mind, one's controlling power and choice, in order to liberate the more primitive emotions. Pleasure was derived from the expression of the emotions, and so long as we repressed our natural emotions, as we did in the present state of civilization, everything which freed them produced a pleasurable sensation. It was of little use to take substances which depressed all nerve-cells as a result of dissolving into the lipoid of the cell, like chloral hydrate or veronal; one must take something which selected the higher faculties of mind, and liberated the others. The other group consisted of something which caused a powerful excitement of the cortex of the brain of the emotional and relatively lower centres. Caffein did not come into that category, it only excited the higher faculties of mind and even improved control. Under the influence of caffein a man followed more definitely the conventions of the world than without it.

Butyn was a substance at which Dr. Copeland had been working; it had a stimulant action on the brain cortex, and was in some respects in the same position as cocaine. Its effect was temporary like that of novocaine. The animals to which butyn was given had convulsions. Under the influence of an anæsthetic the convulsions did not occur, nor did they occur if one cut the crura. If the animal were anæsthetized and one cerebral hemisphere destroyed, and if it were then kept lightly anæsthetized with ether, butyn injected caused convulsions which occurred on the side opposite to the whole hemisphere. So there seemed little doubt that butyn caused convulsions by stimulating some portion of the cortex of the brain, and that was followed after a time by mild depression. Also, in relatively small doses, butyn temporarily paralysed nerve ganglion cells throughout the body; after a small injection the splanchnic nerve ceased to act, though post-ganglionic fibres were active, the vagus acted in a diminished way, so also did the sympathetic in the neck. One characteristic about butyn and novocaine was the remarkable evanescence of their action.

There were two objections which had been made to butyn. The first was that it caused increased secretion; excessive salivation followed its absorption, and when put into the eye there was some secretion of the lachrymal gland. The other objection was that sometimes it did not act, especially if it were used in strong doses. The explanation he believed to be, not what the advertisement of the proprietors stated, namely some psychic effect, but because butyn was precipitated, especially in strong solutions, by common salt; in a 0·9 per cent. solution there was also some precipitation of proteins and tissue fluids. Five per cent. butyn added to egg albumin caused a white precipitate.

In conclusion he wished to mention the effect of butyn on the nose. Dr. Copeland had devised a very practical method of showing the effect of drugs on the mucous membrane of the nose. He trephined the frontal sinus, screwed in a small tube, then, at a small pressure, allowed the solution to pass in and drip out from the animal's nose, the head of which was hanging over a table. In that way he could accurately measure the amount of fluid passing per minute at a constant pressure. Stimulation of the sympathetic nerves in the neck at once increased the flow of fluid 200 per cent. to 300 per cent.

Dr. Copeland put 5 per cent. butyn in the nose and left it there five minutes and the result was that the flow of fluid was for all practical purposes stopped; the mucous membrane was so congested that little fluid could find its way through. When it was in that condition adrenalin had very little effect. Possibly in the nose these strong solutions of butyn were sufficient to paralyze the sympathetic endings, so that the butyn ceased to exert any action. All the local anæsthetics served to congest the nose with the one exception of cocaine, and that, for a time at all events, facilitated the flow.

Professor C. S. GIBSON.

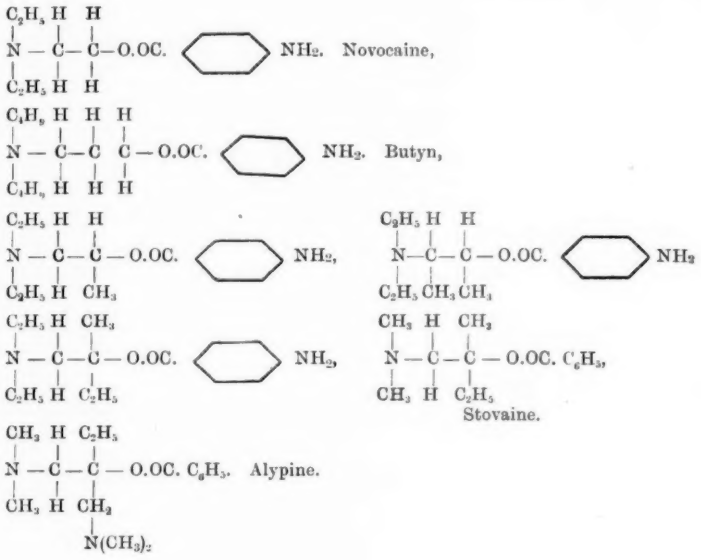
(Chemistry Department, Guy's Hospital Medical School.)

Although the clinician may be of the opinion that at the present time no synthetic compound is available which can replace the naturally occurring cocaine, it is probable that the chemist can synthesize a substance having all the valuable properties of cocaine and one in which the harmful or dangerous ones are reduced to a minimum. If this end is to be achieved, very much more work will have to be done by chemists in this country. The work of Fourneau in this field is pre-eminent, and indeed nearly all the progress which has been made is either French, German or American. The importance of financing chemical research on local anæsthetics cannot be too strongly emphasized from a national point of view, and a small group of chemists working sympathetically with the clinicians would soon achieve results of the highest value in clinical practice.

The lines on which chemical investigation is urgently needed are:

(1) A more complete investigation of the *novocaine group* of anæsthetics.

If we consider a few of the many compounds of this type which have been synthesized we see that they are built up with the same nitrogen-carbon chain arrangement.

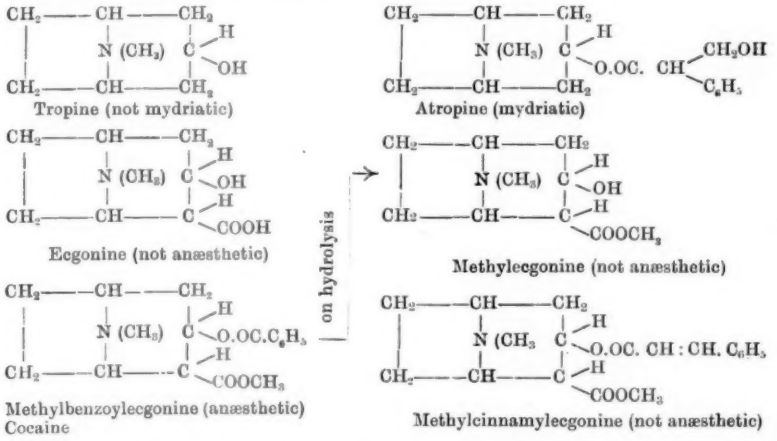


The above compounds, of which the chemical names are omitted, indicate that there are still serious gaps in our knowledge of this type of compound. For example, there are many compounds intermediate in complexity between novocaine and butyn and the chemist is led to ask whether these intermediate compounds have been investigated clinically as local anæsthetics. There is no reason to suppose that the substitution of the *n*-butyl group in butyn for the ethyl group in novocaine is any more advantageous, having regard to the special properties we are considering, than the substitution of other alkyl groups. It is, therefore, most important that the whole series of these compounds should be synthesized and submitted to careful examination.

In all probability the effective cocaine substitute will be optically active and yet none of the compounds of the novocaine group which have been synthesized and which are capable of existing in optically active forms have been subjected to stereochemical investigation. The last five of the above seven compounds are capable of existing in optically active forms and the pharmacological and clinical examination of such compounds cannot be complete until the optically active as well as the externally compensated forms have been examined. King (*Journ. Chem. Soc.*, 1924, cxxv, p. 41), has recently described the stereochemical investigation of the synthetic β and *iso*-β-eucaines, and the relative anæsthetic action of the optically active and inactive forms were compared by Burn. Although, in this case, the optically active forms and the unresolved substances have approximately the same anæsthetic action further information is desirable in many other cases to determine the uniqueness or otherwise of the eucaines in this respect.

Apart from the probability of the ideal cocaine substitute being optically active, the optical rotatory power affords at once a ready means of evaluating samples of the drug.

(2) An investigation which aims at synthesizing suitable compounds based on what may be termed the cocaine model, which, although related is, in many ways, different from the novocaine type.



In the first place, the naturally occurring cocaine is lævorotatory and, as one expects, the synthetic compound is optically inactive. No results of any

stereochemical investigation of inactive cocaine have been published and we know nothing about the anæsthetic properties of the possible *d*-cocaine, which might well prove to be a very desirable compound from a clinical point of view. In any case, it is most important that an extended stereochemical study of synthetic cocaine should be undertaken at the earliest opportunity.

D-cocaine, like *l*-cocaine, would have the drawback of being too easily hydrolysed by boiling its aqueous solution and thereby becoming converted into methylecgonine, which is not an anæsthetic. The chemist, therefore, should aim at synthesizing a compound or compounds of the cocaine type which would not be decomposed when its solutions are sterilized.

There is every reason to believe that amongst the many possible compounds which the chemist is capable of synthesizing and investigating, one or more will prove even more desirable for clinical use than the naturally occurring cocaine. Certainly, no effort should be spared in placing the best material at the disposal of the clinician. All that is needed is the provision by the authorities of the small amount of money by which the research can be carried out by competent chemists.

Local Anæsthetics in Rhinology.

Mr. T. B. Layton

said that the drugs about which they had been speaking that evening raised many interesting problems from the scientific point of view, but, for his own part, as a pure clinician, they were of interest to him almost only as a matter of technique. They were simply a method whereby one desired to get something which would replace general anæsthesia in a way more efficient, more safe, and more pleasant. His own opinion was that the future of rhinology was wrapped up in the question of local anæsthesia, and that they should at the present day look upon this as the normal method of preparing a patient for operation, reserving a general anæsthetic only for those people in whose case it was impossible to do the work under a local anæsthetic. He would be inclined to compare the present position in rhinology with the position of wound treatment in general surgery in the nineties of the last century. At that time there was an antiseptic technique which was more or less the routine method for wound treatment. But there came along the aseptic technique advocated by some of the younger surgeons of that day, and when ten years had passed the aseptic technique absolutely held the field. The antiseptic technique was relegated to the small number of cases for which the former method could not be employed. The speaker believed that it would be the same ten years hence with regard to the use of anæsthetics in rhinology, and that the surgeon who adopted a general anæsthetic as a routine method for taking out the tonsils or operating on the septum would be considered in the same light as the surgeon who, in the early years of the present century, was opening the abdomen under the carbolic spray. If this was the case, some ideal local anæsthetic was badly wanted. He did not suppose they would ever get to their ideal, because before that happened the chemist and the pharmacologist would have learned so much more; moreover, clinicians would have pushed their ideals further ahead and would be calling upon the chemist and the pharmacologist to go still further. But how should one describe an ideal local anæsthetic at the present day? There would be certain differences on this matter amongst clinicians. All would agree that it should be a substance which, with moderate rapidity, would produce anæsthesia without toxicity and

should have no addiction power. The rhinologist would add to these requirements that it was absolutely essential that the drug should have some shrinkage power. Others would say that adrenalin, in the proper amount, could be relied upon for the shrinkage power. The ophthalmologist would say that the most important property in an anæsthetic over and above those named was that it should be non-irritating. That did not trouble the rhinologist to the same extent, although he had to consider it. The dental surgeon, on the other hand, might say that the most important thing was to have a drug which could be depended on not to produce after-pain. Something must be found which would satisfy all these various requirements, and he would like to say how he thought the ideal local anæsthetic should be sought. It was necessary for the clinicians definitely to tell the laboratory workers whether they needed them to work out any new drugs at the present time, or whether those already available were sufficient for the purpose. His personal feeling was that they did need new drugs.

Whether there was at present any drug or combination of drugs which would do all that cocaine would do was a small matter compared with the fact that neither cocaine nor any of its present substitutes could be considered as the ideal local anæsthetic.

For this reason he believed that they must turn to the chemist first, and it was in this connexion that he was so interested in what Professor Gibson had said that afternoon. His remarks gave ground for great hopes, as they showed that there were so many gaps in the series and that as these gaps were filled some vastly improved drugs would be found. He thought, therefore, that they must ask the chemists to make a series of the possible new compounds based both on the cocaine and the novocaine model. Then, again, he thought they must follow the hint put forward that afternoon by Professor Gibson and they must ask the chemists to prosecute with the utmost vigour the stereochemical investigation, both of those compounds already known which were optically active, and also of any new substances which would be capable of existing in optically active form. They might have some stereo-isomerid almost ready to their hands which would be devoid of many of the properties that they disliked in cocaine.

Turning from the chemist to the pharmacologist, he wished to warn them that they must not expect too much from the latter. Useful though he was, and would be, the solution of the problem did not lie in his hands. He should be able to guide the chemist in his choice of new compounds that he selected for synthetization by advising which part of the chemical formula had yielded properties of these drugs; and when the new compound was made he would give the clinicians advice as to whether it had any local anæsthetic and hæmostatic action, and warnings as to whether it had any irritating actions. They would also be able to exclude drugs which possessed any severe toxicity. But they would be unable to help much with regard to the symptoms of minor toxicity, and this for two reasons: first, because these symptoms were due to an action upon that part of the nervous system in which the human being varied further from the animals upon which experiments could be made than did any of these one from another; and secondly, because the clinician was unable entirely to dissociate the toxic from the psychic symptoms, and to say exactly which of the symptoms were due to the drug that he was applying, and which were due to fear, to anxiety, or to apprehension aroused by the ordeal through which the patient had to go and the strangeness of his surroundings.

And yet it was these symptoms of minor toxicity which were the greatest hindrance to the use of local anæsthetics and for these reasons he (the speaker) believed that in the end the clinician must decide upon his own observations upon his patients as to which would be the best drug or combination of drugs for any given purpose.

Mr. F. N. Doubleday

said that recently, at the discussion on a similar subject,[1] he reported that the British Dental Association had issued a questionnaire to the dental surgeons of the country as to the necessity of using cocaine for dental operations. Three-fourths of those who replied said they thought cocaine was essential, and the remaining one-fourth, among whom were most of the dental surgeons attached to hospitals, said cocaine was not necessary, and that efficient substitutes for their purposes had been found. He (Mr. Doubleday) contended that medical men had not yet fully appreciated how valuable dental operations might be as affording a clinical test of the relative value of the various substitutes for cocaine which had been suggested. Dentists could use these substances by applying them to raw surfaces, or for sub-mucous injection, or for deep anæsthesia; they could also employ them on patients for operations of an ascending degree of severity, such as cutting a painful cavity in the tooth, or removing difficult teeth by an operation which involved considerable disturbance of bone.

In the discussion to which he had referred, the clinical aspect was dealt with, and he took it that now, with the President in the chair and Dr. Dale and Professor Dixon speaking, the present debate was specially from the pharmacological side. He hoped Dr. Dale would say, in his reply, whether it made any difference as to the manner in which the sub-groups of atoms were brought into the molecules in novocaine and procaine, i.e., did this grouping make any difference to the anæsthetic or toxic properties?

He had recently been re-trying various anæsthetics of the eucaine group. Many of them were very irritating, though their application was without much pain. Professor Dixon spoke of the precipitation of local anæsthetics, and he (the speaker) had noticed, in using local anæsthetics in various parts of the country, that if there were some slight factor, such as increased alkalinity of water, many of the local anæsthetics were precipitable, and then their therapeutic characters were greatly altered. Dental surgeons thought they had, in novocaine, a local anæsthetic which they could prove to the medical profession was efficient for their purposes, and they would like more opportunities of doing so clinically.

Mr. F. St. J. Steadman

said he had found that novocaine, in many hundreds of injections, was an efficient anæsthetic. Painful procedures, such as the removal of a live pulp, could be done, with comfort to the patient, one minute after the injection. The one drawback he had found in these substitutes was the occurrence of after-pain. He had done something towards mitigating that by using quite fresh solutions, but in some cases the after-pain was intense. He did not know the reason. He had used butyn 200 times, but the anæsthesia from this drug did not develop more quickly than after novocaine.

He exhibited a table of cases giving results in the matter of after-pain.

[1] *Proceedings*, 1923-24, xvii (Section Laryng.), p. 28.

It would be seen that butyn produced a high degree of after-pain. In some cases it had lasted three or four hours, and in others several days, and was so intense that some patients had said that they would not have it administered again to them on any consideration. Therefore he had now given up the use of butyn. He had found that if he used a stock Ringer's solution, or distilled water supplied by chemists, after-pain was more frequent than if he used water which he himself distilled every morning.

He also showed a table of cases in which nitrous oxide gas was used, in order to provide some guide as to how much of the after-pain was due to the trauma of the operation. He could claim that if he used freshly-distilled water, the after-pain from a local anæsthetic was less than after nitrous oxide gas, the reason being that, in many of the cases, the pain was fairly acute immediately after taking out the tooth, and lasted for about an hour after nitrous oxide gas. But when a local anæsthetic was used, the after-pain occurring during this time was masked by the anæsthetic, so that by the time the effects of the latter had passed off, all discomfort and pain had gone.

Mr. E. Watson-Williams, M.C., Ch.M. (Bristol).

Local Anæsthesia by Injection.

It is probably safe to say, that with the important exception of dental practice, the use of cocaine for injection has, in this country, practically ceased. The various products that have already been mentioned have in this extensive field almost entirely replaced it, largely one must suppose on account of their being safer in use. Dr. Dale has mentioned benzyl alcohol as recommended for this purpose: I am myself working with saligenin, and with a French preparation called atoxodyne (said to be an amido-aliphatic benzoic ester)—similar substances. But all such local anæsthetics must be regarded rather as substitutes, if we want them, for novocaine, or whatever the individual may prefer to this, than as substitutes for cocaine.

Surface Anæsthesia.

It is when we come to the domain of local anæsthesia by surface absorption that we find the claim of cocaine as yet supreme. Novocaine has been used, I believe, in the urethra, acoine on the conjunctiva; stovaine and others are on their trial; but so far only β-eucaine, alypin, and butyn have been found to give in the nose and throat a degree of anæsthesia by surface absorption sufficient to be useful. The relative clinical advantages and disadvantages of these three has recently been the subject of considerable discussion, both among Members of this Society and elsewhere; and Dr. Dale has summarized in his opening remarks the views expressed, which appear to be notable for the general agreement discovered.

Clinical Experience of Substitutes.

My personal experience of all these is not large, nor does it differ in any way from that of others; except that I have not used them to an extent sufficient to reveal the measure of their unreliability. Of them all, butyn, in spite of being so toxic that especial care is required in using it—and I do not think that the position demands more forcible language—gives in my opinion a perfect anæsthesia, at least as good as that obtainable with cocaine. It is to butyn that I should turn if neither cocaine nor general anæsthesia was desirable.

Apart from there being sometimes a failure to produce a satisfactory anæsthesia with these drugs, the absence of any ischæmic property—indeed, the distinct tendency to produce congestion—is a very serious defect. It is not discounted by the simultaneous use of adrenalin: even in very dilute solution the latter may be objectionable. These anæsthetics, further, are appreciably irritating, butyn more so than the others. Indeed, the manufacturers of the latter point out this property as likely to prove a deterrent to the addict; a belief, I fear, too optimistic.

Up to the present, then, it appears that no one drug is available that possesses all the clinical advantages of cocaine. As to the suggestion that each of these special advantages may be obtained by selection of different drugs for varying conditions, this course is not without serious objections. We must admit that all these anæsthetics are dangerous unless great care is observed in their application: and therefore it is very desirable that our equipment should be as simple as possible. Not a few deaths from cocaine poisoning have occurred because of a mistake between solutions of cocaine and of novocaine, or between strong and weak solutions of cocaine. And if we are to use several different drugs habitually in every clinic, this source of trouble might easily become serious.

Stability of Cocaine.

A consideration of the relative advantages of the substitutes involves an examination of the disadvantages of cocaine. The stability of cocaine, both for keeping and for sterilizing, is not very much less than that of its rivals. Even novocaine will not stand prolonged boiling, nor will it remain unchanged in the presence of alkali. The really substantial disadvantage of cocaine is its toxicity, and another is its liability to be abused by the addict.

Cocaine Addiction.

To deal first with the latter: It appears to be a matter of the most extreme rarity that the use of cocaine for surgical purposes has led to addiction. At the large gathering in this room on December 7 last, although special inquiry was directed to the point, only two cases could be found in the knowledge of those present, and of those only one was a personal observation. It appears most unlikely that with the recent restrictions on the sale of cocaine, addiction from this cause will be a factor of any importance. Are the substitutes free from this danger? A long time must elapse before we can be in a position to judge this question. Heroin was at one time advocated as being free from temptation. And I would remind you that cocaine itself was between 1888 and 1895 advocated and used in the treatment of morphine addiction! I have myself seen a distinct euphoria after injection of novocaine; in point of fact such a condition is by no means always a disadvantage during an operation with local anæsthesia.

Restriction of Right to Prescribe.

As to whether the clinical use of cocaine should be confined to administration by the surgeon himself, I can only say that I should find myself at a grave loss if I could not prescribe it in conditions of nasal or pharyngeal irritation. But I feel that I must record my opinion, that the suggestion that the Government should prohibit the use in medicine of *any* substance, likely in the opinion of the *medical* attendant to be useful, or necessary for the relief of the patient's condition, is a proposition entirely new and entirely inacceptable.

Toxicity of Local Anæsthetics.

But after all the real objection to cocaine as a local anæsthetic lies in the fact that it is poisonous. And when we turn to the substitutes, we observe that they are poisonous also. What is more, the poisonous property seems to bear a relationship to the anæsthetic power. Alypin is practically as poisonous as cocaine, and has roughly the same anæsthetic value: eucaine is appreciably less toxic, but its anæsthetic power is also less: while butyn, which has an anæsthetic value rather more than that of cocaine, is certainly not less toxic, and in my view is definitely more toxic. Nor at first sight is this a matter of surprise. Clearly, a local anæsthetic must be a substance with a special affinity for nervous tissue; otherwise it would not act as a good anæsthetic. The double salt of quinine and urea at one time advocated for this purpose well illustrates this point. It is not strongly selective for nervous tissue; and has been found to have so much effect on other tissues that it is now rather fallen into obscurity—unless we are to use it for producing submucous sclerosis in the treatment of hæmorrhoids!

Estimation of Toxicity.

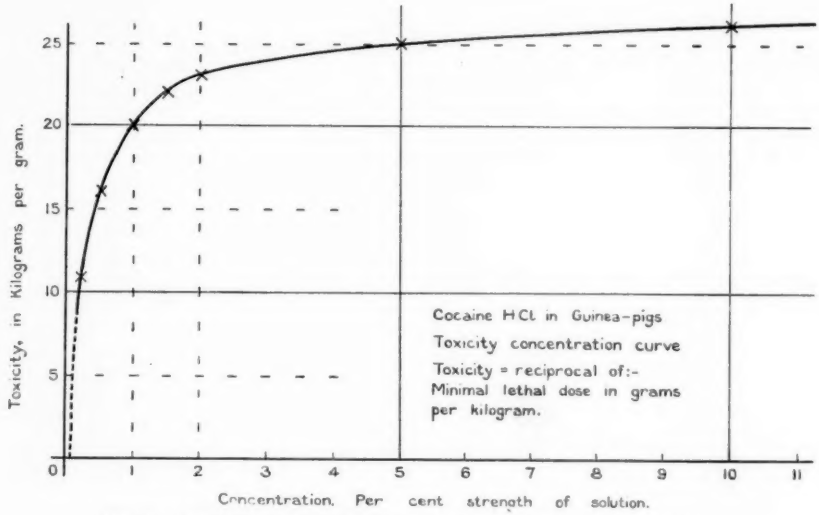
It is not, however, fortunately a necessary conclusion that we shall never obtain a local anæsthetic entirely without the disadvantage of toxicity. It is important when we discuss the toxicity of any drug to state clearly how that toxicity is measured. The pharmacologist estimates the toxicity by means of experiments on animals. Doubtless experiments on apes would be the best method, if it were feasible, of estimating the toxicity for man; but considerations of expense, if nothing more, make this almost impossible, and it seems probable that deductions from experiments on smaller animals, such as dogs or guinea-pigs, may give us valuable guidance. As far as possible the method of administration to the animal should be the same as that used clinically, e.g., subcutaneous. A series of graduated doses of the drug is administered to animals of the species chosen and the smallest dose that will cause death is thus determined. For convenience this may be expressed in terms of grams of drug per kilogram of animal. The reciprocal of the figure thus obtained is a measure of the toxicity of that drug for that species; it indicates, of course, the weight of animal that will be killed by one gram of the drug. Having found this figure for a drug of which we know the toxicity in man, and also for the drug under investigation, we have a measure of the relative toxicity, at the concentration used, for the species chosen, of the two drugs; we may then assume that their *relative* toxicity in man is the same, and so estimate the toxicity of the new drug for man. The assumption may appear a large one, but provided we regard the figure obtained only as a rough guide to the minimum value, the method seems to work satisfactorily. It does, however, seem to me to be very unwise to suggest that the toxicity of a new drug, which in cats is more toxic than cocaine, is for man less toxic, until there is a definite body of experimental or clinical evidence to support this contention.

Concentration of Solution.

I have ventured to lay some emphasis on the question of the *concentration* of the drug in relation to the toxicity. The actual killing power, for example, of a gram of cocaine depends very much on the strength in which it is injected. The results of an extensive series of experiments[1] with cocaine, subcutaneously

[1] The cost of these experiments has been defrayed by the Colston Research Fund of Bristol University.

injected into guinea-pigs, is indicated in the chart below. Horizontal measurements indicate the per cent. concentration of the drug, vertical, its toxicity in kilograms per gram. It is the shape of the curve which is of interest. In concentrations above 2 per cent., and especially above 5 per cent., the toxicity is not very materially affected by variation in the concentration—the curve is nearly horizontal. But in larger dilutions the toxicity of the same gram of cocaine falls rapidly; at ¼ per cent. it is only half as toxic as at 2 per cent. This might not appear very important, but I would remind you that most of the experimental work has been done with concentrated solutions, while clinically only the dilute solutions are injected. If the curves for all the drugs were of the same shape the validity of the comparisons at high concentrations applied to low concentrations would not be affected. I have, however, definite evidence that this is not the case. The curve for butyn, which I have not finished, is



Effect of dilution on the toxicity of one gram of cocaine hydrochloride.

much more rectangular than this; that is to say, dilution has a less effect in lowering the toxicity of butyn than in the case of cocaine. The reverse condition is indicated for novocaine.

Non-toxic Concentration.

There is another point in connexion with this curve. It comes down as if it would meet the line of zero toxicity before it reaches zero concentration. In other words, if we sufficiently dilute the drug, a point is reached at which it is entirely non-toxic. For cocaine, this point is probably below the limit of clinical usefulness, but for novocaine it is possible that it lies near ¼ per cent.; that is to say, it is impossible to administer subcutaneously a dose of novocaine in ¼ per cent. solution that will kill a guinea-pig. I believe this is supported by

clinical observation. Perhaps it is in this direction that the solution of the problem of an entirely non-toxic substitute for cocaine will be found—a substance which gives good surface anæsthesia in its non-toxic concentration. Let us wish our synthetic chemists " more power to their elbow."

Danger Ratio.

The clinician does not estimate the toxicity of his anæsthetic by the lethal dose. A knowledge of this is useful to him, but in practice he hopes always to keep his dosage far below this dangerous point. For him the toxic dose is that which exposes his patient to the risk of serious disturbance, such as fainting, palpitations or the like, not to mention convulsions. With each anæsthetic, and varying possibly with each concentration, such a dose may be a different fraction of the lethal dose. This fraction I have called the danger ratio. It appears that cocaine has a low ratio, while stovaine and novocaine have a higher, and alypin and butyn yet a higher. Exact measurements of the smallest dose that produces such effects in experimental animals is manifestly a matter of great difficulty, and I cannot claim a higher degree of accuracy than to state that this is how the experiments impressed me. I suggest that for cocaine the ratio in man is about one-eighth. It is to this low value that the relative safety of cocaine in practice is possibly partly due : those who have been tempted to exceed the safe dose would be warned by the occurrence of cases of fainting, &c. Perhaps some of the newer anæsthetics will be less ready to afford this warning—an additional reason for caution in using any new drug. The bad reputation of cocaine for toxicity and for demonstrating idiosyncrasy almost certainly depends on some such factor rather than on the occurrence of deaths from proper clinical use. The rather wide variation between the relative toxicity of several drugs as estimated by the clinician and by the pharmacologist may be explained on these grounds, and a consideration of the story told by the concentration-toxicity curves.

Possible Substitute for Cocaine.

A due recognition of the value and importance of a really satisfactory substitute for cocaine is evidenced by the ceaseless work that has now for years been carried out by the chemists and pharmacologists in this country, in America, and on the Continent. The search is arduous and intricate. Apart from considerable technical difficulties, although some glimmering of light is beginning to be thrown upon the relations between chemical constitution and pharmacological action, this field of knowledge is so generally obscure that a method of trial and error is necessarily adopted. The surprises and disappointments are many: Dr. Dale mentioned that butyn, the most toxic of the substances we are discussing, is homologous to novocaine, the least toxic. At Bristol University, a homologue of β-eucaine has been synthesized in the hope that it would be found less toxic than β-eucaine; it proved to be more than twice as toxic. It is almost certainly only a matter of time before we are in possession of the perfect local anæsthetic. But I feel that if we are asked whether we have now any substance that can replace cocaine in our pharmacopœia, the answer must be an unqualified negative.

Mr. Frank Coleman

remarked that local anæsthetics were employed in dental surgery in several ways. From the dentists' point of view, the crux of the matter seemed to be in finding an efficient local anæsthetic, apart from cocaine, for operations such

as the removal of pulps of teeth. He knew of no substitute which was so efficient for this purpose when employed in the form of so-called "pressure" anæsthesia, and the operation of removal of the tooth-pulp was a common one in dental practice. The alternatives to this procedure of "pressure" anæsthesia for pulp extirpation were regional anæsthesia (block) and decortical bone anæsthesia (intra-osseous) in the region of the tooth apex. The former, which entailed infiltration of the main branches of the second or third divisions of the fifth nerve, was unduly severe for such a small operation as pulp extirpation, the method entailed a deep injection among important anatomical structures, the anæsthesia took at least twenty minutes to develop, and was less reliable. The second alternative of decortical bone anæsthesia (intra-osseous) was difficult to carry out at the back of the mouth, and was also less reliable than "pressure" anæsthesia with cocaine.

The local ischæmia produced by cocaine was an advantage in most dental operations especially for those that entailed searching for buried roots in the jaw, or portions of a pulp in a root canal.

Most patients used the word cocaine as a generic term for all local anæsthetics, and not infrequently, patients had remarked to him that they had not experienced any elation or pleasurable sensations after the use of my cocaine, which in reality was a cocaine-free substitute and usually novocaine.

He was interested to hear the statement of Dr. Dale, that solutions of cocaine could not be sterilized by boiling without hydrolysation, as he had been under the impression that cocaine solutions could be boiled for a brief period provided there was an absence of alkali in the contained vessel. He believed that Martindale and Westcott stated this view. This seemed to be a serious drawback to the employment of cocaine as a local anæsthetic.

Sir Maurice Craig.

I have been asked to speak on the question of cocaine addiction, but I am at once faced, as I have always been, with the difficulty of finding out the extent of addiction to cocaine in this country. In thirty and more years of practice, hospital and private, including war experience, I have seen less than half a dozen cases, and this is all the more remarkable when the nervous system is supposed so commonly to suffer. I have asked my medical colleagues, and their experience seems to coincide with mine. I have for several years asked medical practitioners from all over the country whom I have met, and usually their answer is that they have never seen a case at all. Therefore from this class of evidence one is driven to the conclusion either that cocaine is a harmless drug (which we know it is not) or that addiction does not exist to any appreciable extent. We know that round about Leicester Square and similar haunts of degenerate and dissolute persons cocaine seems to be sought for as a stimulant, but I should like to obtain information as to what happens to these persons. Cocaine poisoning, when one meets a case, is fairly easy to diagnose. So far as some other countries are concerned, I understand from reading that cocaine addiction is common, but I only hope that the data are more carefully sifted than they appear to be in this country. There is in my opinion too much danger of jumping to a hypothetical conclusion which is taken up by others and quoted as a fact. There is only one way of getting at the truth regarding this matter, and that is for medical men to record their cases, care being taken that the same case is not recorded twice.

Years ago cocaine was freely used in minor surgical practice, and I do not recall any question of the danger of a habit resulting from such employment ever being raised.

From my experience spirit is infinitely more dangerous from the addiction standpoint, and the misuse of it in this way infinitely more dangerous to our national life, and yet the penalties as regards cocaine are colossal (I do not say that they are wrong), while any attempt to control spirit addiction is practically non-existent.

When I was asked to speak on this subject, I accepted not because I had any positive facts to bring forward, though I have negative results of a very suggestive character, but because I felt that in the interests of sound medicine it was urgent that we should seek, as far as it is possible, to discover what is the true position.

Mr. R. Foster Moore.

The question of local anæsthesia for the ophthalmic surgeon is perhaps simpler than for some other branches represented here this afternoon; at the same time a really efficient anæsthetic is of critical importance for him, for the bulk of his most delicate and important operative work, viz., intra-ocular operating, is done under the influence of local anæsthesia.

Three methods of application are employed: (1) By infiltration of the tissues; (2) by application of the solid drug; (3) by instillation into the conjunctival sac. I have mentioned the two least important methods first, so as to dispose of them, and leave the most important for more detailed consideration.

(1) Infiltration Anæsthesia.

This method is employed for plastic operations on the lids and adjoining parts, for curetting meibomian cysts, for removal of the lachrymal sac, for enucleation of the eyeball and acute glaucoma, in some cases for paralysing the orbicularis muscle during cataract operations, and for a few other purposes.

I believe I am expressing the opinion of ophthalmic surgeons when I say that novocaine in 2 per cent. or 4 per cent. solution, is so efficacious and so little toxic, that unless it should be found to be a drug of addiction, it is a complete substitute for cocaine for the above purposes, and that cocaine therefore does not enter into competition with it.

(2) Application of the Solid Drug.

This method has not a wide field of application. Solid cocaine is used for direct application over a meibomian cyst before incising it, over the insertion of the muscles in some muscle operations, and for other small miscellaneous operations on the conjunctiva and lachrymal passages. It is a quick and convenient way of producing complete surface anæsthesia. I have used butyn in the same way, and whilst it produced severe smarting, it gave good anæsthesia. My opinion is, that whilst it is not quite so convenient, it is possible to substitute novocaine infiltration anæsthesia for all these purposes, or, alternatively, to use solid butyn; and that it would not be a serious deprivation if cocaine was not available for this purpose.

(3) Instillation Anæsthesia.

The following are the chief manipulations for which instillation anæsthesia is required: Examination of the exterior parts of the eye where corneal irritation is severe; tonometric observations; syringing the lachrymal passages and small operations on the ducts; removal of foreign bodies from the cornea

or conjunctiva; curetting and cauterizing corneal ulcers, &c.; and by a long way the most important, all intra-ocular operations on uninflamed eyes, except where lack of control on the part of the patient renders general anæsthesia necessary.

For the last mentioned operations it is imperative to have the best possible anæsthesia; they constitute the crux of the whole matter for the ophthalmic surgeon: for if the patient at all relaxes control of himself because some pain is felt, not only are delicate intra-ocular manipulations rendered more difficult, but damage may easily be done which results in irreparable hurt to the eye, and perhaps in its loss. A point of less importance is that some of the drug may obtain access to the interior of the eye during the operation, and its entry may cause post-operative iritis if it is particularly irritating.

Advantages and Disadvantages of Cocaine.

It may be well now to analyse the advantages and disadvantages of cocaine with regard to the following points: (1) Degree of anæsthesia; (2) toxicity; (3) irritation produced; (4) stability; (5) effect on the sympathetic nerve; (6) liability to become a drug of addiction.

Several of these have already been dealt with by those who are competent to speak of them, and in these cases I shall therefore have very little to add.

(1) *Degree of Anæsthesia.*—For superficial manipulations, e.g., removal of foreign bodies from the cornea, there are a number of drugs which give a satisfactory anæsthesia and can be substituted for cocaine without great disadvantage. So far as rapidity of action and duration of anæsthesia are concerned, none of the drugs I have tried have exhibited any defect in either of these respects.

With regard to the deep anæsthesia required for intra-ocular operating, my belief is that cocaine produces a better anæsthesia of the deeper structures than any other drug, and this point has greater weight than any other consideration.

(2) *Toxicity.*—Apart from the occasional use of the solid drug which has been referred to, the strongest solution of cocaine that is at all commonly used in eye work is 4 per cent., and whilst some gets into the lachrymal passages, and is therefore swallowed, the total amount which enters the system by this means or by absorption from the conjunctiva, is very small. On a few occasions I have seen mild toxic symptoms, but never any that were at all serious: and it is not easy to be sure to what extent such symptoms were psychical in origin. I do not think the toxicity of cocaine as used by instillation for ophthalmic work is of such degree as to militate against its credentials.

(3) *Irritation produced.*—Smarting is produced when any local anæsthetic is dropped into the conjunctival sac; in some cases it is quite severe, and with some, considerable injection is produced; in each of these respects, the effect produced by cocaine is mild.

Cocaine has a notoriously deleterious effect on the corneal epithelium, causing it to become œdematous; it is a disadvantage of the drug, and is, I think, more marked than with any of the substitutes.

(4) *Stability.*—As has been pointed out by Dr. Dale, cocaine solutions cannot be sterilized by boiling; when kept for some time they tend to develop a fungus growth, both of which facts must be considered as points against the drug. Freshly made solutions, however, are most unlikely to contain any

virulent organism, and I have never had a case of suppuration which I believed to be due to this cause. I have used cocaine solution in ampoules which I had kept for two years, and I did not notice any deterioration of its anæsthetic properties. I do not think that either of the above two disadvantages should be considered to have great weight.

(5) There is another action of cocaine, apart from its anæsthetic properties, and that is its action on the sympathetic nerve, producing mydriasis, by stimulating the dilator pupillæ fibres, and widening of the palpebral fissure by stimulating unstriped muscle tissue in the lids. This action is valuable in two ways: it forms an important test for paralysis of the sympathetic nerve, and its mydriatic action is valuable in those cases in which dilatation of the pupil for fundus examination is necessary, but in which less easily controlled mydriatics are contra-indicated on account of the danger of glaucoma. The only other drug which I have found to possess this action is stovaine. It has been suggested to me that this might prove to be a test as to the power of a drug to stimulate the central nervous system, and, consequently, as to its liability to become a drug of addiction, a test of some value should it be substantiated; neither butyn, holocaine, nor novocaine have this action.

(6) *Addiction.*—All that it is appropriate for me to say is, that I know of no case in my own experience, nor have I been able to hear of any from my ophthalmological colleagues, in which cocaine addiction has arisen from its use for surgical purposes, nor from the prescribing of it. I believe most ophthalmologists would agree in thinking cocaine of little therapeutic value apart from its anæsthetic properties, and that there are few cases in which it is of benefit when prescribed for use by the patient: I believe, too, that the restrictions which have been placed on the prescribing of this drug have resulted in less of it being prescribed for use by the patient.

Cocaine Substitutes.

The following are the substitutes which I have tried: (*a*) Novocaine; (*b*) butyn; (*c*) stovaine; (*d*) holocaine.

(*a*) *Novocaine.*—I need add nothing to what I have said about novocaine except that I have not tried it for instillation anæsthesia. Any of the remaining three will produce quite satisfactory superficial anæsthesia.

(*b*) *Butyn.*—I have tested butyn against cocaine in twenty-eight patients, by dropping 2 per cent. solution of the one into one eye and of the other into the other eye. Smarting was rather greater and was developed at once with butyn, with cocaine it was a little delayed, the difference was not great; butyn caused more injection of the conjunctival vessels. Its effects lasted longer than those of cocaine. In no case did I observe signs of stimulation of the sympathetic nerve or œdema of the corneal epithelium. I have used it only a few times for intra-ocular operating; there was more bleeding than with cocaine, and whilst anæsthesia was good I thought it was not so good as with cocaine. I have used the solid drug in my own eyes and it certainly causes smarting which I should call severe; the conjunctiva and cornea were rendered quite insensitive.

(*c*) *Stovaine.*—I have used 2 per cent. and 4 per cent. solutions. In twenty-two cases I have tested it against butyn, by using 2 per cent. solution of each, putting one drug into one eye and the other into the other eye. Stovaine produces distinctly more smarting and conjunctival injection than butyn. I have used both strengths of solution for intra-ocular operating, cataracts and needling; the anæsthesia was not satisfactory, and for this reason alone if for

no other, I should consider stovaine an unsuitable substitute. As already stated it causes dilatation of the pupil and widening of the palpebral fissure, but to a less extent, I think, than cocaine.

(*d*) *Holocaine.*—I have used this drug in 2 per cent. solution on myself, and on a large number of cases for superficial anæsthesia. It produces more smarting than cocaine or butyn and less than stovaine. Superficial anæsthesia is satisfactory, it does not dilate the pupil and has little effect on the corneal epithelium. I have not given it a proper trial for intra-ocular operating, but in the three cases in which I used it I did not think the anæsthesia very good.

If now one tries to form a judgment as to the respective qualities of these drugs, it seems to me that novocaine is almost ideal for infiltration anæsthesia. For instillation purposes stovaine is unsatisfactory and so far as my examination has gone holocaine is not very good in this respect; the drug that seems to come most closely into competition with cocaine is butyn. Butyn has advantages in that it is more stable, and can be sterilized by boiling; it affects the corneal epithelium less, and its effects last rather longer. On the other hand it causes a little more smarting and conjunctival injection, and it does not stimulate the sympathetic nerve. Deep anæsthesia (the one really essential point), though good, does not seem to me to be so good as with cocaine; this is a matter, however, on which I prefer to suspend judgment. Further use of the drug will enable us to arrive at a more positive opinion. Like Dr. Dale and Mr. Layton I desire to make it clear that I have only expressed my own views based on my own experience, and that I have made no use of information which has come under my official notice as a member of the Ministry of Health Committee.

Dr. DALE (in reply)

said that, as far as he was aware, procaine and novocaine were identical, the former being only an American name for the latter. In the *Journal of Pharmacology*, which was a joint Anglo-American publication, there recently appeared an account of experiments made to see whether the substances so named had the same local anæsthetic action, and the conclusion reached was that they had.

The toxicity curve (p. 80) exhibited by Mr. Eric Watson-Williams, in support of his contribution, was a very interesting one; it brought out a very important point. He (the speaker) took it to mean, as Mr. Watson-Williams said, that the local anæsthetics had different degrees of instability. It was shown, some time ago, that novocaine, in whatever strength it was injected, seemed, when used hypodermically, to be very much less toxic than cocaine, but that if the injection was carried out directly into the blood stream, the contrast was much less striking. Novocaine was either very rapidly eliminated through the kidneys, or more probably, was very quickly hydrolysed into inert compounds as it got into the blood-stream. Therefore if it did not get in quickly, it did not produce its full toxic effect. That butyn showed a more sharply right-angled curve in Mr. Watson-Williams' diagram was an indication that it was a more stable substance than some of the others.

The desirability of a better local anæsthetic than any yet available had been mentioned. He had private information of a new local anæsthetic which had recently been produced in Germany, and he hoped to receive a sample of it in a few days. He would then hand some to Professor Dixon and Dr. Copeland and ask them to be kind enough to conduct some preliminary experiments with it. The preliminary report on that substance was to the

effect that it was seven or eight times as potent as novocaine, and only twice as toxic.

He agreed with Professor Gibson and Professor Dixon that there was no reason why something better than had hitherto been evolved should not emerge from the work being done. If he had suggested that cocaine would not, under any condition, stand being boiled, that was an exaggeration. He thought the main danger was that cocaine, like all esters of that type, was extraordinarily unstable if the solution was even slightly on the alkaline side. For instance, boiling in ordinary tap water sufficed to destroy a material amount. But he thought a method could probably be devised by which cocaine could be sufficiently sterilized without losing more than a trivial amount of its activity.

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A Critical Report on One Hundred Cases of Spinal Analgesia with Tropacocaine.

By Henry Featherstone, M.B., B.C.

In this paper I propose to emphasize certain points in the selection of cases, administration of the drug, condition of the patient, and local effect at operation, together with the ultimate progress, as noted in my last hundred administrations.

Selection of Cases.

In common with many other anæsthetists, I have never been wholly convinced that spinal analgesia, even for those operations for which it is permissible, is as satisfactory as general anæsthesia. Mental distress and lack of co-operation on the part of the patient, failure to insert the needle (particularly in the extremely obese and the deformed), deterioration of the drug, limitation of the time available for operation and the added strain on all workers in the theatre, are of course, only a few of the disadvantages of the method. For these and other reasons, presently to be discussed, the spinal method was used only when definitely indicated, and not on every possible occasion.

Many patients suffered from several conditions, each of which would have led one to select an intrathecal anæsthetic. The following list, however, includes only the principal indication for each administration:—

Operations upon moderately good subjects, involving severe trauma	26
The presence of acute shock from trauma and loss of blood	2
The presence of acute toxæmia and shock from peritonitis or intestinal obstruction	6
The presence of acute or chronic infection of the lungs	15
,, acute infection of the upper air passages	6
,, marked pre-operative exposure to infectious colds	3
,, obstruction of the larynx from enlarged thyroid	2
,, tuberculosis	5
,, heart disease, chronic myocardial disease	3
,, ,, severe mitral disease	1
,, ,, auricular fibrillation	2
,, thrombosis of veins in thigh and pelvis	1
,, albuminuria	2
,, diabetes	2
,, eclampsia or pernicious vomiting of pregnancy	2
,, old age	3
,, feeble general condition	2
For Cæsarean section	12
To prevent uterine hæmorrhage during evacuation of uterus	1
For patients who disliked general anæsthetic	7

Of two patients suffering from crushed lower limbs, one died immediately after the necessary amputation, the other recovered. It was, of course, shown conclusively during the war, that the spinal method, with its accompanying fall of blood-pressure, may be positively dangerous to men suffering from acute traumatic shock and from loss of blood. My fatal case was admitted to the hospital in great agony from a tourniquet, and an immediate spinal injection relieved his suffering, but, I am inclined to think, hastened his death. In view of these considerations, the procedure was rarely employed for this type of case. In two cases, pneumonia developed after operation. One was a case of ruptured ectopic gestation; the patient's upper abdomen contained a quantity of blood, and she presented signs of capillary bronchitis before operation. The other was a case of Cæsarean hysterectomy for toxic intra-uterine hæmorrhage, bloodless before operation and very shocked afterwards. Both patients recovered, but as these were the only two cases in which pneumonia developed, they demonstrate the importance of loss of blood and abdominal shock as factors in the causation of post-operative pneumonia.

Twenty-four patients, who showed pre-operative infection of the lungs and upper air-passages, were not appreciably worse after the operation, at least so far as the lungs were concerned.

Six patients, in a grave state from peritonitis or intestinal obstruction, resulting from a leaking gastric ulcer, suppurating Fallopian tubes, or strangulated bowel were not harmed by the injection, but shock was not always eliminated, even with relatively high analgesia, particularly in upper abdominal cases.

Five tuberculous patients made satisfactory progress after operation under spinal analgesia.

Excellent results were obtained in six patients who exhibited severe heart disease. The fall in blood-pressure was not more marked than in normal cases, and not one complained of faintness. For example, in a severe case of mitral disease, the patient, who had had fibrillation of the auricles, was given spinal analgesia for forceps delivery at full term. The systolic pressure dropped only from 140 mm. to 125 mm., while the diastolic pressure and the pulse-rate remained stationary at 70 mm. and 88 per minute respectively.

Thrombosis of the thigh was well treated by this method, subtotal hysterectomy for enormous bleeding fibroids being performed without much fear of movement immediately after the operation, leading to embolism. In this case, as the operation proceeded, it was convenient to obtain the patient's consent to a more radical operation than the simple myomectomy originally planned.

Individuals affected with albuminuria, diabetes, eclampsia, and hyperemesis gravidarum, made such good progress that evidence was afforded of the value of a non-toxic method (that is to say, gas and oxygen, spinal or local analgesia) in diseases of metabolism, or in severe toxæmias. The patient, who exhibited pernicious vomiting of pregnancy, stated that evacuation of the uterus for the same complaint, on two previous occasions, under general anæsthesia, had been followed by even more severe vomiting. On the other hand, evacuation under spinal analgesia led to immediate and lasting cessation of vomiting. I do not know of any reports on the effect on the kidney function of stovaine or tropacocaine during excretion.

Two old men, one 81 and the other 77 years of age, had prostatectomy performed and both wrote two months later declaring that they were walking, and were free from headache and giddiness.

During the period covered by this series, seven patients expressed dislike of a general anæsthetic, and a spinal anæsthetic was administered, preceded by omnopon or morphia. The area of operation in each case was suitable, and with the patient's help and co-operation good results ensued.

In certain operations which involve considerable surgical trauma, and in Cæsarean section, I hope to show that the spinal technique may be of great help.

ADMINISTRATION.

As regards administration a few points may be mentioned. During lumbar puncture I prefer to lay the patient on his side. He is more at his ease, and, if necessary, a general anæsthetic may be begun at once. The sitting posture is, of course, contra-indicated in many conditions.

In this series, I employed a 5 per cent. solution of tropacocaine in normal saline, prepared by Messrs. Allen and Hanbury. I found that the dose should vary, to a certain extent, with the dimensions of the vertebral canal, the level of analgesia desired, and the duration of the operation, 2·5 c.c. of the solution was not too much for an hour's operation on a big man, but 1·5 c.c. in a boy, 8 years old, yielded a high level of analgesia lasting an hour and a half.

I think ethyl chloride spray dulls skin sensibility sufficiently for incision. Gas often induces stiffening of the erector spinæ, thereby increasing the difficulty of finding the intervertebral space.

Attention has wisely been drawn to the mistaken assumption that cerebro-spinal fluid appearing at the butt of the needle ensures the contents of the syringe reaching the intrathecal space. Of course, the eye of the needle may be only partly through the membrane and some of the drug may be lost in the epidural space. I find that an infallible sign may be obtained by connecting the charged syringe to the needle when in position and withdrawing the piston a very little way. If fluid readily enters the syringe, the injection may be made. The cerebro-spinal fluid, on entering the syringe always diffuses rapidly upwards, thus demonstrating the heavier specific gravity of the tropacocaine solution. The tropacocaine solution has a specific gravity of 1016, while that of cerebro-spinal fluid is 1006.

I generally made the injection through the second lumbar space. However, examination of the cord, both with the naked eye and microscopically, in two autopsies of patients who had received the injection through the seventh dorsal space, showed no change.

Extension of the analgesic area upwards depends upon: (1) drawing off a considerable quantity of cerebro-spinal fluid (2) inclining the body with the head downwards, when using a relatively heavy solution; (3) rapid injection of the solution. There are objections to the first two methods. Rood and others have pointed out that considerable loss of cerebro-spinal fluid is the most certain cause of so-called "spinal headache." In eighty-nine of my cases, a minimum quantity was released, three suffered from headache during the next two or three days, i.e., 3·5 per cent. In nine cases, however, a free flow of more than 5 c.c., was followed by headache in seven, i.e., 78 per cent. Not one ndividual, after combined spinal and general anæsthetic, complained of this post-operative headache.

A young woman, suffering from nasal catarrh, underwent cervical dilatation for dysmenorrhœa. The drug was mixed intimately with the cerebrospinal fluid by withdrawing and reinjecting several times. High but imperfect analgesia was the result. Two hours afterwards, vomiting, acetone in the breath and urine, and a state

of shock supervened. This lasted for two days. It was the only case of toxic symptoms after the injection of tropacocaine, and I suggest that diffusion and dilution of the drug led to more rapid absorption into the blood stream and hepatic shock. This patient wrote six months afterwards, to say that she was "better in every way." I have never repeated this free mixing of the drug and cerebro-spinal fluid.

Sudden inclination of the body, in common with any other violent alteration in posture, led to a more rapid fall of blood-pressure than was noted with more gentle movement of the patient. In view of these considerations, I injected the tropacocaine solution fairly rapidly, laid the patient horizontally on his back, with the head on a pillow, and the legs were raised. In this way, analgesia was found to extend to an inch or two above the umbilicus. By gradual inclination of the table, head downwards, sensation could be abolished as high as the sixth dorsal vertebra, without discomfort to the patient.

The following operations were performed in this series:—

Nature of operation	Total	Recovered	Died
Rammstedt's for hypertrophic pyloric stenosis	1	*Nil*	1
For perforated gastric ulcer	1	,,	1
Cholecystectomy	1	,,	1
Resection of small bowel	1	,,	1
For relief of intestinal obstruction	1	,,	1
Appendicectomy	1	1	*Nil*
Herniotomy	2	2	,,
Cystoscopy	4	4	,,
Prostatectomy	2	2	,,
Wertheim's hysterectomy	6	6	,,
Abdominal hysterectomy	4	4	,,
Vaginal hysterectomy	2	2	,,
Ventral fixation	1	1	,,
Ovariotomy for large cyst	1	1	,,
Salpingo-oöphorectomy	4	4	,,
Dilatation and currettage	3	3	,,
Evacuation of uterus by hysterotomy	5	5	,,
Cæsarean section and one Cæsarean hysterotomy	15	15	,,
Parturition	2	2	,,
Operation on pregnant woman not directly connected with pregnancy (vaginal cyst)	1	1	,,
Plastic operations such as perineorrhaphy	17	17	,,
Excision of vulva and glands	4	4	,,
Amputation of penis with glands	2	2	,,
Kraske's resection of rectum	5	5	,,
Combined abdomino-perineal resection	2	1	1
Hæmorrhoids	6	6	*Nil*
Amputation at hip-joint (sarcoma)	1	1	,,
Amputation through thigh	1	1	,,
Excision of knee	1	1	,,
Amputation through leg	2	1	1
For varicose veins	1	1	*Nil*

There was no death on the operating-table, and only one immediately after operation (an amputation through the leg to which reference has already been made). Attention is drawn to the high mortality among the upper abdominal cases.

If the drug lost its action before the end of the operation nitrous oxide, oxygen, and perhaps a little ether was employed for so long as was necessary. I never give a second injection. Patients dislike the full Trendelenburg position, so that for Wertheim's hysterectomy, gas and oxygen was always given in addition.

Site of Operation.

I believe there is strong evidence that operations on the abdomen in which any organ above the colon is involved, are better performed with a general anæsthetic. While it is true that skin sensibility may be abolished up to the

clavicles without serious danger, it has been agreed that the higher the analgesia, the greater the fall in blood-pressure, and few surgeons or anæsthetists aim at analgesia above the costal margin. Both the patient with perforated gastric ulcer and the patient in the case of cholecystectomy, complained of pain in the right shoulder, although the drug was injected through the seventh dorsal space, and high analgesia obtained.

In each case of salpingo-oöphorectomy, or hysterectomy, in which adhesions to the small bowel were divided (five cases in all) the patient was much distressed, although unable to localize the pain. Six patients undergoing similar operations in the absence of adhesions, suffered no discomfort.

The method is seldom employed in children's hospitals for Rammstedt's operation on the pylorus, because the mortality is higher than with the gas and oxygen method.

Appendicectomy under spinal analgesia was performed on a boy whose lungs gave signs of early pneumonia, but analgesia to the costal margin did not prevent considerable shock and vomiting immediately the bowel was handled during the search for the appendix. This lad was quite placid until the intestine was pulled.

A somewhat feeble woman underwent herniotomy for incarceration of omentum and small bowel in a left inguinal hernia. No discomfort or mental distress was experienced until the sac and its contents were pulled by the surgeon. She then cried out, and, when questioned as to the site of the pain, she exclaimed, "you are crushing my chest." Of course no one was touching her chest. The sac contents were reduced, and after that, notwithstanding powerful manipulation of the sac, the patient became quite comfortable and talked cheerfully.

I suggest that, unless the drug has reached the upper dorsal region, afferent sympathetic impulses from the mesentery, stomach, and small bowel will reach the brain. Stimulation of the diaphragm, directly or indirectly, causes distress unless the cervical cord itself be anæsthetized.

Local operations, such as amputation of the penis, excision of the vulva, perineorrhaphy, operation for hæmorrhoids, and varicose veins, were performed in great comfort, both for the operator and the patient, with a minimum loss of blood.

For such severe procedures as Wertheim's hysterectomy, Kraske's resection of the rectum, and amputation at hip-joint, excellent results were obtained by the combination of spinal analgesia and light narcosis with morphia, gas and oxygen. These patients, fourteen in all, were surprisingly fit after the operation, and thirteen are still alive. In a case of rectal carcinoma the patient died from embolism two months after the operation.

The action of lumbar analgesia on the pregnant uterus is of great interest, and Mr. Beckwith Whitehouse and I have laid stress on this phenomenon in two recent papers. Briefly, we have noticed that paralysis of the lumbar and sacral regions of the cord promotes immediate tonic contraction of the body and cervix, the circular muscle-fibres, as we believe, alone contracting. This will explain the following observations:—

In fifteen Cæsarean sections the uterus contracted tonically and at once, before opening the abdomen. The uterus bled very little from its cut surfaces. The placenta, where removal *per vaginam* was necessary, was delivered with difficulty through the tight os, although dilatation had been complete in these cases before operation had commenced.

In two cases of normal labour, the injection was given after full dilatation of the os. The patients bore down willingly and without discomfort; but the fœtus did not make much progress in either instance, until, as the drug was losing effect, contraction of the longitudinal fibres occurred, sensation, as usual, reappearing last. At this stage

in each case, with the attendant's hand on the abdomen, the child was expressed painlessly, and it cried at once, showing no distress from its long compression.

The severe heart case, already mentioned, was particularly remarkable. Professor Russell and Dr. K. D. Wilkinson, our heart specialists, gave the most gloomy prognosis, stating the risks of Cæsarean section were enormous, and that with such marked heart failure it was very doubtful if the patient would survive labour. Labour set in, and when the os was fully dilated the injection was made. The stoppage of pain and the absence of voluntary expulsive effort afforded immediate relief. Forceps readily extracted the full-term child, without injury to the perineum. Only a teaspoonful of blood was lost, and the placenta was expressed at once without further loss of blood. The uterus had steadily and progressively contracted. Rapid improvement in the condition of the heart ensued.

Evacuation of the uterus by vaginal hysterotomy at the third, fourth, and fifth month was an almost bloodless procedure.

Spinal analgesia administered to a woman, three months pregnant, for a large vaginal cyst, did not disturb the course of pregnancy. Normal labour followed at term.

Effect on the Blood-pressure.

The blood-pressure changes always were considerable. A fall in the systolic pressure of less than 15 mm. was not recorded, while in one case in which the patient had been tilted too suddenly, the systolic reading dropped from 120 mm. to 50 mm. The cause of this fall in pressure is by no means clear. There is vaso-constriction rather than vaso-dilatation, for hæmorrhage from the wound is scanty, and the surface of the analgesic area is relatively cold and pale. Thus dilatation of the arterioles will not explain the fall. The Trendelenburg position, as soon as the drug is fixed, will relieve faintness; but even if, in addition, the legs be bandaged and the abdomen compressed, the blood-pressure does not return to normal. This suggests that engorgement of the splanchnic veins and other large vessels is not the sole cause of the fall. On the other hand, in comparatively slight operations, a normal blood-pressure is observed as soon as the drug has lost power. We see, then, that engorgement of the arteries or of the veins will not explain this fall, Dale, in his Oliver-Sharpey lectures, will not agree to a central nervous control of capillary contractility. I am anxious to go further into the matter at a later date, but I should like to suggest, in a most tentative manner, that the fall may be due to engorgement of the capillaries and small vessels in the fully relaxed muscles of the abdomen and the lower limbs.

Just as the contraction of the peripheral arteries will drive blood into the brain, so, as I surmise, tonic contraction of the normal structures above the analgesic area may force blood into inert muscles, whence return of blood to the heart largely depends on muscular compression of the vessels.

After Operation I prefer to lay the patient in a horizontal bed with the head on pillows and the legs well raised. In this way comfort and safety is assured.

Immediate Untoward After-effects were extremely rare in this series, vomiting and headache resulting, as I have said, from the removal of too much cerebro-spinal fluid. Meteorism, flatulence and incontinence, or retention of urine or fæces were never attributable to the spinal anæsthetic; in fact, as I think Mr. Apperly has said, spinal analgesia may promote contraction in cases of intestinal stasis.

Remote After-effects.—I sent a simple questionnaire to each of the patients

in this series. Not one complained of serious disability which could be laid to the charge of the spinal method. Headache and giddiness were the most frequent sources of complaint. Of seventy replies, some degree of late headache was said to be present in twenty-eight cases, that is to say 40 per cent. One man complained; the remainder were women. Astonished by the high incidence of the trouble, I wrote to twenty patients who, under general anæsthesia, had undergone operations of a similar nature to those suffering from late "spinal" headache. There were sixteen replies, and in eleven of these cases the patients had headache. Thus of those who under general anæsthæsia had undergone operation for conditions likely to cause headache, 68 per cent. did in fact develop late headache. I think I am justified in blaming the patient's complaint rather than the spinal anæsthetic for this late headache. Giddiness did not occur in the case of those patients who were otherwise robust.

Conclusions.

The following are the conclusions at which I have arrived after careful consideration of this series of administrations and of very numerous others, which I have either carried out or witnessed.

(1) Patients suffering from diseases of the lungs, heart and kidneys, from chronic toxæmia, and from diseases of metabolism, may well be treated by the intrathecal method.

(2) The presence of acute traumatic shock is a contra-indication.

(3) The method does not prevent abdominal shock associated with severe handling of a viscus above the colon, without very high paralysis and its attendant risks.

(4) Shock from surgical trauma to the pelvic organs, including the pregnant uterus, is rarely seen.

(5) Association with light general narcosis is most helpful.

(6) Early post-operative headache rarely occurs with tropacocaine, provided that there be little loss of cerebro-spinal fluid.

(7) Most careful handling of these cases is desirable, if a considerable fall in blood-pressure is to be avoided.

(8) Late complications attributable to the spinal anæsthetic are extremely uncommon, in the absence of infection.

In conclusion, I have to tender my sincere thanks for their very great help to the surgeons, obstetric officers, resident staff and sisters of the Birmingham General Hospital, and to Mr. Beckwith Whitehouse in particular.

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Discussion.

Mr. Beckwith Whitehouse said he wished to limit his remarks to the application of spinal analgesia to gynæcology and obstetrics. He had had considerable experience of the method described by Dr. Featherstone and he had been much impressed with certain advantages attaching to its use in pelvic surgery. The area involved was below the danger zone of the colon and in addition there were certain special assets in the use of the spinal method to which Dr. Featherstone had drawn

attention in the course of his paper. Personally he felt greatly indebted to the anæsthetists for the improvement of technique rendered possible by spinal tropacocainization. The indications in his own practice were quite definite, as also were some of the contra-indications. Briefly he would say that the indications might be classified as follows: (*a*) All operations upon the pregnant uterus at any period of gestation, including of course classical Cæsarean section; (*b*) Wertheim's hysterectomy for carcinoma of the cervix uteri, and other pelvic operations involving considerable dissection, and traumatism; (*c*) excision of the vulva and inguinal glands for epithelioma, and leucoplakic vulvitis; (*d*) labour complicated by mitral disease when pregnancy has advanced to full term.

With regard to operations upon the pregnant uterus, spinal analgesia with tropacocaine had proved most useful in promoting uterine contraction and diminishing hæmorrhage. This was not only the case with the uterus at full-term but also during the earlier months of pregnancy.

Most gynæcologists were familiar with the severe and sometimes dangerous bleeding which followed evacuation of the uterine contents from the third month of pregnancy onwards. This risk was not present in those cases in which tropacocaine had been used in preference to a general narcotic. In Cæsarean section the surgical technique was facilitated by the diminished hæmorrhage and strong uterine contraction which aided accurate coaptation of the surfaces of the uterine incision. In pregnancy complicated by severe cardiac lesions the spinal method afforded an easy and comparatively safe method of delivery either by Cæsarean section or by the application of forceps at the end of the first stage of labour.

The contra-indications had been stated by Dr. Featherstone in his paper. One of them was the difficulty in technique introduced by a patient's obesity. To a certain extent it appeared that this difficulty could be removed by practice, and he (Mr. Whitehouse) could not recall an instance in which Dr. Featherstone had failed to reach the theca in any of the cases in which he had asked his co-operation. Another contra-indication mentioned had been the strain imposed upon the theatre staff. If by this Dr. Featherstone meant that there was less talking and general disturbance he thought that the spinal method made for efficiency rather than otherwise! The point after all was not of great importance as frequently gas and oxygen was administered in addition to tropacocaine.

There were however certain definite local contra-indications on which he desired to lay emphasis. One of these was the presence of intestinal adhesions. In operations for pelvic inflammation, e.g., pyosalpinx, the spinal method introduced unnecessary difficulties from the fact that analgesia was deficient, the full Trendelenburg position was impossible, and movements of the intestines much interfered with the surgeon's work. Again, in normal labour spinal methods were, in his (the speaker's) opinion, inadmissable, since the expulsive power of the uterus was inhibited, and the necessity for forceps delivery with all its attendant risks increased. Similarly in abnormal labour, operations such as internal version could not be carried out safely under lumbar cocainization owing to the contraction of the circular muscle fibres which occurred.

In conclusion, Mr. Beckwith-Whitehouse said he wished to emphasize the importance of the closest co-operation between the anæsthetist and the surgeon. He was a great advocate of "team" work, and in order to obtain the best results in a method such as this he thought it most important for the surgeon and anæsthetist constantly to work together. Personally, he had never given a spinal anæsthetic himself and he never intended to do so. It was essentially the rôle of the anæsthetist, the technique necessarily involving the closest attention to detail and the constant care of the patient throughout the operation.

Dr. C. F. HADFIELD said that he had always been so satisfied with stovaine that he had confined himself to its use. He also spoke of the great advantages of combining spinal analgesia with some form of "twilight sleep" which he regarded as, on the whole, preferable to a combined inhalation anæsthesia. He also expressed surprise that so many anæsthetists still used the so-called "heavy" solutions in preference to the "light" solutions which allowed and even rendered desirable the immediate

adoption of the Trendelenburg position. With regard to subsequent headache he said that after prolonged trials he had not been able to establish any relationship between its incidence and the amount of cerebro-spinal fluid withdrawn. He also gave instances of the large amounts of fluid often injected in the treatment of tetanus without causing this complication. In his experience headache was noticeably more frequent after relatively minor operations than after serious and prolonged ones. Dr. Hadfield expressed great interest in the recent work of Mr. Beckwith Whitehouse and Dr. Featherstone on the innervation of the uterus, as demonstrated by the contraction of the circular fibres, when lumbar impulses were cut out by spinal analgesia. He regretted that he could not, from his own experience of Cæsarean section under stovaine, quite concur with their results, as in all cases surgeons had answered his inquiries at the time of operation by stating that the condition of the uterine muscle was not noticeably different from that under inhalation anæsthesia. Reference was made to the somewhat variable duration of spinal analgesia and members were asked to state their experiences in the matter.

Dr. CECIL HUGHES said he believed that a combination of a light general anæsthesia with the spinal analgesia was most necessary. He found vomiting and headache quite infrequent when this was used. Psychical shock and severe fall of blood-pressure were avoided. The position of the patient after the operation was very important: in cases of prostatectomy, for instance, which had been performed in the Trendelenburg position the patients were only very gradually lowered, and were taken back to bed on an inclined plane, the bed also being propped up at the foot, and remaining in this position for three or four hours. A small dose of the "light solution" of stovaine was used, and with the adoption of this routine there had only been one bad headache in just upon five hundred cases. With a light C.E. and spinal combination, the most perfect form of anæsthesia with quiet respiration could be obtained. He (Dr. Hughes) believed that the "heavy solution" made with glucose, was largely responsible for the occurrence of headaches. The duration of spinal anæsthesia, when combined with light general anæsthesia, was more prolonged; and the application of stimuli and manipulations when the effect of the spinal anæsthesia had passed off produced no unpleasant reflexes.

He was not satisfied that spinal anæsthesia was to be recommended for diabetic patients; and he mentioned two such cases in which coma and death had followed operation under this method of local anæsthesia.

Dr. W. J. MCCARDIE said that one of the most serious drawbacks to the adoption of spinal analgesia was vomiting during laparotomies. He had seen this happen twice abroad and once it had happened in his own experience. It had been recorded as occurring as soon as the small intestine was pulled upon and this he had known to happen. Vomiting during laparotomy was said to occur in 9 per cent. of cases and often in the middle of operation, i.e., when the blood-pressure was at its lowest point. Thus spinal anæsthesia did not always abolish shock, especially in deep abdominal manipulations. He (Dr. McCardie) would like to have heard mention of some of the contra-indications of the method. He believed that these were chiefly myocardial disease and cases in which the blood-pressure was low, as from shock, hæmorrhage or disease.

Some twenty years ago Doléris described the striking uterine contractions caused by spinal analgesia with cocaine. The uterus remained firm and contracted after delivery and during suturing in the Cæsarean operation. He (Dr. McCardie) agreed with a view that had been expressed, that the cause of the fall of blood-pressure was more or less due to large dosage, particularly in the case of stovaine. In his experience tropacocaine was less toxic than stovaine. He believed that spinal analgesia should not be used for short operations. It was difficult to compare the statistics of after effects and deaths with those of general anæsthesia because the numerous short narcoses under inhalation anæsthesia would invalidate them. As had been said "big operations and bad patients worsen the figures of spinal analgesia."

Mr. KIRKBY THOMAS stated that his experience of spinal analgesia was limited to the use of tropacocaine, and as he had found that, if the technique was correct, this drug fulfilled all surgical requirements, he did not feel justified in experimenting with others.

He thought, however, that the drug as now supplied seemed less potent than the pre-war article (manufactured at Darmstadt), and he had found it necessary to employ a rather larger dose than formerly to produce the same effect.

He agreed with Dr. Featherstone that headache was less frequent and, if present, less severe when a minimum amount of cerebro-spinal fluid had been withdrawn. With regard to the pain said to be caused by the insertion of the needle, Mr. Thomas said that after slightly chilling the skin-area with the ethyl-chloride spray, he invariably punctured the skin with a sharp-pointed tenotome and inserted the needle through the small incision thus made. This method certainly reduced the patient's discomfort, and in most cases no complaint was made of any pain at all.

Dr. FEATHERSTONE, in reply to a question asked, said that the age in the case of hypertrophic pyloric stenosis was under six months. He found that patients, with safety, could be inclined head downwards six or seven minutes after the injection, but analgesia extended upwards, in some cases, if inclination were delayed until a quarter of an hour had elapsed. He had not seen a case of complete respiratory failure.

Tropacocaine was not used for several reasons. It was a weaker drug than stovaine, and he (the speaker) was in accord with Professor Morrison and Dr. McCardie in their view that tropacocaine was much less toxic. The larger amount of drug ensured a more efficient action on the spinal cord. Headache appeared to be less frequent after tropacocaine. It was convenient to employ the same drug throughout this series. With regard to the fall in blood-pressure, he agreed that larger doses appeared to cause rather more disturbance. The most marked fall in systolic blood-pressure occurred in cases of high blood-pressure. Possibly in a high case of analgesia to which reference had been made, the blood-pressure was maintained by the unusually rapid heart beat.

He also found that the blood-pressure did not fall on raising from the Trendelenburg position to the horizontal, provided that the legs were well raised.

He quite agreed with Dr. Hadfield that, as a rule, vomiting was the result of a considerable drop in blood-pressure. It was remarkable that Dr. Hadfield had not observed uterine contraction when using stovaine, but there were very numerous witnesses to confirm the observations on the action of tropococaine made by Dr. Whitehouse and himself. Obesity led to exaggeration of the spinal curves, and the lumbar spines therefore receded from the surface. On the other hand, the more pronounced dorsal curve enabled intrathecal puncture at the tenth or eleventh dorsal space to be performed with reasonable ease. Neurologists had pointed out that diagnostic lumbar puncture was followed by headache, and in these cases air entry could not occur, consequently intrathecal air was not, as the French suggested, a frequent cause of headache.

Section of Anæsthetics.

President — Dr. A. L. Flemming.

A Surgeon's Thoughts on Anæsthetics.

By Leonard Gamgee, F.R.C.S.

In the first instance let me express my gratitude for being allowed to address the members of this Section. I hope that some opinions expressed by one who is, as regards the giving of anæsthetics, at the same time a co-operator and a critic, may be of some interest. I do not propose to raise the old vexed question of chloroform versus ether, but to attempt to inquire why anæsthesia in any particular case is unsatisfactory from the surgeon's point of view, and why after-complications rather often ensue. I say that I am both a co-operator and a critic and it is on this point, "co-operation," that I wish to speak first, for it appears to me that difficulties arise as regards anæsthesia from the fact that the operator and the anæsthetist do not work together. To illustrate this, let me take the case of operations on the thyroid gland. One often hears of the advisability of doing thyroidectomy under local anæsthesia, when there is evidence of pressure on the trachea; but I am sure this is very seldom necessary: in fact, personally, I have had to do thyroidectomy under local anæsthesia once only. If there are any signs of pressure on the trachea present before operation, the anæsthetic should be begun in the theatre with the surgeon ready to operate at a moment's notice. The anæsthetist, on seeing signs of distress, should cease administration and should tell the surgeon, who should rapidly expose the thyroid and turn out one lobe. Again, a point of the operation when breathing may be interfered with is while the thyroid is being dissected off the trachea. Here, again, co-operation between the operator and the anæsthetist is essential and, if the operator drops the gland back into the wound and so ceases pulling on the trachea and waits awhile, the emergency is soon over.

In another class of operation, also, co-operation is most necessary. The operator, before he dilates the sphincter ani should never fail to ask the anæsthetist whether the patient is ready, or a disaster may occur. Of one thing I am quite convinced—lightness of anæsthesia tends to increase the shock of certain procedures. Thus, I once saw a patient, who was being operated upon for carcinoma of the penis, die suddenly as the corpora cavernosa were being cut through. The anæsthesia was very light; the anæsthetist did not recognize the necessity for deep anæsthesia at that particular part of that

particular operation and the surgeon did not pause to inquire as to the depth of anæsthesia. Both were to blame.

It would be easy to go on giving examples showing the necessity for the surgeon and anæsthetist to work in unison. Sometimes the surgeon is asked to take some special measure by the anæsthetist, sometimes the anæsthetist by the surgeon. I think that, if attention be paid to this point, many difficulties will vanish.

Apart from the question of what anæsthetic is being used, this question of lightness or depth of anæsthesia is one that appeals very strongly to the operator. Anæsthetists naturally and rightly do all they can to avoid danger on the table; but I do not think they always realize that, by their endeavours to keep a light anæsthesia, they often add materially to the risks of the operation. I have already spoken of the danger from shock when such a procedure as dilating the sphincter ani is carried out, when the patient is not deeply under. But there are other risks appertaining to light anæsthesia. This especially applies to abdominal operations, in which rigidity of the abdominal wall makes it practically impossible for the surgeon to explore the abdomen properly and makes it most difficult and often dangerous for him to do such a thing as divide a band and release a loop of congested, soft, friable bowel. Again, lightness of anæsthesia, especially if the anæsthetic be ether, seems to give rise to that peculiar type of breathing characterized by forcible expiratory efforts, which push the intestine on to the edge of the knife or on to the point of the needle. This type of breathing is always embarrassing and, when it is set up, most difficult to alter. Lightness of anæsthesia is one of its causes, and I must admit that the operator being in a hurry to begin is often another. When the operator is hampered by this type of breathing, he should tell the anæsthetist and should stop manipulating, so as to give the anæsthetist a chance of putting the patient more deeply under and also of showing that he does not intend to have the risks of his operation unnecessarily multiplied.

I pass from these general remarks to a consideration of the question of the advisability or otherwise of giving drugs before the administration of an anæsthetic. I remember the day when no one thought of giving drugs before the patient was anæsthetized. That was going too far in one direction. Now I think there is a tendency to go too far in the other direction.

Scopolamine and morphia I look upon as most dangerous, and I have seen several instances in which respiration was dangerously depressed for some hours after operation when these drugs had been given. A preliminary dose of morphia alone is not so objectionable, but at the same time I do not like giving it for the following reasons: (1) There may be special reasons, e.g., the presence of nephritis, why morphia should not be given; (2) in abdominal operations a dose of morphia produces respiratory depression and often cyanosis without producing anæsthesia, the result being that it is difficult with safety to give sufficient anæsthetic to produce full relaxation; I have observed this many times; (3) the patient is longer coming round and the effort to expel mucus is delayed.

So strongly do I feel about this preliminary dose of morphia that I hold that the anæsthetist should *never* give it without consulting the operator. Probably many of those present will not agree with me in this, but it is a view I hold rather strongly. A preliminary dose of atropine is another matter. Atropine undoubtedly diminishes pharyngeal and tracheal secretion, and I think the question of its administration should be left to the anæsthetist.

With regard to the choice of anæsthetic: That ether is, for the moment, the safest anæsthetic, excluding nitrous oxide, there can be no possible doubt; but, equally, there is no doubt that it has its after-risks. Thus: (1) When ether has been given for, say, more than half an hour, the post-operative shock is, I am sure, increased. I have had many examples of this. In fact, I think that after ether has been given continually for half an hour a change over to chloroform should be made. When I used to give anæsthetics I dreaded this change over, because I thought that the deep respirations caused by the ether would result in an overdose of chloroform being taken in. Dr. McCardie has taught me that I was mistaken in this, and I am sure he is right. As regards lung complications after anæsthesia, I am convinced that these occur much more often after ether than after chloroform. I have heard it stated that the organisms must have been in the patient's respiratory passages before the anæsthetic was given and that the ether cannot be blamed. Perfectly true; no one ever suggested that pneumococci and streptococci abound in a bottle of ether. But I do say that ether is responsible for the explosive activity of these organisms by: (1) Causing deep respirations; (2) by chilling the lining membrane of the air passages and causing a great lowering of local resistance.

I should never think of asking an anæsthetist to give chloroform against his wiser will; but I do ask anæsthetists to reflect for a moment, to reconsider their position a little, to realize that ether has its dangers, that the lung complications following it are often fatal and that the deep forcible respirations caused by ether often seriously embarrass the surgeon in abdominal operations, and, at all events, I ask them not to fall out of practice in giving chloroform.

Looked at from the surgeon's point of view "gas and oxygen" is a most valuable anæsthetic in many cases; but from what I have seen the combination with it of a small quantity of ether is necessary. Of this I am certain that in most abdominal operations gas and oxygen alone without some ether do not produce sufficient relaxation.

Now as regards actual administration: I will take chloroform first and, for my own information, will ask this question—Why is the Vernon Harcourt apparatus so seldom used? I know it produces a slow induction, but the administration seems almost mechanical and I have seen it work splendidly in a great many cases. It seems to me difficult to give an overdose with it and I cannot think why its use is not more frequent. As regards open methods of giving chloroform I recall a saying of Mitchell Banks: "Plenty of chloroform and plenty of air," and in these words the true principle of giving chloroform is embodied. I know I am expressing a heretical opinion when I say that the drop-bottle and the mask are the curse of chloroform-giving. When a mask, fitting closely over the face, is used, no air except that impregnated with chloroform is breathed; while the result of using the drop bottle is that induction is very slow and more chloroform than is realized is used in getting the patient under. I am sure that the best way to give chloroform by the open method is to hold a square of two or three thicknesses of lint over the thumb and finger several inches away from the patient's face and to pour chloroform freely on this from the bottle. "Plenty of chloroform and plenty of air"—not drop, drop, drop of chloroform and no fresh air. I look upon pallor of the face as a danger signal when chloroform is being given and when this symptom comes on I would at once change to ether. If regard be paid to this point, heart failure can be anticipated. It is the patient who goes pale who is in danger in chloroform anæsthesia. It is many years

since I gave anæsthetics, so perhaps it may be argued that I am a "back number," but I really do feel that, while many improvements in the administration of anæsthetics have been introduced, the art of administering chloroform has, to say the least, not advanced.

Now, as regards ether—I was brought up to give ether with a Clover's inhaler—a safe, economical and convenient apparatus invented by the first man in this country to give special attention to the subject of anæsthetics. I was taught to use the Clover's inhaler by letting the patient first breathe through the apparatus, minus the bag, and with the indicator turned to 0. Then the indicator was gradually turned through 1 to 2 and when it pointed to about half-way between 2 and 3 the bag was put on. Upon the onset of the least laryngeal spasm or of any respiratory difficulty the indicator was at once turned back and then brought forward again slowly. From the first moment of turning on the ether, the apparatus was raised bodily from the face at every eighth breath and the patient allowed to take a breath of fresh air. When once anæsthesia had been produced, most patients could be kept well under, with the indicator at about 2. Employed in this way I believe the use of the Clover inhaler is the best way of giving ether; but I often see the apparatus with the bag attached pressed on the patient's face from the first, with the result that laryngeal spasm occurs, the patient salivates and becomes cyanosed and a successful administration becomes really impossible. I know that the administration of ether by the so-called open method is now fashionable. I hear a great deal about "open ether," but I never see the method used. What I do see is an enormous layer of many thicknesses of gauze, cotton-wool tissue, lint, &c., put over the patient's face and ether poured on it. This may be an excellent way of giving ether—it is certainly an admirable way of producing a semi-narcosis in those standing round and it is a still more excellent way of using up large quantities of ether; but I venture to say that it is neither "open" nor scientific. From an ordinary surgeon's point of view I would ask why the Clover's inhaler has been so largely abandoned and what seems to be a clumsy method substituted? Is it because it is felt that we must have innovations? I have been at my work long enough to realize that innovations are not always improvements and I feel quite sure that ether is, as a general rule, best administered by a Clover's inhaler, provided that the method of using it handed down by Clover is followed.

The following point relating to hospital construction and management has a practical bearing on the anæsthetist's work. It is the fashion nowadays, and rightly so, to keep the theatre very hot, and the air in it necessarily becomes moist. The moment the operation is over the patient is taken out into a cold corridor, and probably into a still colder and draughty lift on the way back to the wards. This is all wrong. Either warm recovery rooms, in which the patients can be kept for twenty-four hours, should be provided or else the corridors should be as warm as the wards. I look upon this practice of taking a patient through cold corridors as a frequent cause of chest trouble following anæsthesia, and I hope some day to see a hospital so constructed that this particular risk is avoided.

I have merely touched on the fringe of a very large subject by making some rather scattered and cursory remarks. It may be said that I have exceeded my province in speaking of the methods of giving anæsthetics, but a large part of my life is spent in operating theatres, and I cannot blind myself to the advantages and disadvantages of various methods of producing anæsthesia.

And, after all, years ago I had considerable personal experience as an anæsthetist. Whatever anæsthetic is being given, the anæsthetist is dealing with a powerful and a dangerous drug, and no one realizes this and sympathizes with his difficulties more than the surgeon does. If I may give a word of advice, I would say to the anæsthetist: "Do not hesitate to ask the surgeon to delay beginning his operation till the patient is properly under, and do not hesitate to ask him to pause in the middle, and even to tell him that the patient has had enough. The wise operator will welcome such advice." Lastly, I would say: "Do not be afraid of the drug you are using. Get your patient under and keep him under. Remember the risks of an unduly light anæsthesia." And I would say to the surgeon: "Bear in mind that in many operations the anæsthetist's work is more risky than yours; never forget to tell the anæthetist any peculiar points about the case, and always work with him. Above all, try not to hurry him." I try to act on these principles myself, but I am afraid I often fail.

DISCUSSION.

Dr. A. BERESFORD KINGSFORD said that as regarded the use of narcotic drugs before operations, for about two years scopolamine and morphine had been given, with atropine, as a matter of routine at University College Hospital, London. This practice was then abandoned, partly in deference to the views of the ward sisters, and now atropine only was given unless the anæsthetist gave special directions. In a certain series of high abdominal operations scopolamine and morphine were given beforehand, and pulmonary sequelæ were rather frequent; the narcotic injections were discontinued, and open C_1E_7 substituted for a semi-open ether with improved after-results attributable, in the opinion of the speaker, to the disuse of the narcotic injections. The introduction of the simple open-ether method caused a great falling off in the use of the Vernon Harcourt apparatus. This apparatus had become very elaborate, in hospital it was seldom in working order, and its employment single-handed had proved very laborious in long-lasting cases. It was still very serviceable, however, for teaching, as the anæsthetist could regulate the dosage and leave the student-administrator free to study the signs of chloroform anæsthesia.

The Clover inhaler was obsolete, but the Hewitt inhaler, with its much wider bore and other advantages, was still very useful for giving ether vapour warm and moist, i.e., mixed with steam. The full-bore metal attachment for the Ormsby bag should have a shutter by means of which the respiratory current might be directed into the open air and into the bag in equal—or any other—proportions. A few ounces of very hot water were poured into the bag before beginning anæsthesia, and could easily be renewed from time to time (*see* Buxton's "Anæsthetics," 1920, p. 381). Abdominal relaxation might almost invariably be quickly obtained by heating the container with lint half wrung out of *hot* water, the strong vapour thus evoked being rendered tolerable to the patient by its admixture with steam. In the very rare cases in which even this vapour acted too slowly the addition of some C_1E_7 mixture, as suggested by Dr. McCardie (*British Medical Journal*, 1917, i, p. 508) would soon give satisfaction both to surgeon and anæsthetist.

Professor HOBDAY said that he was very much interested in finding that so many of the remarks of Mr. Gamgee applied equally to animal patients as to man. He could speak from an experience of over 8,000 anæsthetizations on horses and cattle and more than 10,000 observations on the anæsthesia of dogs and cats. When he was a student he was taught that chloroform was a very dangerous anæsthetic for the dog, but he was able to assert now with emphasis that the dog was an ideal subject for chloroform if this anæsthetic was given in a rational manner and properly mixed with air. As an anæsthetic for the larger animals chloroform was infinitely preferable to ether, and the same was true for the smaller animals, except for puppies and cats. He agreed with

Mr. Gamgee that with a light anæsthesia there was an increased risk of shock, and when morphia and scopolamine were injected beforehand there was more difficulty in watching the respiration, and the chloroform had to be administered cautiously. He had with him to-night a modification of the original apparatus which he had demonstrated before the Society of Anæsthetists some twenty-five years ago, and which Dr. Dudley Buxton and others had admitted to be the pioneer of the Vernon Harcourt apparatus. He had now got it adapted so that it could be driven by electricity and the proportion of chloroform to air could be estimated with exactitude. The proportion of chloroform vapour to air was worked out in the Physiological Laboratory of the University of London by the late Professor Waller and by Dr. Symes.

(Here Professor Hobday demonstrated the apparatus.)

Dr. W. R. JORDAN said he had given chloroform by the open method in some 11,200 cases, mainly at an orthopædic hospital, and, therefore, obviously, in a limited variety of conditions. He had one death at the outset of his career in the case of an aged man with aortic atheroma. Otherwise he had had no death, he had had no case of acid-intoxication and no bronchitis nor pneumonia case. Recovery was quick and there was little vomiting. Two-thirds of the cases at least were those of children. He submitted that chloroform might be chosen as an anæsthetic much more often than it was.

Section of Anæsthetics.

President — Dr. A. L. Flemming.

On the Narcotic Action of Purest Ether.

By Professor W. Storm van Leeuwen.

Director of the Pharmaco-therapeutical Institute of the University of Leyden (Holland).

Before entering into a discussion on the narcotic action of purest ether, I would like to say a few words on the question as to how far a pharmacologist is qualified to judge these matters. I first want, so to speak, to define the limits of my competence.

It is a general rule that results of experiments carried out on animals in the laboratory should not be applied to human beings without due caution. This rule especially holds in all cases where an action on the central nervous system is involved. And if the action of some of the alkaloids acting on the nervous system, as morphine, scopolamine, or atropine were the subject of our discussion, I should have entered this field with much more hesitation. Morphine, for example, differs considerably in its action on various animals. It induces excitation in the cat, but narcosis in the dog; it will induce vomiting in the dog, but seldom in men; and many other differences may be found.

If, however the action of narcotics belonging to the aliphatic series—like ether, chloroform, &c.—be studied, one is struck by the fact that the action of these drugs varies very little in different animals, and this assumption holds good from the qualitative as well as from the quantitative point of view. For example, there is no appreciable difference in the qualitative action of ether on mice, guinea-pigs, rabbits, cats, dogs, monkeys and men. Some years ago I studied, quantitatively, the action of ether and chloroform, and the effects of various mixtures of them, on various animals, and, on comparing my figures with those obtained by others, it was found that the amount of narcotic present in the air, or the concentration of narcotic present in the blood, during various phases of narcosis, was alike in different animals.

In addition to that, special investigation taught us that for these narcotics the relation between the concentration present in the blood of animals and the action on the nervous system is a very simple one. Expressing it in our nomenclature, the concentration-action curve of these narcotics follows a straight line. This has the following meaning: If the dose of narcotic is increased, the action will increase accordingly, so that a double concentration of the narcotic in the blood will give a two-fold narcotic action. For most of the other drugs, for example the alkaloids, this is different; their

concentration-action curve has the form of a hyperbola, which means that in certain regions a slight increase in concentration will induce a great variation in action, whereas in other regions it is the reverse. Drugs belonging to this last group may always—especially when administered in combination with similar drugs—tend to produce unexpected effects, like potentiated synergism, reversal of action, &c. This feature is not to be expected in working with the narcotics of the aliphatic series.

We have carefully studied this phenomenon with many drugs acting on many animals, and we always found simple relations. Ether, chloroform, chloral hydrate, urethane and similar drugs, will in small concentration produce slight symptoms, and, if the concentration of the drug in the blood is gradually raised, the narcotic action will rise accordingly; and slowly, without interruption, the phase of respiratory inhibition will be reached. If two of these drugs are used in combination, simple relations again prevail. The action of one drug will be added quantitatively to the action of the other drug, without any sign of so-called potentiated synergism. Figs. 1 and 2 give examples of the two different forms of concentration-action curves.

The fact, then, that narcotic action of ether and of drugs of the same group is the same qualitatively and quantitatively for all laboratory animals and for man, added to the fact that the concentration-action curves of these drugs run a straight course, so that no "surprises" are to be dreaded, opens the possibility for a pharmacologist to discuss the narcotic action of purest ether before a group of men who use the same drug in their practice daily, and whose practical knowledge of the subject by far surpasses mine.

As you all know, the narcotic action of ether was discovered accidentally. A flask containing ether broke, and some moments later a laboratory attendant was found narcotized on the floor by the chemist Jackson. *A priori* considerations might lead to the conviction that it is highly improbable that the accident should have happened with precisely the best narcotic which exists; and so the supposition, that a better narcotic than ether may be found, is not at all absurd. It has been claimed in recent years that combinations of ether with other substances exist, the narcotic action of which exceeds the action of pure ether considerably. Indeed some investigators supported the view that pure ether has no narcotic action at all, but that the narcotic action of ether, noticed from Jackson's time till now, is only due to impurities present in ether. As a consequence of this assumption two preparations, called "Cotton's ether" and "ethanesal," were put on the market and were claimed to have a narcotic action far beyond that of pure ether.

This conception attracted the attention of a number of surgeons and anæsthetists in my country, and it so happened that the first rumours of the non-activity of ether came to us precisely at a time when some factories in Holland were doing all they could to produce very pure narcotic ether, as the German ether, which was in use in pre-war times, was no longer available.

Consequently there arose a very unfortunate position, in which surgeons lost their faith in "purest" ether, whereas the ether manufacturers, whose ether had reached a high degree of purity, were at a loss how to proceed.

When, in this state of affairs, I was asked by our largest ether manufacturers, "Nederlandsche Gist- en Spiritusfabriek," to investigate the matter experimentally, I felt that this request could not be refused. So, with the assistance of Dr. Nijk, I undertook to investigate the following points:—

(1) Has pure ethyl ether a narcotic action?

(2) Is it possible to increase the narcotic action of pure ether by adding

"impurities" or other constituents to it? This last question included a study of Cotton ether and ethanesal, which were said to consist of pure ether contaminated with known amounts of known impurities.

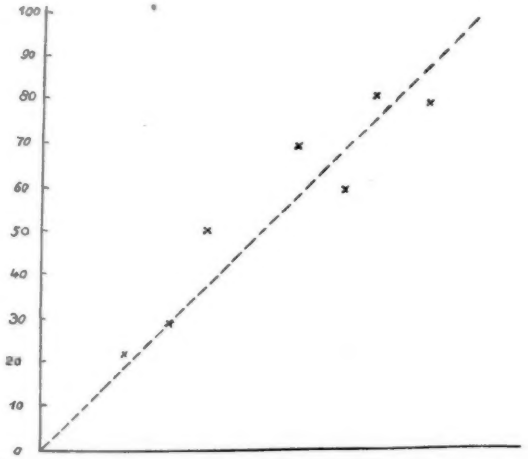


FIG. 1.—CONCENTRATION-ACTION CURVE OF CHLORAL HYDRATE.

Abscissa: Dose of chloral hydrate in milligrammes per kg. animal, injected intravenously. Ordinate: Action of the drug on homolateral reflexes of decerebrated rabbits. Each point plotted gives the average of six experiments.

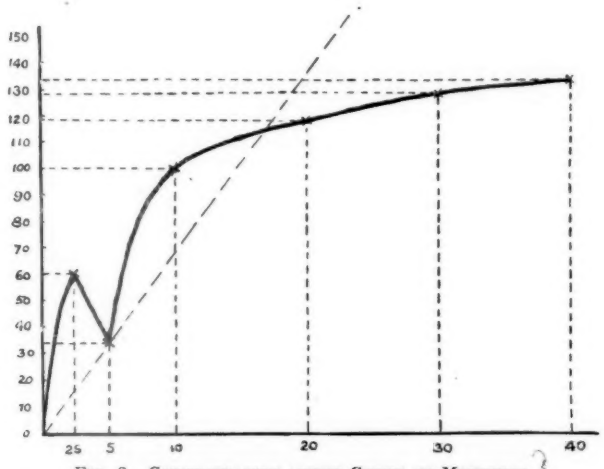


FIG. 2.—CONCENTRATION-ACTION CURVE OF MORPHINE.

Abscissa: Dose of morphine in milligrammes per kg. animal, injected intravenously. Ordinate: Action on homolateral reflexes of decerebrated rabbits.

The Narcotic Action of Purest Ether.

As a first step we investigated the degree of purity of a number of commercial "narcotic ethers," including ethers of Kahlbaum, Schering, Mallinckrodt, various samples of ether from our Dutch firm, Cotton's ether, ethanesal, and a very pure ether prepared in our own laboratory.

Various tests for purity, among them those described in the pharmacopœias of different countries, were applied. Briefly stated, the result was that all ethers had reached a considerable state of purity (apart from the impurities consciously added to Cotton's ether and ethanesal) so that all of them—with the exception only of Mallinckrodt ether—were equal to the demands of the Dutch pharmacopœia. But, on the other hand, none of them were absolutely pure. Small amounts or traces of aldehydes or alkylenes are usually found. Even the purest ether—ether G. and S. Fabriek, *absolutum purissimum pro narcosi*—showed a slight reaction with Nessler's reagent on standing.

The statement that pure ether is not narcotic is due to Cotton and to Mackenzie Wallis.[1] Cotton's ether and ethanesal are claimed by them to be pure (and consequently non-narcotic) ether to which impurities had been added. In order to investigate the correctness of their statement, it was only necessary to prepare an ether which would be of the same degree of purity as Cotton's ether or ethanesal, minus the added impurities. As a matter of fact it would not have been necessary to take the trouble to prepare new samples of ether, since the purity of some of our ethers was greater than the purity of the ethyl ether (minus the added impurities) contained in Cotton's ether and ethanesal. As these ethers (viz., Schering Kahlbaum ether, and ether *absolutum purissimum pro narcosi* G. and S.) have been used thousands and thousands of times for narcosis, the deduction may be made, that the ethyl ether of Cotton, ether and ethanesal (which is even less pure than the ethers above mentioned) certainly must be also active. So that Cotton and Mackenzie Wallis must have made a mistake.

It was, however, deemed advisable to bring a more direct proof that purest ether has a narcotic action.

Ether was prepared from pure sulphuric acid and ethyl alcohol; it was repeatedly washed with pure water, and treated with calcium chloride. On each occasion when distillation was necessary, only the middle fraction of the distillate was taken. The ether was subsequently distilled over lime, and thereafter kept over metallic sodium. Of this ether, five litres were taken and distilled. From the middle fraction, which distilled exactly at 34·6° C. at 760 mm. Hg (being the exact boiling point as indicated by Timmermans and not significantly different from that of Wade and Finnemore) one litre was taken. This is called "purest laboratory ether." On applying the purity tests, this ether proved to be very pure, only traces of alkylene were found.

The narcotic action of this ether was tested, and no differences whatever from other ethers were found. It is superfluous to give any more details on this matter as Dale, Hadfield and King[2] have studied this phase very carefully and they published their results a year ago. They state that the purest sample of ether which may be made with ordinary laboratory methods has a normal narcotic action. This statement we can confirm entirely.

Discussion of this part of the question then could have been considered as closed were it not for the following reason.

[1] Mackenzie Wallis, *Proc. Roy. Soc. Med.*, xiv, No. 10, 1921. Mackenzie Wallis and Hewer, *Lancet*, 1921, i, p. 1173.

[2] Dale, Hadfield and King, *Lancet*, March 3, 1923, p. 424.

Dale, Hadfield and King had proved—and we had confirmed it—that the purest ether which may be made is active. The statements of Cotton and Mackenzie Wallis were in this way shown to be incorrect, but this did not as yet exclude every possibility that impurities might be the real narcotic factors in ether. None of our ethers are—theoretically speaking—absolutely pure. Moreover, it must be regarded as theoretically possible that ether contains a small amount of an unknown substance, which also boils at 34·6° C and which is responsible for the narcotic action.

I should like to call attention to the fact that, personally, I would never have undertaken to propound such a theory. It may be remarked, in passing, that a similar theory may be put forward for every action of any other drug. Under ordinary conditions the work of Dale and his co-workers would have been entirely sufficient. In this particular case, however, and with the object of excluding all possibility that a claim similar to that of Cotton and Mackenzie Wallis might be made in the future, we determined to go a step further and to prepare a sample of ether of undeniable purity.

We early recognized that all difficulties would disappear if we could crystallize ether. Hence attempts were made to secure a preparation containing ether in crystalline form.

Crystallization of ether, by cooling ether, could not give the desired results, since such crystals might be contaminated with unknown impurities from the mother liquor. Moreover, the exact melting point of such ether is not known. So we hoped to obtain chemical compounds which would crystallize with ether. The known Grignard compounds (ethyl magnesium bromides, magnesium iodo-etherates and beryllium chloro-etherates) were unsuitable for the purpose, since such compounds would decompose on heating and make the ether obtained impure. Hence an organic substance, which crystallizes easily with ether and which does not decompose on subsequent heating, had to be found. After a long search Dr. Nijk found that benzidine answers completely to these demands.

If pure and perfectly dry ether is mixed with benzidine, heated to boiling point and subsequently cooled, large crystals of prismatic form are found. Upon the heating of these crystals the ether very easily becomes free, and the residual mass of benzidine may be used several times for the same process.

Benzidine possesses the following advantages: It does not decompose ether during this process, but it destroys peroxides, especially ethylperoxide, diethyl-peroxide and acetal hyperoxide.

Ether obtained in this way is absolutely pure. Its boiling point is exactly at 34·6° C. (760 mm. Hg), and it gives negative results with all reagents for impurities contained in the various pharmacopœias, including the reaction with ferrosulphocyanide which formerly was present in the German pharmacopœia, but had to be left out as being too sensitive. This ether is not only free from any peroxides, but also it *does not form peroxides* if left in the sunlight for a considerable time. If air (freed from aldehyde and acids) was conducted through ether of crystallizaion in the sunlight for some hours no peroxides were formed.

Moreover it was found that also ether, obtained by redistilling the mother liquid used in making ether of crystallization, was entirely free of peroxides and did not form peroxides if left exposed to the air. This is of advantage, since the process of making ether of crystallization is a slow one. On using 1 kg. of benzidine only 150 gr. of ether of crystallization are obtained. Since the distillate of the mother liquor is for practical purposes to be considered as equal

to ether of crystallization, the crystallization process is an effective one in purification of narcotic ether.

Our first step after obtaining ether of crystallization was to investigate its narcotic action and to compare this with the action of other ethers.

Narcotic Action of Ether of Crystallization and other Ethers.

The proof, that the purest ether we could prepare, viz., ether of crystallization, had indeed a narcotic action, was easy to obtain. Animals were narcotized with this ether, and no difference from other ether could be perceived. In the surgical clinic a child who was to be operated upon for appendicitis was narcotized with ether of crystallization without any additional drug being given, and the narcosis was in no way different from others, indeed, its course was very smooth. The amount of ether used was the same as is usual in such cases, and the patient's recovery was uneventful. Although in this case it was shown that purest ether of crystallization has a narcotic action which, on clinical observation, does not differ from that of known ethers, it had to be proved that our ether had *quantitatively* exactly the same narcotizing power as other ethers. This point was investigated by experiments on animals carried out as follows :—

Experiments on Mice.

Some mice are put in a large 10-litre bottle, which is closed by a rubber stopper. The stopper is perforated by the needle of a syringe for injecting ether. If 1½ c.c. of ether at 15° is introduced into the bottle and the bottle is turned several times to obtain an even mixture of ether and air, the air in the bottle will contain 3 volumes per cent. of ether, and the mice, which have been put into the bottle, will be narcotized (i.e., they are sleeping and do not make spontaneous movements if turned on their backs) in three to ten minutes. This is the case with mice weighing about 20 grm. With purest ether of crystallization exactly the same results were obtained. Narcosis ensued after application of 1½ c.c. ; smaller quantities had no effect, larger quantities gave a deeper anæsthesia.

Experiments on Cats.

Two series of experiments were performed.

In the first series the object was to determine the amount of ether present in the animal's blood, if narcosis was pushed so far that respiration stopped. This method has been used in a large number of experiments by the present author.[1] Cats are narcotized with the ether to be investigated, tracheotomy is performed and a cannula is fixed in the trachea. Air is blown through a Woulff bottle containing ether, the air saturated with ether then passing through a narrow rubber tube which is introduced into the tracheal cannula of the animal. In this way narcosis can be continued easily. The animal is narcotized very deeply, and the depth of the narcosis is increased gradually, till the limit at which respiration stops is nearly reached. In this phase the animal is kept for half an hour, then narcosis is gradually pushed further till respiration stops entirely. At this moment the tracheal cannula is clamped, the thorax at once opened, the right heart incised, and 10 c.c. of blood are drawn and introduced into a flask containing a picric acid solution. The ether is distilled off and a quantitative determination is made, Le Heux's modification[2] of a method described by Nicloux being used.

[1] W. Storm van Leeuwen, Pflüger's *Archiv. f. d. gesammt. Physiol.*, 1916, clxvi, p. 65.
[2] Le Heux, *Zeitschr. f. physiol. Chemie*, 1919.

In using purest narcotic ether, prepared in our laboratory, the figures given in Table I are obtained, which show that on an average of nineteen experiments a concentration of 0·114 per cent. in the blood is found.

TABLE I.—PERCENTAGE OF ETHER IN VENOUS BLOOD OF CATS NARCOTIZED TILL RESPIRATION STOPS. PUREST ETHER PREPARED IN THE LABORATORY.

Exp. No.	Percentage of ether in blood	Exp. No.	Percentage of ether in blood	Exp. No.	Percentage of ether in blood
1 ...	0·11 per cent.	7 ...	0·12 per cent.	13 ...	0·14 per cent.
2 ...	0·11 ,,	8 ...	0·15 ,,	14 ...	0·12 ,,
3 ...	0·10 ,,	9 ...	0·09 ,,	15 ...	0·11 ,,
4 ...	0·10 ,,	10 ...	0·10 ,,	16 ...	0·12 ,,
5 ...	0·09 ,,	11 ...	0·12 ,,	17 ...	0·12 ,,
6 ...	0·12 ,,	12 ...	0·10 ,,	18 ...	0·10 ,,
				19 ...	0·14 ,,

Average of all values 0·114 per cent.

In six animals narcotized with ether of crystallization we found an average concentration of 0·118 per cent. Ethanesal was investigated in five animals and gave an average of 0·114 per cent.; narcotic ether of a Dutch factory (Ned. G. en S. Fabriek) gave 0·12 per cent. as an average of five experiments.

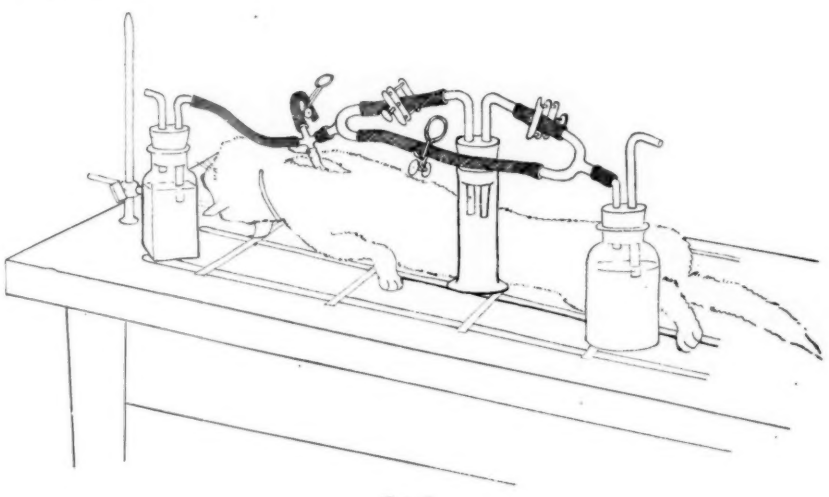


FIG. 3.

These experiments prove that the quantity of ether present in the animal's blood, which is just sufficient to cause cessation of respiration, is the same for pure laboratory ether, for purest ether of crystallization, for ethanesal, and for a sample of commercial narcotic ether.

In a second series of experiments we determined the amount of ether which is sufficient to keep a cat within a certain depth of narcosis during an hour.

A cat is narcotized with ether of the sample which has to be investigated. Tracheotomy is performed, and a cannula is fixed in the trachea. This cannula it connected with a rubber tube, which leads to a small apparatus, shown in fig. 3, (p. 23). The cat breathes spontaneously by means of "Müller valves," the inspired and expired air being forced to follow the desired direction. By opening or closing a clamp, the inspired air is led through a graduated cylinder containing a known amount of ether from the sample under investigation. On one side of the animal the peroneal nerve is dissected out. Every two minutes the nerve is stimulated electrically to control the depth of narcosis. Throughout the experiment the quantity of ether to be inspired by the animal is so regulated that stimulation of the peroneal nerve with an electric current of constant strength just gives a slight reflex movement in the contralateral hind leg (crossed extension reflex). After the animal has been kept in this phase of narcosis for a quarter of an hour, the amount of ether in the graduated cylinder is noted and the experiment is continued for another hour. Then again a reading is taken, and the difference of both readings gives the amount of ether used during an hour. Then the animal is allowed to recover, and six or eight hours later the same experiment is repeated with another sample of ether, and after that the animal is killed and an autopsy made. If pulmonary or other lesions are found, the results are rejected. If for one animal sample A is used in the morning and sample B in the afternoon, the next day, when experimenting on another animal, sample B is used in the morning and A in the afternoon. Each sample is used in the case of several cats, and only averages of these determinations are compared. This method gives reliable results. With the use of the purest narcotic ether prepared in the laboratory we found the figures given in Table II.

TABLE II.—CAT NARCOTIZED DURING ONE HOUR WITH INTERVAL OF SIX HOURS.

Quantity of Ether used in c.c.

Exp.	First observation	Second observation six hours later
1	8 c.c.	7 c.c.
2	8 ,,	7 ,,
3	7·7 ,,	7 ,,
4	11·5 ,,	11·5 ,,
5	12 ,,	13·5 ,,
Average	9·4 ,,	9·2 ,,

The table shows that, as a rule, the first observation gives a figure somewhat higher than the second observation. This does not show clearly in the average figures, since the course of experiment 5 deviated from the other experiments. Also, later experience taught us, as could have been anticipated, that afternoon figures (second observation) are somewhat lower than morning figures.

In the next series of experiments on five cats, we compared the action of ether of crystallization with the action of the pure laboratory ether used in Table II.

The results are shown in Table III.

TABLE III.—QUANTITY OF ETHER USED DURING ONE HOUR'S NARCOSIS.

Exp. No.	First observation Ether of crystallization	Second observation Pure laboratory ether
1	3 c.c.	5 c.c.
2	6 ,,	4·5 ,,
3	4·5 ,,	4 ,,
Added	13·5 ,,	13·5 ,,

Exp. No.	First observation Pure laboratory ether	Second observation Ether of crystallization
4	8 c.c.	6·5 c.c.
5	5 ,,	6 ,,
Added	13 ,,	12·5 ,,

These experiments show that the narcotizing effect (measured by determining the amount of ether necessary to keep an animal in a certain depth of narcosis for an hour) of purest ether of crystallization does not differ from the effect of commercial narcotic ethers.

We feel that on the basis of these observations the conclusion may be drawn that the narcotic action of the purest ether which can be obtained does not differ from the action of pure laboratory ether or of commercial narcotic ether. In relation with the experiments first mentioned, viz., those in which the concentration in the blood which causes the respiration to stop is determined, the criticism might be suggested that the figures thus obtained do not represent a narcotic but a toxic or lethal action of the ether. In our opinion such a criticism would be unsound. It has been pointed out above that the concentration-action curve of ether is very straight. If the concentration in the blood is gradually increased, the depth of narcosis also gradually increases. Different centres of the nervous system are successively involved, and finally the respiratory centre is narcotized. This narcosis does not differ from the narcosis of other centres, since it also is a temporary and perfectly reversible inhibition of the function of a nervous centre. Inhibition of respiration by ether is one easily determined point on the concentration-action curve of this drug.

Investigation of the Influence of Addition of Impurities to Purest Ether.

After it had been proved that purest ether was a narcotic which manifested the same narcotic action as commercial narcotic ethers, the second question, mentioned above, remained for investigation, viz., the possibility of increasing the narcotic action of purest ether by the addition of impurities.

In most of these experiments various products, which may occur as impurities in ether, were added to our purest laboratory ether, and the effect of these mixtures was studied in the two ways described in the preceding section.

Since, however, only very small differences were found, we used in the majority of the experiments only the method by which the lethal concentration of ether in the blood is studied, as we assumed that if great differences in narcotizing action were present, this would involve differences in lethal concentration. Moreover, if such differences did occur, they would not, if of any consequence, escape our attention during the performance of the experiments.

The results of all these experiments were rather astonishing, as there appeared to be very little difference in the narcotic action of most of the ethers and mixtures studied. Indeed, purest ether and crude commercial ether did not differ considerably in narcotizing effect. It must be stated emphatically, that these experiments teach nothing about the suitability of these ethers for producing anæsthesia in man. A very intensive purification of anæsthetic ether is necessary to exclude the danger caused by the irritative action of impurities on mucous membranes; moreover, some impurities may be harmful to the kidneys, liver and even to the heart. These properties exclude such impure ethers from clinical use, but do not influence in a marked degree their

narcotizing action; and since this was precisely the action we wanted to study, the other actions of impurities are only casually mentioned in this paper. Only in those rare cases in which a mixture seemed to have a *better* narcotizing effect than purest ether have we been obliged to make a study of the *side* actions of these mixtures. As will be shown later, this task was not difficult, since in those ethers which differed from purest ether in narcotizing effect we easily detected other actions which excluded their use in therapeutics.

TABLE IV.

Substance	Lethal concentration of ether in blood in per cent.	Number of determinations made
Purest lab. ether	0·114	19
Ether of crystallization	0·118	6
Ethanesal	0·114	5
Ether pur. pro narc. (Delft)	0·12	5
Impure ether (Delft)	0·125	2
Impure commercial ether	0·104	5
Purest ether + ½ per cent. acetone	0·115	4
,, ,, + 1 per cent. methylethylketone ...	0·127	6
,, ,, + 5 per cent. methylethylketone ..	0·105	3
,, ,, + 1 per cent. n-butylalcohol ...	0·128	5
,, ,, + 1 per cent. iso-butylalcohol ...	0·14	2
,, ,, + 1 per cent. n-butylalcohol + 1 per cent. iso-butylalcohol ...	0·12	1
,, ,, + ½ per cent. peroxides	0·10	3
,, ,, + 1 per cent. peroxides	0·11	1
,, ,, + ½ per cent. amino-ethylalcohol ...	0·115	6
,, ,, + 1 per cent. amino-ethylalcohol ...	0·13	5
,, ,, + ½ per cent. iso-amylalcohol ...	0·104	5
,, ,, + 1 per cent. n-butylalcohol + 0·5 per cent. iso-amylalcohol ...	0·104	5
,, ,, + 1 per cent. iso-amylalcohol ...	0·09	6
,, ,, + 1 per cent. mesityloxide ...	0·11	5
,, ,, + 1 per cent. n-propylalcohol ...	0·115	5
,, ,, + ½ per cent. pyridine	0·12	3
,, ,, + ½ per cent. furfurol	0·10	1
,, ,, + 1 per cent. furfurol	0·143	3
,, ,, + 1 per cent. ethylacetate ...	0·065	4

TABLE V.—PUREST ETHER COMPARED WITH COTTON ETHER.

Quantity of ether used to keep a cat in narcosis during one hour.

Exp.	Purest ether	Cotton ether
1	6 c.c.	5 c.c.
2	8 ,, ...	8 ,,
3	8·5 ,, ...	9 ,,
4	7·5 ,, ...	7·25 ,,
Average ...	7·5 ,,	7·3 ,,

Result: no difference between Cotton ether and purest ether.

TABLE VI.—ACTION OF PUREST NARCOTIC ETHER COMPARED WITH ETHER DELFT (G. en S. Fabriek).

Quantity of Ether used (in c.c.) for cats during one hour narcosis.

Exp.	Purest ether	Ether Delft
1	7	6
2	11	10
3	7	6
4	10	9
5	8	9
Average ...	8·6	Average ... 8

Result: no difference between the two ethers.

TABLE VII.—PUREST ETHER COMPARED WITH ETHER + 5 PER CENT. METHYLETHYLKETONE

Investigations on Cats.

First observation purest ether + 5 per cent. methylethylketone	Quantity of ether used for one hour narcosis	Second observation	Quantity of ether
	5 c.c.		7 c.c.
	9 ,,		9 ,,
	8 ,,		9 ,,
	7 ,,		8 ,,
	4 ,,		6 ,,
Average	6·6 ,,	Average	7·8 ,,

TABLE VIIa.

First observation purest ether		Second observation ether + ketone	
	9 c.c.		3 c.c.
	10·5 ,,		4 ,,
	11 ,,		7 ,,
	7 ,,		6 ,,
	9 ,,		8 ,,
Average	9·3 ,,	Average	5·6 ,,

Results.—The quantity of ether used is smaller in all experiments with methylethylketone. The difference of course is more demonstrable in cases in which first purest ether, and afterwards methylethylketone ether, is given, since the figures of the second observation always tend to be a little lower than those of the first observation.

TABLE VIII.—ACTION OF PUREST ETHER COMPARED WITH CRUDE COMMERCIAL ETHER.

Quantity of Ether used (in c.c.) in Cats during one hour's narcosis.

Exp.	Purest ether	Crude commercial ether
1	8·5 c.c.	7 c.c.
2	19 ,,	24 ,,
3	13 ,,	9 ,,
4	10 ,,	10 ,,
5	18 ,,	17·5 ,,
6	20 ,,	20 ,,
Average	13·1 ,,	Average 12·9 ,,

Result: No difference.

TABLE IX.—ACTION OF PUREST ETHER COMPARED WITH PUREST ETHER + 1 PER CENT. VINYLETHYLETHER.

Quantity of Ether (in c.c.) used for Cats during one hour's narcosis.

Exp.	Purest ether	Ether + 1 per cent. vinylethylether
1	7 c.c.	6 c.c.
2	7 ,,	6 ,,
3	10 ,,	12 ,,
4	6 ,,	8 ,,
Average	7·5 ,,	Average 8 ,,

Result: No difference.

TABLE IX*a*.

Exp.	Purest ether	Ether + 0·5 per cent. vinylethylether
1	13 c.c.	14 c.c.
2	5 ,,	6 ,,
3	6 ,,	8 ,,
4	4 ,,	5 ,,
5	6 ,,	5 ,,
6	13 ,,	14 ,,
7	6 ,,	8 ,,
8	6 ,,	5 ,,
9	6·5 ,,	6·5 ,,
10	4 ,,	3·5 ,,
11	5 ,,	6 ,,
12	4 ,,	5 ,,
13	3 ,,	6·5 ,,
14	8 ,,	8 ,,
15	4·5 ,,	8 ,,
Average ...	6·3 ,,	Average ... 7·2 ,,

TABLE X.—ACTION OF PUREST ETHER COMPARED WITH OTHER ETHERS. (Average figures.)

Quantity of ether used in one hour.

Purest ether	9·4 c.c.	Purest ether	9·2 c.c.
,, ,, ...	7·5 ,,	Cotton ether	7·3 ,,
,, ,, ...	8·6 ,,	Narcotic ether (Delft)	8 ,,
,, ,, ...	7·8 ,,	Ether + 5 per cent. methylethylketone	6·6 ,,
,, ,, ...	9·3 ,,	Ether + 5 per cent. methylethylketone	5·6 ,,
,, ,, ...	13·1 ,,	Crude commercial ether	12·9 ,,
,, ,, ...	7·5 ,,	Ether + 1 per cent. vinylethylether ...	8 ,,
,, ,, ...	6·3 ,,	Ether + 0·5 per cent. vinylethylether	7·2 ,,

Experimental Results.

Table IV gives the results of all experiments in which the lethal concentration was determined. For each substance, and for each mixture, only the average dose determined is given, and the number of experiments made has also been recorded. In cases in which a sample gave, on one or two examinations, a figure approximating to the average for purest ether, we felt that one or two experiments would be sufficient. In all important cases five or more experiments with the same ether were made.

Tables V to X give the results of experiments in which the quantity of ether necessary to narcotize a cat for one hour was determined.

A survey of these tables shows that the values obtained for most of the samples deviate little from the figures of purest narcotic ether. However, the following samples create an exception.

Impure Commercial Ether.—The lethal concentration is 0·104 per cent., which implies that the narcotic action of this ether is higher than that of pure ether. Since crude commercial ether notoriously contains many irritating substances—which was clearly demonstrated by an excessive secretion of saliva from our cats under experiment—it was of no use investigating this ether further.

Ether with peroxides, with furfurol, and *with ethylacetate* (which was very toxic), had to be discarded for the same reason.

The only samples which seemed worth a further examination were ether with iso-amylalcohol and with methylethylketone. Iso-amylalcohol lowered the lethal dose, which shows that its narcotic, or toxic, effect is greater than that of purest ether. Further experiments on this point appeared to be unnecessary, as the mixture proved to be too toxic. Cats narcotized two or three times with ether containing iso-amylalcohol secreted an immense quantity of saliva and died. We do not know whether the small quantities of iso-

amylalcohol found in ethanesal will prove to be dangerous, but we believe that they certainly will exercise no beneficial action, and that if they have any action at all it is presumably a noxious one. The use of this substance in ether has to be discarded.

Methylethylketone.—This substance added in quantities up to 5 per cent. to purest ether did not seem to affect the lethal dose materially. It was, however, noticed that narcosis in these cases went very smoothly, and there was little salivary secretion; moreover, we had the impression that during narcosis with mixtures of ether and this substance less ether had to be used than usually. Hence we considered it worth while to investigate mixtures with this substance more thoroughly. This was the more desirable as it has been stated by Mackenzie Wallis that ethanesal contains 5 per cent. of methylethylketone.

Investigations on Purest Ether + 5 per cent. Methylethylketone.

The first point to be investigated was, whether indeed methylethylketone intensifies the action of ether. If this should be proved, the next point for investigation was whether the relation between therapeutic concentration and toxic concentration was advantageous, which includes a study of eventual toxic symptoms due to methylethylketone. Finally, granting that all these inquiries gave favourable results, it would be necessary to find the optimal concentration of methylethylketone to be added to purest ether.

Estimation of the Quantity of Ketone-ether Needed to keep a Cat Narcotized for One Hour.

Using the technique already described above we made a number of experiments tabulated in Table VII, in which it was found that, as a rule, during narcosis with ether + 5 per cent. methylethylketone, less ether is used than with purest ether alone. It is not yet clear how this action is to be explained. The boiling point of methylethylketone is higher than that of ether, so it may be expected that the boiling point of the mixture is higher than that of ether alone. This may cause a slower evaporation from the ether bottle, and may cause, with the technique used, less waste of ether during the experiments. On the other hand, methylethylketone is a toxic substance. Mice brought into an atmosphere containing 3 volumes per cent. of methylethylketone are narcotized within a few minutes, but, on withdrawal of the narcotic, they remain unconscious for many hours. So it may be possible that addition of 5 per cent. of methylethylketone to ether increases the toxicity, and this might explain that less ether is used to maintain narcosis during an hour. This point will have to be investigated more fully.[1]

[1] *Note.*—We have recently conducted some experiments which decide this question. Five cats were narcotized every second day with ether containing 5 per cent. of methylethylketone, the narcosis always lasting one hour.

A second batch of cats was narcotized in the same way with pure ether. It is known that most cats will stand the procedure for a considerable time if pure ether is used. One of our cats was narcotized seven times with ordinary pure ether.

In the series narcotized with ether + methylethylketone all the cats died after having been narcotized two or three times. They did not die during narcosis, but afterwards. In all cases we found multiple hæmorrhages in the lungs, liver, kidney, peritoneum. The urine contained blood and bilirubin in most instances.

This fact shows that the addition of 5 per cent. methylethylketone to ether makes it very toxic, so that the use of such ether in men would be criminal.

It may be remembered that ethanesal, is claimed to contain 5 per cent. of methylethylketone, but it contains only n-butylalcohol.

It is self-evident that no time need be wasted in a further inquiry into the effects of addition of methylethylketone to ether.

Therefore, as a result of these experiments we are enabled to state that most of the impurities added do not affect the narcotic action of ether materially. Some impurities do, but the addition of these impurities to pure ether, with the view of obtaining a better narcotic ether, is inadvisable, since they increase the toxicity of ether.

The final conclusion arrived at is that the purest ether which can be obtained is the best narcotic.

It is necessary briefly to discuss two commercial preparations, "Ethanesal" and "Cotton's ether," since it has been claimed that both preparations possess a narcotic action superior to that of pure ether.

Ethanesal.

Wallis and Hewer claim that purest ether has no narcotic action; the narcotic effect of their product "Ethanesal," is said to be due to ketones. In one of their first contributions to the subject[1] they state that "Ethanesal" is a mixture prepared by taking ether, purified by treatment with permanganate and copper sulphate, and adding CO_2, ethylene and an unspecified amount of ketones (comprising those in the middle of the series). In a later paper, Wallis identifies the "ketones" as methylethylketone, added in amounts of 2 or 5 per cent.

Hewer states that Mackenzie Wallis has shown that animals can be anæsthetized with a mixture of methylethyl- and vinylethylketones, provided that the temperature is high enough for adequate vaporization.

It is unnecessary to enter into a detailed description of this matter, since it has been extensively dealt with in the paper written by Dale, Hadfield and King.[2]

On examination of "Ethanesal," bought in England on the open market, we found: (*a*) That ethanesal contains CO_2, the major part of which at once escapes on opening the container. (*b*) Reaction for ethylene (conduction through bromine water and reaction with Bayer's reagent) slightly positive. (*c*) No aldehydes. (*d*) No methyl ketones or other ketones (reaction with sodium nitroprusside and with alkaline mercuric oxide; no precipitate with paranitrophenyl-hydrazine). (*e*) No peroxides. (*f*) No methyl compounds (reaction of Riche and Bardy). (*g*) A small amount of iso-amylalcohol. (*h*) A considerable amount of normal butylalcohol.

Our analysis entirely agrees with the analysis of Dale, King and Hadfield, only we found a small amount of iso-amylalcohol. The presence of this substance was ascertained as follows:—

If ethanesal is shaken up with water and subsequently separated, the watery layer becomes cloudy on heating and clears up again on subsequent cooling. The residue of evaporations of ethanesal gives the smell of iso-amylalcohol, but it is difficult to prove the presence of an alcohol function.

The main facts are, then, the absence of methylethylketones, or other ketones, the presence of only very small amounts of ethylene, and the presence of normal butylalcohol. It is of course impossible to ascertain why a product that is said to contain methylethylketones does not contain this substance, but does contain n-butylalcohol. But, as has been pointed out by Dale, King and Hadfield, it is quite possible that the manufacturers of ethanesal intended to add iso-butylalcohol, which on oxidation would have produced methylethylketone.

[1] Wallis and Hewer, *Lancet*, 1921, i, p. 1173

[2] Dale, Hadfield and King, *Lancet*, 1923, i, p. 424.

COTTON'S ETHER.

This is said to be pure ether containing ethylene and CO_2. On chemical examination of this ether we found: (*a*) Little or no CO_2; (*b*) no acetone or aldehydes; (*c*) no peroxides; (*d*) ethylene.

Our animal experiments with both ethers have shown that their narcotic action is exactly the same as that of purest ether.

The fact that these ethers, in the hands of some well-known anæsthetists, yielded good results, need not surprise us. Apart from the impurities purposely added (none of them, through the fortunate mistake of the manufacturers of ethanesal, are toxic), ethanesal and Cotton's ether are very pure ethers. Since the requirements of the British Pharmacopœia are not very exacting, and since, during the war and shortly afterwards, narcotic ethers containing other impurities may have been put on the market, ethanesal may have been superior to some other samples of ether, *not* because it contains substances which increased its narcotic action, but because it was a pure ether.

Finally, one other point has to be mentioned. It often happens that pure ether, which does not contain peroxides, forms these bodies on standing. It is known that exposure to air and light increases this tendency. On examination of a number of ether samples we found that some ethers do not show this peculiarity. Among these were: Schering's ether, ethanesal, ether of crystallization, and also a distillate of the mother liquor of crystallized ether.

Schering's ether contains a small amount of alcohol. Ethanesal contains butylalcohol. It is probable that the addition of a small amount of alcohol may prevent the formation of peroxides, i.e., it may stabilize the ether.

Temporary addition of benzidine to pure ether has a similar effect. Probably benzidine acts by destroying the last traces of peroxides or aldehydes and thus prevents the new formation of peroxides.

It is evident that the question of stabilization of ether is of interest. We intend to study this point more in detail in the near future, our aim being to find a simple and effective method for stabilization of pure ether. We know already that the addition of benzidine has a stabilizing effect. This, however, would involve an extra distillation of the ether; hence it would be more advantageous if a substance was found which would be absolutely innocuous, and which would, on addition to purest ether, stabilize it.

On the basis of the investigation related, I should like to present the following conclusions:—

(1) Purest ether has a narcotic action which in no way differs from the narcotic action of commercial narcotic ethers, which have till now been in use.

(2) Addition of impurities does not, as a rule, materially influence the narcotic action of pure ether. In those cases in which the narcotic action was increased, the addition proved to increase the toxicity of the ether to such a degree that it would be impossible to use such a mixture on man.

(3) The action of ethanesal and Cotton's ether differs in no way from the action of pure ether.

(4) Consequently the demand should be made that narcotic ether should be an ether of the highest degree of purity possible.

(5) It would be desirable to add to such an ether a substance which would prevent the formation of peroxides and thus stabilize the ether without interfering with its narcotic action.

DISCUSSION.

Dr. H. H. DALE stated that he considered that further controversy, at that meeting or in the press, on the question of the anæsthetic properties of pure ether, would be useless. The statement made by himself and his colleagues had been abundantly confirmed, by the much more thorough investigation carried out by the lecturer. Dr. Mackenzie Wallis and Dr. Hewer naturally trusted the evidence of their own observations. He could only repeat, in public, an offer, or challenge, already made privately. If Dr. Mackenzie Wallis and Dr. Hewer obtained another sample of pure ether which was not anæsthetic, he begged them to communicate with him, so that, either in his own laboratory or at St. Bartholomew's, they might arrange to make a joint observation, and endeavour to find the cause of the discrepancy. Meanwhile, it was useless to argue about a direct contradiction of experimental fact.

With regard to ethanesal he felt bound to record his opinion that the position was a most unsatisfactory one. Dr. Wallis repudiated responsibility for the product of the manufacturers, whom he had himself indicated as being charged with the preparation. Dr. Wallis reaffirmed that ethanesal ought to be a mixture of pure ether and methylethylketone, and suggested that butylalcohol had been substituted by the makers on account of its cheapness. The speaker regarded this as an extraordinary suggestion, especially as methylethylketone was not, as Dr. Wallis seemed to imagine, a costly and rare material, but a common solvent, obtainable in any quantity, and cheaper than good ether itself.

Dr. C. LANGTON HEWER said that he had neither the desire nor the ability to enter into any controversy on chemical questions, but would confine himself to the action of the pure ether submitted to him by Dr. Mackenzie Wallis.

Experiments on animals having shown that the fluid appeared to have an extremely weak anæsthetic action, he (Dr. Hewer) administered it to several children. The method used was by open mask with about twenty layers of gauze, and in no case could surgical anæsthesia be obtained, although the quantity used on each patient was up to one pint. It appeared to be impossible to pass beyond the excitement stage, although the substitution of ordinary ether produced almost immediate narcosis in each case. The fluid was next tried upon a girl of 19, using a Clover's inhaler. By keeping the indicator at "full" for six minutes (having previously taken about five minutes to rotate it from "0"), a light degree of narcosis was obtained, but on adopting the open method it was impossible to keep the patient sufficiently still for the operation to proceed, and a change over to ordinary ether was made, after which no further trouble was experienced. The remainder of the pure ether was then given to Dr. B. B. Sharp (late resident anæsthetist to St. Bartholomew's Hospital), who employed it on a child aged 7. After fifteen minutes' inhalation, the child was struggling violently, and a C.E. mixture was substituted, with immediate narcotic effect. In the foregoing experiments, the bottles were numbered, some being pure ether, and others controls of ordinary ether, and thus all bias was eliminated.

The action of Cotton process ether was next investigated by himself and Mr. Boyle, and it was also found to be too weak an anæsthetic to be satisfactory, unless the fluid was used immediately the tin had been opened. This opinion appeared to be shared by other anæsthetists who had used the preparation, and the subject was discussed at some length in the *American Journal of Surgery*, vol. xxxvi, No. 7, by Dr. Paul Cassidy and Dr. Webster. It might be remembered that Dr. Cotton himself regarded his ether as a weak anæsthetic after the dissolved ethylene had been given off.

It thus appeared to be beyond dispute, unless at least seven different observers were suffering from the same delusion, that pure ether prepared either by the process of Dr. Wallis or Dr. Cotton was at least demonstrably weaker in anæsthetic action than ordinary ether.

He concluded by expressing his pleasure at the many points which Professor Storm van Leeuwen had confirmed in the previous paper which had been read by Dr. Mackenzie Wallis and himself, e.g., the importance of small quantities of impurities in anæsthetic ether, the fact that ethanesal contained a very pure form of ether which

did not tend to decompose, the anæsthetic action of ketones, and the observation that 5 per cent. of methylethylketone dissolved in pure ether diminished the quantity necessary to produce anæsthesia.

Dr. C. F. HADFIELD, referring to the numerous complaints that had been made as to the variations in quality of different bottles of ethanesal, said he thought that the makers could hardly be blamed. He had himself found it impossible to discover the correct formula. At the meeting of the Section held on April 1, 1921, Dr. Mackenzie Wallis described it as containing certain ketones in the middle of the series,[1] whereas in the *Lancet* of March 3, 1923, these middle ketones were deserted in favour of 2 per cent. ethylmethylketone. Since that date the third edition of Boyle and Hewer's "Practical Anæsthetics" had appeared, and on page 98 it was stated once more that ethanesal contained 5 per cent. of the mixed ketones. All these statements referred not to the experimental mixtures but to the standardized "XD Brand."

Mr. H. FINNEMORE asked the author to state in detail which peroxides were referred to, of which 1 per cent. was added. The observation that the purest ether was unstable agreed with that made by the late Dr. Wade and himself some years ago. They found, however, that the addition of a small quantity of alcohol rendered such a pure ether stable. The addition of stabilizers to anæsthetics was not new, as chloroform of the British Pharmacopœia contained a small quantity of alcohol for this purpose. Some remarks had been made upon the lower stringency of the British Pharmacopœial tests for ether as compared with pharmacopœial tests of other countries, and he was glad to hear that steps were being taken by the Section of Anæsthetics to improve this state of affairs. With regard to the cost of methylethylketone, it had been stated that this was an expensive substance. As a matter of fact large quantities were available as a by-product at a very low cost.

Dr. F. E. SHIPWAY wished to draw attention to the work of Stehle and Bourne in 1922 on ether made from sodium ethylate and ethyl iodide. This ether was found to be free from all impurities and was proved to have perfect anæsthetic properties. Dr. Shipway thought that this work confirmed the work of Dale and King and the exhaustive researches which Professor Storm van Leeuwen had just described to them, and he would like to know what Dr. Dale thought of this method of preparation of ether.

Mr. H. E. G. BOYLE said that as he was not a chemist he could not pretend to discuss the chemistry of ethanesal, but with the President's permission he would like to show two slides on the screen giving the results obtained by Dr. C. Langton Hewer and himself in 10,000 cases—in all of which ethanesal had been used—either by itself or in sequence or combination with other drugs.

He had been quite satisfied with ethanesal, but at the same time if the chemists produced something better he was fully prepared to use it.

ANALYSIS OF 10,000 ADMINISTRATIONS OF ETHANESAL ANÆSTHESIA.

Method	Number
1. Clover's inhaler (with and without preliminary nitrous oxide), continuing with open mask	3,100
2. Open ethyl chloride, continuing with open ethanesal on drip-feed mask. (The majority of these patients were children)	1,200
3. Nitrous-oxide, oxygen, ethanesal with sight-feed machine and bag ...	875
4. Nitrous-oxide, oxygen, ethanesal with sight-feed machine and bag, with 50 per cent. chloroform	2,400
5. Endo-tracheal and endo-pharyngeal air and ethanesal (with electric pump)	350
6. Endo-tracheal nitrous-oxide, oxygen, ethanesal	1,525
7. Endo-pharyngeal nitrous-oxide, oxygen, ethanesal (throat cases) ...	250
8. Endo-pharyngeal nitrous-oxide, oxygen, ethanesal, with 50 per cent. chloroform (throat cases)	300
Total ...	10,000

[1] *Proceedings*, 1921, xiv, Sect. Anæsth., p. 40.

RESULTS OF ABOVE-RECORDED 10,000 CASES.

Anæsthesia satisfactory, with no immediate or remote complications ... 9,903 cases (99·03 per cent.)
Death during operation ... ... 3 (0·03 per cent.)

Method	Operation	Cause of death
(*a*) Endo-tracheal ...	Decortication of lung	Division of left vagus
(*b*) Open mask ...	Excision of new growth of liver ...	Excessive hæmorrhage
(*c*) Open mask ...	Excision of very large malignant ovarian cyst in feeble patient ...	Shock. (Patient was an extremely bad operative risk)

Pulmonary complications—	Non-fatal	Fatal
(1) Bronchitis. Mild	25	—
(2) Bronchitis. Severe	10	—
(3) Broncho-pneumonia	6	1
(4) Lobar pneumonia and collapse of lung	2	1
(5) Pleurisy	1	—
(6) Empyema	1	—
Total ...	45 (0·45 per cent.)	2 (0·02 per cent.)

Other complications (all non-fatal)	
(1) Pharyngitis	11
(2) Laryngitis (all endo-tracheal) ...	5
(3) Tracheitis	1
(4) Intractable vomiting (lasting for more than thirty-six hours)	8
(5) Conjunctivitis (the majority being in exophthalmic goitre patients) ...	10
Total ...	35 (0·35 per cent.)

Professor W. STORM VAN LEEUWEN (in reply to Mr. Finnemore) said that since he did not venture the risk of making an inexact statement he preferred to answer the question after consulting his notes at home.

Later Note.—The peroxide added was ethyl peroxide.

The Preparation and Condensation of Di-methyl Ether, and its use as an Anæsthetic Agent.

By K. B. PINSON, M.B.

OUR present anæsthetic agents are by no means perfect, we are not quick enough or safe enough. Ethyl chloride is the most rapid agent we possess, but it is not so safe as ether; we wish to influence the higher nerve centres, and we poison every cell in the body. The ideal anæsthetic should not be unpleasant to take, should be instantaneous in action, safe in the hands of experienced people, should produce unconsciousness and allow rapid recovery without after effects.

It seems proper, therefore, that other drugs than those at present in use should be tried in order to find out whether any may be more useful.

I read in Gwathmey's book on anæsthesia, that Sir B. W. Richardson had made a trial of di-methyl ether, and said: "I consider methylic ether to be the safest of all anæsthetics hitherto discovered. The one practical objection to it is that it is a permanent gas at ordinary temperatures." If it could be conveniently liquefied this objection becomes an advantage, so I set out to devise a method of making and condensing it. Of those agents which we use, it is noteworthy that the most rapid in action are ethyl chloride and nitrous oxide, both of which have boiling points below room temperature (11° C. and − 90° C., respectively) and it seems possible that this is due to their having a greater vapour tension, when dissolved in the plasma, than substances with higher boiling points.

The saturation vapour pressure of ordinary ether at 100° C., is only 97 lb. per square inch, and its boiling point is 34° C. (66° C. lower). By analogy, the pressure of di-methyl ether at 42° C. (i.e., 66° C. higher than *its* boiling point which is − 24° C.) should be at least of the same order (i.e., not ten times as great). A calculation made it 250 lb. per square inch or thereabouts at 40° C., after which it rises steeply at higher temperature. It should then be fairly easily condensible at ordinary temperatures, and I have, for want of something better to show you, brought this compressor, which, as you cannot buy these things except at fabulous prices, I had to design myself. Of course, I cannot as yet say whether the di-methyl ether will have any action on the oil, or even on the metal itself; that will be a matter for experiment, the gas itself being now little more than a chemical curiosity. You will readily agree, however, that should it substantiate such claims as Richardson was inclined to make for it, ways and means of economical manufacture would soon be devised. The compressor is driven by power, and compresses the gas in two stages by these water-cooled cylinders, and I have brought one or two vessels which would be suitable for containing the resulting liquid under pressure; a simple valve would deliver a regular and controllable flow of gas for inhalation.

The preparation will perhaps be from di-methyl sulphate, a substance used in quantity in the dyeing industry. This substance, which is very poisonous, is likely to constitute the chief impurity, but as it is liquid at ordinary temperatures, it should be easy to freeze out.

I am sorry I was unable to get any further; I managed to procure only a very small quantity of the compound, and was proceeding to test its saturation vapour pressure when the tube burst and the contents vanished. The pressure at room temperature appeared to be between 200 lb. and 300 lb. per square inch, and it has an agreeable ethereal, but quite faint odour. It condenses to a colourless mobile liquid.

Section of Anæsthetics.

President — Dr. A. L. Flemming.

The Use of Chloroform and the Misuse of Ether.

By W. Howard Jones, M.B.

Before commencing this paper I will make a few preliminary remarks and set forth the scope of this inquiry.

I am well aware that in practice we are often able to escape altogether from this problem by the employment of other agents and methods, which have now become firmly established. Nevertheless the great majority of operations are still performed under the influence of ether and chloroform administered by the ordinary method of inhalation. In addition, moreover, every registered medical practitioner is in the eyes of the law a competent anæsthetist, but only a few equip themselves either with much experience or the necessary apparatus for the more ponderous methods, from the application of which even the specialist may sometimes find it convenient to shrink.

Secondly, it must not be anticipated for a moment that I intend to advocate a general use of chloroform in preference to its much safer rival, ether. I would not waste my time and yours in pursuing such a stupid and useless argument. The subject is only approached in the sense that chloroform may still be of the greatest value in protecting our patients from the only too common evil effects of ether.

In thus taking up to a certain extent the defence of chloroform and in opening a discussion on this extremely controversial subject, I must ask to be allowed all the usual privileges of the advocate.

Lastly, you must not expect to hear from me anything whatever that is original upon such a well-worn subject, for a mere compilation of already expressed opinions, with which my own happen to coincide, is all that I have to offer. If I make use of other people's words without acknowledgment in detail it is only to avoid prolonging an already tedious paper.

It is with this outlook, therefore, that I will ask you to discuss "The Use of Chloroform and the Misuse of Ether."

The word "chloroform" seems to provoke in some people an exhibition of almost as much fanaticism, almost as much bigotry as is occasionally evidenced against the use of that poisonous but popular substance, alcohol. In both cases we have our prohibitionist who never mentions the name of his pet antipathy without indignantly exclaiming that he "never touches it." In both we have the unostentatious user, who likes to keep a little in a convenient cupboard, or in the bottle of his gas and oxygen apparatus. Both these substances are known occasionally to damage the liver, yet in each instance those are to be found who, after all, sometimes think that the risk is worth while.

The ether enthusiast is often as aggressive as the teetotaller in forcing his opinions on other people.

This controversy is as often as not originated by some person with no special experience of anæsthetic work, who makes wild statements about deaths under chloroform or anæsthetics in general, and does his best to alarm the public.

But, strange to say, the public refuses to take fright, and seems to assess these outbursts at their true value, for most reasonable people are aware that there is no operation however small which is quite devoid of all risk.

The old proverb, "Chloroform kills to-day and ether kills to-morrow," is as true now as ever it was, but there *is* an added and very sinister significance attached to the second part of the statement owing to the great increase in surgery and the employment of open ether as the routine anæsthetic.

Who will deny that hundreds are dying of post-operative bronchitis and pneumonia, not always of course entirely due to ether, for we get similar cases occasionally after spinal analgesia and after chloroform? But I think it is not possible to acquit ether of the charge in the majority of cases.

It has recently been inferred that there is a weekly "holocaust of chloroform deaths," and that if open ether were exclusively used fatalities would not occur. The first statement is a gross exaggeration and the second is not true.

I am not specially concerned here with sudden deaths under chloroform except to say in passing that they would be quite infrequent if it were not for the fact that chloroform is so often misapplied, and used in an unguarded manner. Contrary to the old belief, chloroform is especially dangerous for children, and the occasions on which it should be used as the anæsthetic of choice from start to finish in any ordinary case up to past middle life are so rare as to be almost non-existent. Nevertheless there may be occasions, not of common occurrence, when the respiratory tract is the seat of inflammation or disease, in which its application has to be considered, and in which its use may be the less of two evils and quite justifiable.

The well recognized contra-indication in cases of diabetes or starvation, where urinary changes give a danger signal, scarcely calls for much comment, but if in such a case there was a co-existing bronchitis of any importance whatever, especially in an elderly person, I incline to the belief that chloroform might be the slighter risk.

As to the statement that deaths would not occur if open ether were invariably used, it ought to be more fully recognized that deaths on the operating table associated with ether are not uncommon, and that deaths from chest complications following operations are so common as to have been a matter for concern among all observant members of the profession during the past few years. Setting aside all those cases in which the patient may be almost moribund when a forlorn hope operation is attempted, and all those in which a severe operation exhausts the strength to the vanishing point, instances do occur of death on the table of the type usually associated with chloroform.

A recent instance occurred at Charing Cross Hospital:—

A man was anæsthetized with open ether with a view to unilateral orchidectomy. The incision was made and the testicle delivered from the scrotum. Further manipulation of the testicle resulted in a cessation of respiration, and the patient died about fifteen to twenty minutes from the commencement of the administration.

Another case of a different kind, recently reported in the press, is that of a child who died during a mastoid operation—not a very severe proceeding.

I have myself had a case of acute œdema of the lungs, and others have been reported.

Again, the President of the Royal College of Surgeons in Ireland, (Sir W. I. de C. Wheeler) in a paper read before the British Medical Association,[1] threw a lurid light upon happenings under ether. He says :—

"Every surgeon knows that apart from prolonged and dangerous operative procedures, and apart from chloroform anæsthesia (which I never employ), and from such conditions as the status lymphaticus and unrecognized idiosyncrasies, there are times when a patient collapses and approaches the point of death on the table. The exact cause of the collapse is not always evident, nor is it always the same; on some occasions the anæsthetic is undoubtedly responsible."

If I might hazard a guess at the cause of this trouble, I should say that it was due to a slight obstruction acting over a long period, resulting in deficient oxygenation, with all its evil effects. Arrest of respiration and secondary heart failure can occur in these circumstances as certainly under ether as under chloroform.

Ether then is no certain road to safety either in the immediate or remote sense.

Now in all questions relating to anæsthesia, the anæsthetist ought to be in a much better position to form a correct judgment than anyone else. Not only has he a vast collection of personal experiences of success or failure behind him, but he is also a kind of central focus upon which many diverse views may be concentrated. Surgery has thrown out so many branches of practice that he is likely to associate with a number of different operators, whose requirements, opinions, technique, and preferences in minor detail, exhibit a variety which sometimes taxes the memory and even the resources of the theatre staff to the utmost. In order to meet this demand, and to attain that complete harmony and co-operation upon which so much stress has been laid, in spite perhaps of a secret desire for simplicity, he must endeavour to equip himself with many agents, much experience, great tact, plenty of confidence, and such a collection of apparatus as will cumber his, perhaps modest, house to the point of inconvenience.

He may then be not a little astonished and indeed considerably vexed, when he is informed by some person with no particular experience in the administration of anæsthetics, that the whole art is so simple and so safe that even an unqualified layman can easily acquire proficiency in it, provided that he confines himself to the administration of open ether, and that all the apparatus that may be necessary is a "Rendel's inhaler or even a tumbler, a sponge and a towel." This latter apparatus, so suggestive of after dinner conjuring, seems but ill-suited to many cases of which I still retain a lively recollection.

Nor must he be too ready to accept without reserve the pronouncements of individual surgeons or anæsthetists however distinguished, for some are inclined to indulge in rather too much idealism. It has been said that the steady concentration of the mind upon ideals is likely to induce a kind of self-hypnosis in which visions of perfect performances readily arise.

There is no person in so favourable a position to appreciate delicate surgery as the anæsthetist. Great advances have been made, but we must not be carried away by theories of shockless operations performed under non-toxic anæsthetics, or by stories from foreign countries of delicate manipulations successfully carried out under light anæsthesias.

[1] *Brit. Med. Journ.*, 1923, li, p. 793.

Surgeons sometimes give each other away. Farr, for instance, in his "Local Anæsthesia" is most indiscreet. He says:—

"Many times has the author seen in the best and largest clinics in the country, the surgeon actually fighting the loops of bowel as they insisted on protruding from the abdominal wound during general anæsthesia, until the gut became congested and even bled."

He goes on to say, somewhat unnecessarily:—

"This is due to the positive intra-abdominal tension which sometimes exists."

It does.

The anæsthetist who can always secure relaxation of the abdomen with a light ether anæsthesia, must necessarily work with the surgeon who can handle all its contents without producing any reaction. The one may truly be said to be the counterpart of the other, but whether it could be said with equal truth that the one is complementary to the other will depend upon the choice of a vowel.

Can it be doubted that what we are required to do on the average difficult abdominal occasion, is to get the patient well into surgical anæsthesia and maintain it until the difficult passage is over? It is these deeper grades of ether anæsthesia which are likely to lead to trouble if the need for them is prolonged.

It will be convenient first to consider how ether may be misused and then to remind you how chloroform may come to the rescue.

In the first place ether is often used in totally unsuitable cases. Surely it cannot be denied that it is dangerous in old people, especially if they are asthmatical or bronchial or the operation is likely to be a long one.

The age limit at which it may be dangerous is elastic—each case must be judged on its merits. I have known also of cases of tuberculosis of the lungs which have run a rapid course after its administration. In fact any known infection of the bronchi or lungs has long been considered a contra-indication—a truth to which every text-book will bear witness. When chloroform is avoided in these cases it is generally on account of prejudice on the part of the surgeon or fear and apprehension on the part of the anæsthetist, who may not be practised in its application.

How do the surgeons who never use chloroform deal with some of these cases? It is characteristic of their arguments in favour of the exclusive use of ether that they are silent upon this point. They never dwell upon its special suitability for a bronchitic old man of 65 or 70 who is suffering from some acute abdominal condition. Nor do they praise the soothing after-effects which it has on a fat asthmatical lady whose breast has been removed under its influence.

Secondly, it is sometimes given in too great a dosage and over much too long a period of time. About light anæsthesias such as recommended by Dr. Dickinson Berry for thyroid operations and other suitable cases there is nothing to be said except that they are admirable. But the amount or degree of anæsthesia required depends entirely on the site and nature of the operation, and when we deal with abdominal work we are bound to go further.

In the endeavour to meet the requirements of the surgeon ether is liberally applied to the mask, and the patient sinks deeper and deeper under its influence. A too intense effect may now easily pass unnoticed, and inattentive and inexperienced people sometimes allow a slight depression of the jaw and tongue, introducing an element of partial obstruction resulting in

deficient oxygenation of the blood. The respiration becomes jerky and irregular, indicating poisoning of the respiratory centre, and there is a venous ooze from the wound. As previously stated, in these circumstances arrest of respiration and secondary heart failure can occur as certainly under ether as under chloroform. It is not sufficiently recognized that after having obtained surgical anæsthesia and relaxation, much more ether may be given before the pupil dilates or glaring signs of overdose appear. Nor is it possible to exaggerate the disastrous effects which a slight deprivation of oxygen must have when continued over a long period either in the operating theatre or afterwards in the ward. In serious cases there can be no doubt that these factors will turn the scales against the patient.

In order to avoid these defects the anæsthetist must be constantly endeavouring to find the least possible degree of anæsthesia which is consistent with the surgeon's comfort, and a nurse should always be in attendance on the recovering patient until all danger of partial obstruction has passed off.

Surely one of the most important factors which determines success or failure is the time taken over an operation. What a difference it must make to the patient if he is one and not two hours on the table. It seems likely that there is hardly a set operation in surgery, however big, which cannot be performed by some person who has devoted special attention to it within or round about one hour.

We have very definite evidence that short administrations of ether, even in apparently risky patients, are not specially prone to be followed by trouble. Patients at rectal hospitals, who are often big, muscular, chesty emphysematous alcoholics, are regularly dealt with by the gas-ether sequence in Clover's inhaler, for short operations lasting ten to twenty minutes. Yet it is a striking fact that respiratory troubles are extremely rare. But in long operations, especially where the site of the operation is in the belly, evidence points the other way. It cannot be denied, and I believe that hospital physicians and general practitioners would testify to the fact that post-operative bronchitis and pneumonia have become far too common, and the upper abdominal operation a positive bugbear to all concerned.

Ether given by the open method even in moderate dosage and with a perfect airway does not always produce an entirely satisfactory picture. A quick bounding pulse, dilated vessels, rapid deep respirations, a moist and often sweating skin considerably exposed to the cooling effects of evaporation, seem very unnatural accompaniments to a serious and exhausting operation. The muscular effort involved in this exaggerated breathing must in itself contribute much in the way of fatigue, and the increased amount of blood in the skin, the deep and rapid breathing and the evaporation of sweat constitute the natural physiological mechanism of heat loss. Moreover, the dilated condition of the small vessels, as often shown by the rash on the skin and free oozing from the wound, seem likely to favour that disappearance of fluid out of the circulation into the tissues which physiologists inform us is the chief accompaniment of shock. After an hour or more, even in cases not specially liable to shock, the skin tends rather to pallor, except for a slight pinkness about the ears and lips, the eyelids half open, and the globe looking a little sunken in its socket.

If, then, chill has any influence predisposing to shock and respiratory complications, here we have every known condition favourable to its onset, and in order to counteract this tendency, the theatre must be kept at such a temperature as will cause the surgeon to sweat as much as the patient. The unfortunate

sufferer, still clad in his damp coverings, may now be taken through cold and draughty corridors to his bed in an almost equally cold ward. The large wards of our hospitals are very unsuitable for the reception of seriously shocked patients, and the day is near when these cases will be dealt with in smaller recovery rooms where the temperature can be more easily regulated.

There is much, then, in the open ether patient that is so suggestive of shock itself, that it seems likely to be an extremely doubtful proceeding in serious cases. F. C. Mann, an American writer, states that deep etherization may produce most of the symptoms of shock.

With regard to the so-called upper abdominal operation, everybody agrees that post-operative respiratory complications are extremely common after these operations, and that certain mechanical deficiencies leading to imperfect ventilation of the lungs are strong predisposing factors. Some go so far as to say that the anæsthetic is seldom responsible and that whatever agent is used the result is likely to be the same, This is an excellent theory and ought to be supported by every anæsthetist who has any regard for his reputation. All surgeons, however, are not educated up to this climax, and instead of focussing their attention on the mechanical deficiencies of the patient are apt to make irrelevant remarks about the irritating qualities of ether, and to condemn the pharmacological and mental deficiencies of the anæsthetist. They are likely to be right, for in the majority of cases the prolonged administration of ether is probably the cause of the trouble.

Farr, in his book on local anæsthesia, says:—

"Every prolonged ether narcosis is followed by small pneumonic foci in the lungs, with mucus and extravasated blood-cells in the alveoli and some round-cell infiltration. This may occur after a short operation in the predisposed or susceptible. Thus is laid the foundation of a subsequent pulmonary œdema, or a postoperative bronchitis or pneumonia."

These operations, especially those on the stomach, have of late years become extremely common, and scarcely any long hospital list is without its gastro-enterostomy.

Now, it is unfortunate that quite a large proportion of these cases occur in people past middle life whose chests are not above suspicion. Indeed, it is not uncommon to meet with those in which the chest is of the barrel-shaped order usually associated with emphysema and scarcely moves on respiration. These are the cases which exemplify better than any other the misuse or the unwise unrestricted use of open ether. Another doubtful practice in these cases is the preliminary medication with heavy doses of narcotic drugs, which can but tend to increase any deficiency in lung ventilation and to decrease the rate of ether elimination during the recovery period,

Thirdly, I believe that the so-called closed method often gives better results and entails less risk than the open. We can all remember how, when open ether first came into fashion, we were soon told by superior people that Clover's inhaler was a septic and a horrible instrument, and that it was criminal to allow any patient to re-breathe into a bag. It is an almost pathetic reflection that some of those who were formerly most prominent in the condemnation of the rubber bag may now be equally prominent in the assiduous use of it in the latest method, fortified no doubt in their own minds by the Roman doctrine that the end justifies the means. But Clover's inhaler can be perfectly sterilized by boiling as easily as can a pair of Spencer-Wells forceps or a pair of rubber gloves.

Rightly used this method can produce an anæsthesia equal in quality to

that of the open, and at the end of two hours will render up a patient in much better condition than the open mask and with less tendency to chest trouble. There must be less heat loss through respiration; there is certainly less sweating, the pulse does not get so quick, the face keeps a better colour and is warmer to the touch, and return to consciousness is more rapid.

There is, however, a good deal of misunderstanding about this method, and it is associated in the minds of some with a black, bloated and semi-asphyxiated patient making horrible noises and frothing at the mouth. The fault lies not in the method but in the administrator, who requires to have a large amount of experience before he acquires proficiency in its use.

The re-breathing can be reduced to a minimum; the colour can be as pink as under any other method, the respirations of only moderate excursion, while the strength of vapour is easily regulated and can be kept constant. There is, moreover, the great advantage of being able to commence with gas and oxygen, and the ease with which oxygen can be given whenever it is desirable.

Ether, then, although it must always be the anæsthetic of choice for induction purposes and for short operations, unless specially contra-indicated, will often lead us into trouble unless we recognize its limitations. The slovenly use of it by inexperienced people who take shelter behind its immediate safety is fraught with as much danger as is to be found in the use of chloroform itself.

Now when we come to consider how chloroform may sometimes be beneficially used to save the patient from the evil effects of ether, my task has been considerably lightened, for a distinguished operator has recently made a bold pronouncement. Mr. Trotter, of University College Hospital, in a most interesting address read before the British Medical Association last year,[1] has already told us that he prefers chloroform, in the aged, in head, jaw and tongue cases, in operations on the larynx and in breast cases, a somewhat formidable list, containing some types in which many of us would be content to administer intratracheal ether. He notices the comparative safety of chloroform when given through a laryngotomy or tracheotomy tube, which is only another way of saying that 999 times in 1,000 difficulties and dangers with chloroform are due to an unrecognized obstruction to respiration. He also notices its quieting effect on the circulation and the smaller amount of bleeding resulting when the area of the cut surface is large. He speaks of the "artificial and exhausting floridity" associated with ether and says that in "some cases it may be the decisive factor against success."

Could anything worse be said about chloroform?

In referring to the breast operation he says:—

> "Moreover the same effects of ether, while they add to the shock disguise its onset; it is because choroform so precisely avoids all these disadvantages that it seems decidedly to be preferred in this operation."

But if in this operation why not in others? Why not engage these good qualities of chloroform to save the patient from the prolonged or mischievous use of ether in any case? It is in this direction that I would remind you of that ancient proceeding, the gas-ether-chloroform sequence.

It has long been known that the risk to the patient of inhaling chloroform after having been thoroughly anæsthetized with ether is extremely small. All the difficulties and dangers of induction are avoided and very small dosage of chloroform is required to maintain anæsthesia.

[1] *Brit. Med. Journ.*, 1923, ii, p. 791.

In addition there are few people who will not stand a short administration of ether. Let me describe a case:

The patient passes easily and comfortably from gas and oxygen into ether anæsthesia which must attain full relaxation before the surgeon commences.

The belly is then opened and fully explored while the patient is under deep ether anæsthesia, the abdominal reflexes not being in evidence if the proper level has been reached.

A certain procedure having been decided upon, it may be that the exaggerated type of ether breathing is now a hindrance to the surgeon. The Clover's inhaler is laid aside and chloroform is applied. The respirations will now tend to decrease both in rate and amplitude for well-known physiological reasons and also on account of the gradual introduction of chloroform in place of the more stimulating ether, until a condition is reached which may be called ideal for abdominal surgery. The colour is perfect, the skin of a natural dryness, the pulse approaches the normal and the respirations may be almost imperceptible to the casual observer. The patient seems to be making no effort and to be in a condition resembling natural sleep. It is at this juncture that the doctor who is assisting often inquires if the patient is all right and the surgeon, if unaccustomed to chloroform, sometimes causes a violent expiration by suddenly compressing the thorax.

In order to give confidence to students in the matter of this shallow but perfect breathing I am accustomed to advise them that when quietly seated in an arm-chair they should, like the monks of Mount Athos, contemplate on the movements of the umbilical region. They will then appreciate the meaning of natural breathing and will not be alarmed when they see it on the operating table. Proceeding with our case, chloroform is continued in decreasing dose until towards the end of the operation, when, preparatory to sewing up, the anæsthesia is deepened either by increasing the amount of chloroform or by again resorting to ether. By this method the patient has all the advantages of both drugs and none of the disadvantages of either. He has a safe induction to an effective depth of anæsthesia but is spared the prolonged administration of ether. All the rough work is done under ether and the more delicate under chloroform. The patient is put to much less effort, loses less heat, finishes with a better pulse and is not nearly so liable to chest complications afterwards.

Whenever possible it is of the greatest advantage to the patient, the surgeon and the anæsthetist if all exploration is done at the outset under the deep ether anæsthesia. When this precaution is neglected, and an after-thought examination of the stomach and gall-bladder is made just before sewing up the whole picture is spoiled, much valuable time is lost and a needless amount of extra anæsthetic has to be given before the operation can be completed.

Now if it be granted that sometimes the use of chloroform is advisable and justifiable in certain circumstances, if it is allowable in mixtures however dilute, if it is of service in conjunction with gas and oxygen, if it is to be a constituent of "warm ether," if it is to be used in midwifery, we must be consistent, and see that students have an opportunity of learning and practising its application.

The man who on ninety-nine occasions gives open ether and on the hundredth gives chloroform under protest is a danger to his patient, he does not like it, he may be afraid of it, at any rate he is not practised in its use.

The administration of chloroform requires constant practice, an attention so trained that the quality of each single breath is noted, for the almost

automatic correction of defective conditions, long before grave circumstances arise, is essential to success.

The late Sir Frederic Hewitt stated that after a month's holiday he had not his usual confidence in giving chloroform. Moreover students lose much by not being taught how to use chloroform. It exemplifies in its application, in the highest degree every important principle in anæsthesia; the complete freedom of respiration, accuracy of dosage and the necessary watchfulness required, teach the student his duty to the patient more thoroughly than in any other method. It should be the basis of all teaching, for those who understand chloroform will have little difficulty with other agents, but the converse is not true.

A very evil tendency has become noticeable in late years arising out of the use of very dilute mixtures given on the semi-closed mask, or the application of chloroform to one already saturated with ether. This class of mixture has been called "mitigated" ether, a name which is in itself a confession of the imperfection of ether, and I cannot help thinking that this method is a serious violation of one of the first principles of chloroform administration—perfect freedom of air supply. It may be carried out successfully by the expert but is undoubtedly often associated with a slight duskiness in the patient's colour. In the minds of the younger generation it breeds the unfortunate belief that mixtures containing chloroform of various strengths can be so given with safety and I have frequently caught students and house officers attempting to administer C.E. mixture on the open ether mask with considerable air limitation. This is a very dangerous practice and cannot be too strongly condemned. They should be taught that the use of any mixture containing chloroform must be accompanied by all the care for, and freedom of, air supply which is demanded in the use of the pure drug.

Another curious aberration of the open ether practitioner is the use of pure chloroform as an induction preparatory to ether. Having regard to what we know about chloroform fatalities this surely requires explanation.

There is nothing I should resent more than being called either an etherist or a chloroformist, for either label would carry conviction to many that I was only half an anæsthetist and something of a crank. The true solution of this problem lies in the judicious use of both drugs, and the truly skilled anæsthetist is he who can apply them appropriately to the varying needs of his patient, changing from one to the other as occasion demands.

In conclusion, I would plead with the ether enthusiasts to devote their energies to securing reform in the matter of teaching, which would ensure a better understanding of the application of anæsthetic agents in general and the right use of chloroform in particular.

DISCUSSION.

Dr. MENNELL said that Dr. Howard Jones was fortunate in the choice of the title to his paper, which invited discussion. He had also dealt skilfully with many debateable points. For the sake of discussion, although he held no brief for ether, he (Dr. Mennell) would adopt the attitude that whatever could be done with chloroform could be done as well with ether, and with greater safety. He reported two deaths which had taken place during the last eighteen months, and which had been reported to the sub-committee at St. Thomas's, both in apparently strong, healthy men, one under chloroform and the other under ether. The former occurred before the operation was begun, and the latter fifty minutes after the commencement of the operation. No macroscopic evidences of disease could be found in either case, post mortem, and yet there were gross microscopic changes in the heart and liver. The chloroform death took place immediately, and as

the operation was a very small one he thought it a fair conclusion that the man would not have died had ether been used. If anæsthetists were in the future to avoid deaths due to anæsthetics, the idea that certain people were susceptible to chloroform must be put on one side, and they must try to discover clinical evidence of the diseased cardiac muscle before the anæsthetic was administered. It would be impossible to test the basal metabolism of every patient before operation, and even could this be done there was no evidence at present to prove it a reliable guide in these conditions. Both the men mentioned above had been in active work up to the time of operation. He also mentioned two other chloroform deaths in somewhat similar circumstances. As regards the chest complications so commonly associated with ether as a disadvantage, he agreed with another speaker that the extended use of large doses of atropine without morphia greatly diminished this risk, and, although he used open ether and pushed it to deep anæsthesia, he could not discover an undue proportion of cases of pneumonia following ether, as compared with what resulted under other methods. Deaths occurred from chest complications after chloroform, spinal and local anæsthetics. Regarding relaxation of the upper abdomen, he considered this was best obtained by pushing ether and avoiding preliminary narcotics. Had members of the Section ever watched the behaviour of the vocal cords under ether? It was very instructive to do so. The abductors were paralysed first, leaving the adductors unopposed, and then with more anæsthetic the adductors became paralysed and the cords became flaccid and could be seen to lose their tense white appearance. In this way a perfectly free airway was obtained and the respirations became quiet, short and shallow, and at the same time the bright pink colour of a deep ether anæsthesia occurred with flaccid abdominal muscles. This condition he considered safe with ether and unsafe with chloroform or any mixture containing chloroform. Chloroform was not used in America, even in sub-tropical or tropical climates.

Dr. FEATHERSTONE (Birmingham) recorded a case of ether anæsthesia which might have fared better under chloroform. A well-known neurologist recommended exploration of the left frontal lobe for a rapidly growing tumour, causing excessive intracranial pressure and mental degeneration. The patient, a frail little lady of 40 years of age, was readily anæsthetized with open ether, and an intratracheal catheter was passed without any difficulty. Air and ether was blown into the trachea, the patient gave a short cough, and respiratory movements abruptly ceased, never to return. The heart-beat, which had stopped, was restored by direct cardiac massage, and intratracheal insufflation of air maintained life for nearly three hours, but there was no voluntary respiratory movement. The physician who was present considered that the slight additional rise in intracranial pressure produced by ether and the cough after intubation, injured the respiratory centre irrecoverably. Unfortunately, cerebral decompression was not performed. Dr. Featherstone said that possibly the administration of chloroform and the consequent lowering of blood-pressure might have led to a happier result.

Mr. ASHLEY DALY said that in his description of the condition of a patient under ether many of the symptoms described by Dr. Howard Jones were due to faults in technique. There was no need to have a patient dusky, sweating and breathing irregularly. By the use of oxygen a full dose of atropine without morphia, an artificial airway, and the maintenance of a deep anæsthesia throughout, an anæsthesia as quiet as that of chloroform could be obtained, and one much less likely to be upset by a sudden surgical stimulus.

With regard to high abdominal operations, Dr. Howard Jones said it was his practice to keep his patient deeply under ether during the exploratory part of the operation when much manipulation was going on, and to change to chloroform during the actual operative procedure. But in many upper abdominal operations, such as gastrectomy and operations on the bile-ducts, strong surgical stimuli were being applied throughout the operation.

He (Mr. Daly) maintained that it was a better practice to put the patient deeply under ether before operation was begun, and to maintain a deep anæsthesia till the parietal peritoneum was closed. If this practice was adopted the abdominal wall was

well relaxed throughout, the surgeon was able to do his work more quickly, and with the minimum amount of pulling and retraction, to the great advantage of the patient.

Dr. HOWARD JONES (in reply) pointed out that one of the cases described by Dr. Mennell—that of an elderly man who was to have had a wart removed from his lip in the out-patient department of a hospital, and who died during the induction of chloroform anæsthesia—was after all only a classical example of the misuse of chloroform, and no argument against its proper application. No case which was worthy of the administration of chloroform should ever be dealt with in the out-patient department. Dr. Daly's contention that many of the troubles under ether described in the paper were due to faults in technique was quite true. The object of the paper was to emphasize the disastrous consequences of such errors. He (Dr. Howard Jones) could not agree that it was always necessary to maintain a deep anæsthesia throughout a long operation. The patient should be spared as much anæsthetic as was possible, and the anæsthesia regulated to the requirements of the operation.

Section of Anæsthetics.

President—Dr. A. L. Flemming.

Recent Experiences of Anæsthetics in America.

By Z. Mennell, M.B.Lond.

[ABSTRACT.]

Last summer I paid a visit to America to learn how medical work was done in the United States, and more particularly to study the newer methods of anæsthesia practised there. I met with wonderful hospitality, travelled some 14,000 miles, and visited twelve big towns and over thirty hospitals. Without going into details of the work I propose to give as fairly as I can the general impression left upon my mind.

Status of the Anæsthetist.

The Americans are undoubtedly very able business men, and they seem to me to have made a business of the medical profession. There is little of the personal element which we know here. I cannot make up my mind whether this is desirable or not. The patient becomes a case, his wishes are not considered, and he is entirely in the hands of the particular surgeon he consults. Naturally, as in every business concern, the best business man gets into control, but it does not follow he is the best doctor. Again, from a business point of view surgery stands supreme, and the head of the clinic is always a surgeon who makes a final diagnosis, in many clinics pays a salary to other people as assistants, and is intolerant of any suggestion or alteration in his particular methods. One of these paid assistants in a very subordinate position is the anæsthetist. He (or she, as the case may be) has no will of his own and is not expected to know anything or take any responsibility, and all he is expected to be able to do is to give a light ether anæsthetic after preliminary medication, or gas and oxygen with a fool-proof machine. Hence the reason for the nurse-anæsthetist, as seen all over the States, who answers the purpose very well as long as the surgeon is content to put up with a light ether anæsthesia with all the consequent discomforts. When not so content he will practise local anæsthesia, and this has been developed in a wonderful manner, but is not used where the anæsthetics are good. Such conditions do not attract good men to take up anæsthetics, and though a better understanding is gradually arising, the anæsthetists have a long way to go.

Let us consider how the anæsthetists themselves are dealing with the situation and whether they are proceeding along the right lines. In all good fellowship I would say, let them show the surgeons what a good straight ether anæsthetic can be. It is not necessary to produce some new stunt which the nurse-anæsthetist cannot do; let them proceed along simple lines. The whole time I was in America I only saw two good straight ether anæsthetics, though I saw excellent anæsthetics given by other means. I would suggest to them that chloroform has its uses. I never understood its real value until it was brought to my mind by the complete absence of its use.

The Chicago Congress.

At the Congress in Chicago in October I met Dr. F. M. McMechan, secretary-general of the Associated Societies of Anæsthetists, whose whole life and interest is devoted to the improvement of anæsthetics and the status of the anæsthetist. I have rarely heard anyone speak as he can—clearly, concisely, and with obvious sincerity. He is a genius with a mission, who has collected, and keeps in touch with, all the best anæsthetists and physiologists in America. At the Congress there were representatives from all over the States, many of whom had travelled thousands of miles to be present. McMechan knew them all personally, their capabilities and their special branch of work. A great variety of research is going on. During the three days thirty-four papers were read, followed by discussion which was always animated. The discussion on my own paper narrowed down to the *pros* and *cons* of intratracheal ether, which is not extensively used in the States, and for the same reason, I think, as here—namely, the difficulty many people find in passing the catheter.

In Chicago I saw the new anæsthetic, ethylene, given to four patients; quite a satisfactory anæsthesia was produced, but the smell was appalling. It is given with oxygen, the supply being drawn from tanks, which stand about six feet high, each containing some 3,400 gallons and attached to McKesson's apparatus. Narcosis is produced rapidly without struggling or cyanosis; I should place it between nitrous oxide and ether. Unless something can be done to minimize the objectionable smell I do not think it will gain favour in this country; but some of this odour is said to be due to impurities. At least one death has been reported. Professor Hugh McGuigan, of Illinois University, showed me a striking series of experiments illustrating the action of chloroform on the hearts of dogs. This was the only time I saw or smelt chloroform while I was away.

Two Outstanding Anæsthetists.

Dr. E. I. McKesson, of Toledo (Ohio), is a great anæsthetist who uses gas and oxygen for everything: he practises secondary saturation, but admits that even then he cannot obtain a lax abdominal wall. His use of gas and oxygen in maternity work with portable apparatus appealed to me; there is, without doubt, an opening for it here. The uterine contraction is allowed to begin before the gas is inhaled, and analgesia is still produced rapidly enough to counteract the labour pain. It is claimed—and I saw nothing to make me doubt it—that uterine contraction is actually stimulated by the gas. McKesson and others informed me that self-administration was in common use when the coöperation of the mother could be secured. The first special sense to surrender, it appears, is the sense of pain, leaving the other special senses less influenced. I was impressed with the vigorous physical and mental post-partum condition of these patients. They were unanimous about the relief of pain and sense of well-being both during and after labour. I had been told of the efficiency of this analgesia, but had hitherto regarded the story as an exaggeration: I was completely converted by what I saw and heard. The technique needs a high degree of skill and much practice to perfect. In America the majority of confinements take place in hospital, and it is possible to keep the rather cumbersome apparatus ready in the labour rooms. I also saw enucleation of tonsils done under prolonged gas and oxygen anæsthesia. Dr. McKesson has certainly done more than anyone for this form of anæsthesia, and as a rule there was one of his machines in every hospital theatre I visited. He has a large manufactory for these machines at the back of his house, and sells them from there all over the world. I saw gas and oxygen used elsewhere, but not to the same extent nor with the same excellent results.

At Montreal I stayed with Dr. Wesley Bourne, who is, without exception, the best anæsthetist I have ever seen at work. I was with him for two days, and went round with him to see his work both in hospital and in private practice: there is very little difference, as both are done in the same buildings. He does not confine himself to any one anæsthetic, and I was pleased to see him with an old Clover inhaler. Intratracheal insufflation he uses as a matter of routine; Kelly's apparatus is ready for use in the theatres in which he works. He can get excellent results with nasal gas and oxygen, and does all his dental work in this way. He is *au fait* with the importance of a clear air-way and knows how to get a slack abdomen. Dr. Bourne's connexion with McGill University is as a lecturer on pharmacology; he refused the post of anæsthetist because of the subordinate position attached to that appointment.

Average Anæsthetic Practice.

To many hospitals I visited I went unannounced, without disclosing that I was an anæsthetist. In this way I saw at a glance what was going on in the anæsthetic line. In many instances I saw a perfectly smooth, light anæsthesia given by the nurse-anæsthetist, with the surgeon contentedly sewing up stiff abdomens and dealing with protruding intestines, apparently not expecting anything else. Local anæsthesia was much used by the surgeons in minor cases. I had expected to see a more extended use in abdominal work. What I saw I can hardly think to be was representative. The patients were well doped with preliminary narcotics, but even then were restless. Graves' disease and goitre seemed to be the most satisfactory type of case. The only intracranial operation I saw was one for cerebellar decompression; the operation took three and a half hours, and was done with a local anæsthetic. Under these circumstances no other method could have been used with safety, and the condition at the end of the operation was almost as good as at the beginning. The dura was not opened. At the same clinic I saw two patients being dressed, one of whom had an infection under the scalp, and the second sloughing of the edges of the wound; as both of these cases had had a local anæsthetic I could not dissociate cause and effect. The local agent used is practically always procaine, which is the same drug as our novocaine; the chemists are making innumerable other synthetic bodies experimentally. Cocaine is not used at all, and I heard a lecture on its many isomeric varieties which explained the varying toxicity of this drug. Stovaine is not used. I heard it stated that this drug could not be washed out of nervous tissue after introduction. Spinal anæsthesia is under a cloud, chiefly, I think, on account of the late after-effects. I could neither see nor hear of any splanchnic or regional anæsthetic work. I saw one surgeon working with a combination of novocaine and gas-oxygen anæsthesia with the most complete success while performing a difficult abdominal operation. He obtained apparently excellent relaxation by this means. The anæsthetist was completely under his control, moving the indicator on the machine according to directions. This demonstration was a pleasure to watch, and the surgeon was an expert, not only at his own job, but as an anæsthetist. He realized the extreme importance of gentleness in handling the parietal peritoneum, and during the whole operation never made a single rough or unnecessary movement. There was a supply of ether in the bottle attached to the machine, but it was not used.

I saw very few students giving anæsthetics under tuition, and these were chiefly in Canada. The so-called interns, corresponding to our house officers, do comparatively very little anæsthetic work. There is, in fact, not the same reason for them to do so, as all surgery is done in hospital to which there is attached a team of full-time salaried assistants.

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Section of Balneology and Climatology.

President—Dr. CHARLES W. BUCKLEY.

On the Physiological Action of the Dry-Inhalation of Sodium Chloride: An Italian Method of Treatment.

By WILLIAM J. SMITH JEROME, M.A.Oxon., M.B.Lond.

THE method of dry inhalation was introduced for the treatment of those suffering from affections of the respiratory tract only; but in 1908-09 Peri tried its effects on 106 patients, whose ailments differed widely in nature, and the results were such as to convince him that, contrary to what was previously believed, they were due to something more than mere local action of the drug on the mucous membrane. He suggested, therefore, that I should investigate the subject further; and, as the results of his work pointed to a modification of the metabolism, I decided to study this in growing guinea-pigs while undergoing treatment by dry inhalation of sodium chloride. It is true that these animals, in consequence of the fluctuations in weight caused by the bulk and slow passage of the fæces, are admittedly not the best animals for experiments on metabolism; but they are almost everywhere readily procurable, and the objection mentioned can be circumvented by making each experiment of considerable duration.

The apparatus employed is due to a German engineer named Körting, and the first establishment for its use was opened in 1907 at Bad Kösen. In 1914 when the experiments described below were completed, there were twelve such institutions in Italy, two in Germany, and several in Russia.

Körting's aim was to produce a spray, the dry particles of which would when inhaled reach the alveoli. For such penetration by a moist spray Emmerich[1] postulates two conditions, viz.: that the droplets shall be in a state of extremely minute division, and that they shall be present in a high degree of concentration. Bonetti[2] has shown that with Körting's apparatus this is achieved also by the dry spray; for after letting guinea-pigs inhale from a solution of methylene blue 0·5 per cent., and sodium carbonate 14·5 per cent., he detected with the microscope particles of methylene blue in the sub-epithelial and interalveolar tissue of the lungs.

Peri[3] has described the mode of production of the spray as follows: Fresh air brought from outside by an aspirating ventilator receives, after being heated, the spray from the medicated solution. This spray, produced by a current of air compressed by an electric motor, passes through one of two

[1] *Münch. med. Woch.*, 1902, xlix, p. 1610.
[2] *Com. al Cong. del Soc. Ital. di Laring., di Otol., e Rinol.*, Roma, 1911.
[3] *Archiv. Ital. di Otol. e Laring.*, 1909, xx, Fasc. 4.

nozzles in the spray-producer; is invested by the current of hot air issuing from the other nozzle; and has its water removed by this air. The spray-producers are single or multiple according to the size of the chamber. The air thus prepared and carrying the dry particles in suspension, is carried under the influence of the ventilator into the inhaling chamber, and distributed uniformly there by suitably placed openings, while other openings remove the vitiated air. The quantity of medicated solution used in a given time is regulated by raising or lowering the receptacle containing it, and thereby increasing or lessening the height of the column of liquid which is being aspirated; or by the number of spray-producers employed. The good ventilation permits a long stay in the chamber, and the warm air introduced makes further heating of it unnecessary (see figs. 1, 2).

Although when the process is efficiently carried out the moisture of the air in the chamber is not appreciably increased, the atmosphere in a few minutes becomes so clouded as to make objects invisible at the distance of a yard. There is, however, no discomfort produced, either through cough or from irritation of the conjunctiva, even when the solution of sodium chloride is saturated, and the stay in the chamber prolonged. When the machinery ceases to act the cloud in the room subsides very slowly, and deposits after ninety minutes the thinnest of visible films on any dark objects present.

The solutions used up to the date of my experiments were: Saturated solution of sodium chloride, saturated solution of sodium benzoate, 15 to 20 per cent. solution of salts contained in Bad Kösen water, natural Salzomaggiore water, sea-water, sea-water with 5 per cent. iodine, and sea-water saturated with Castrocara salts.

The guinea-pigs used were young males, with little difference in the colour markings, and with one exception each pair came from the same litter. Their small zinc cages were of the same form and capacity, each having a double floor, the upper part of which, of wire-netting, supported the animal, and was removable for purposes of cleansing, &c. Wire-netting with a wider mesh covered the top of each. To equalize as much as possible any effect of insolation on the metabolism, the cages were put side by side and their places changed every second or third day. When the fæces were removed the animals were transferred to duplicate cages, and a special one was used for transference to and from the inhalatorium. The duration of each sitting was ninety minutes, and the food, given at fixed periods during the day, varied both in quantity and quality in the several experiments. It consisted of bread, potato, cabbage, lettuce, and in one case a little boiled milk. To make that eaten by the two animals as much alike as possible every cabbage and lettuce leaf was divided along the mid-rib and one half given to each of the guinea-pigs under observation at the time. The bread, after being dried at a fixed temperature, was weighed, mixed with some water, and then placed in a receptacle so constructed that the food could not be pushed over the edge. Any residue after twenty-four hours was dried at the same temperature as before, and then, together with any uneaten cabbage, &c., weighed to ascertain the amount eaten. While one animal was inhaling, all food was removed from the cage of the other. In Experiment I the guinea-pigs were weighed in the evening only, in the others both morning and evening, the mean of the two weighings being taken.

Five experiments were made of 67, 40, 102, 96, and 90 days respectively. In No. I the diet was good in kind and unrestricted in amount; in No. II scanty and unsuitable during an experiment of short duration; and in No. III imperfect during a longer period.

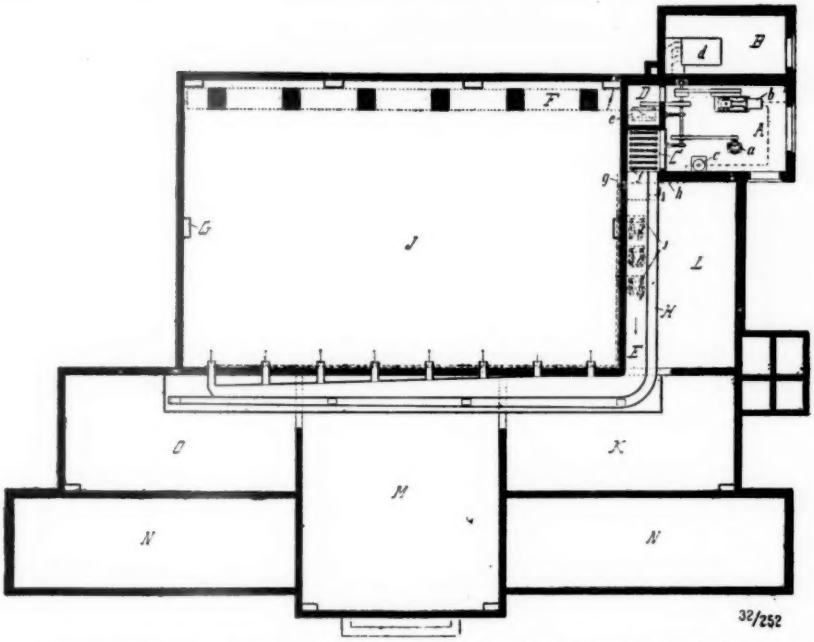


FIG. 1.—*Ground-plan of Kösen Inhalatorium.*—*A*, local machine; *B*, local furnace for heating; *C*, radiator; *D*, chamber for ventilator; *E*, pipes for fresh air; *F*, pipes for circulating the air; *G*, pipes for exit of vitiated air; *H*, pipes for heating of the building; *I*, large inhaling chamber; *K*, small inhaling chamber; *L*, cloak-room; *M*, reading-room; *N*, verandah; *a*, electric motor; *b*, air compressor; *c*, ventilators; *f*, heating apparatus; *g*, for transport of the solution; *h*, for transport of compressed air; *i*, nozzles of spraying apparatus.

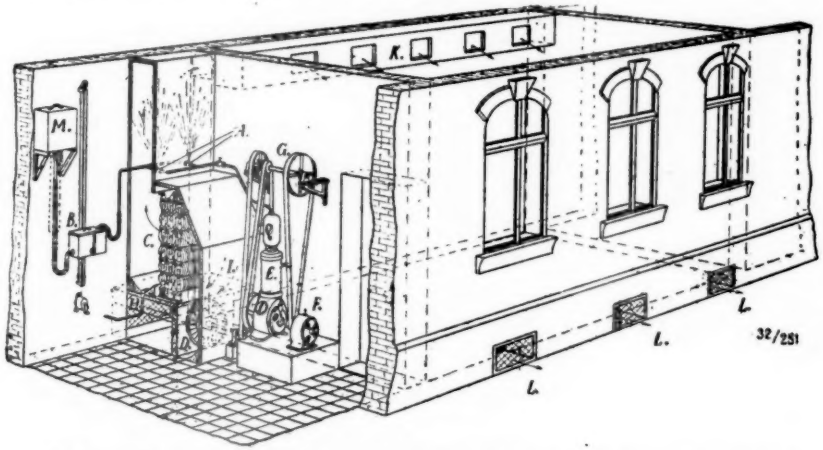


FIG. 2.—*A*, spray apparatus; *B*, regulator of amount for spray; *C*, heating apparatus; *D*, ventilators; *E*, air compressor; *F*, electric motor; *G*, fly-wheel; *H*, entry for fresh air; *I*, opening for transmission of air; *K*, place for distribution of air; *L*, exits for vitiated air; *M*, reservoir for medicated solution.

In No. IV a vain effort was made to see if dry inhalation of sodium chloride would modify the body temperature, and one of the guinea-pigs inhaled for the purpose of fitting it to play the rôle of a special control in No. V.

An experiment consists usually of three parts: An ante-inhalation stage, an inhalation stage, and a post-inhalation stage; and each of these for convenience of reference is subdivided into periods of six days.

The numbers of the charts correspond with those of the experiments. Their abscissæ show the times, each division representing a period of six days; and each division of the ordinate 10 grm.

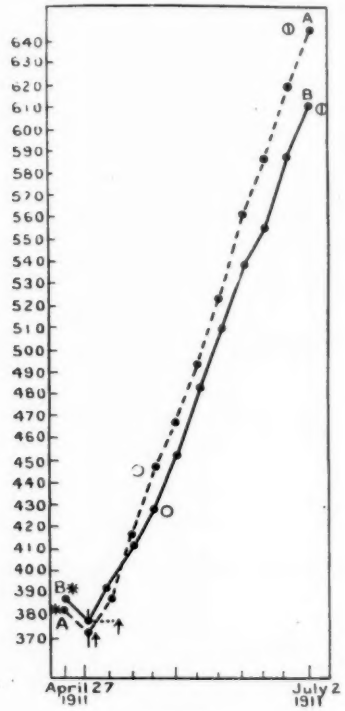


CHART I.

The interval between the marks * and † indicates the ante-inhalation stage, between † and O the inhalation, and between O and Φ the post-inhalation stage.

Experiment I.

Two young male guinea-pigs, (A) inhalor, (B) control, from the same litter. Duration of experiment sixty-seven days. Ante-inhalation stage six days. Animals in same cage. Inhalation stage eighteen days. Nine visits to inhalatorium. Animals in separate cages. Post-inhalation stage forty-three days. Inhalor's surplus-gain

in weight 40 gm. Total gain in weight: (A) 262 gm., (B) 222 gm. Total consumption of food: bread (A) 13·43 gm., (B) 11·84 gm.; cabbage (A) 30·31 gm., (B) 27·63 gm.; lettuce (A) 780 gm., (B) 601 gm.

In Chart I the two lines, after a slight fall from loss of weight ((A) 10 gm., (B) 9 gm.), while the animals were in the same cage, run nearly vertically and are almost straight, showing thus a good and fairly uniform increase in weight from period to period, but with a gradual divergence in conformity with the greater gain by the inhaling guinea-pig.

Experiment II.

Two young male guinea-pigs, (C) inhalor, (D) control, from different litters. In separate cages. Duration of experiment, forty days. Ante-inhalation stage, four days. Inhalation stage, thirty-six days, during which (C) taken to inhalatorium eighteen times. No post-inhalation stage, the experiment having been abruptly terminated through a defect in the machinery. Inhalor's surplus-gain in weight 36 gm. Total gain in weight, (C) 96 gm., (D) 60 gm. Total consumption of food: bread and milk, (C) 19·62 gm., (D) 19·95 gm.; potato, (C) 173·5 gm., (D) 172·5 gm.

During the first twenty-eight days the feeding was very imperfect in respect of both quantity and quality. It consisted of bread, milk, potato and water, without any green food. In it the inhalor gained 16 gm., and the control lost 10 gm.

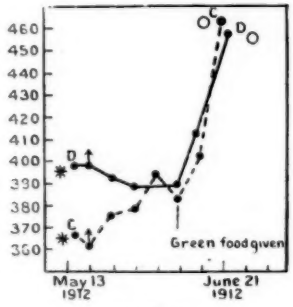


CHART II.

This difference was not due to diarrhœa on the part of (D); but as inhaling generally increases the appetite, and as each animal ate some of its fæces, (C) probably took more of these than (D), for during this period (D) ate a little more bread and milk than (C) did.

During the last twelve days potato was replaced by lettuce, and both this and bread were abundant.

Total consumption of food: bread and milk, (C) 19·62 gm., (D) 19·95 gm.; potato, (C) 485 gm., (D) 483 gm.; cabbage, (C) 20·27 gm., (D) 19·84 gm.; lettuce, (C) 345 gm., (D) 345 gm.

Chart II, especially in the horizontal part, shows that even when the food is scanty and unsuitable the dry-inhalation of sodium chloride will under some conditions act beneficially.

Experiment III.

Two young male guinea-pigs, (E) inhalor, and (F) control, from the same litter. Duration of experiment 102 days. Ante-inhalation stage thirty days. Inhalation stage forty-two days with twenty-one visits to inhalatorium. Post-inhalation stage thirty days. Inhalor's surplus-gain in weight 42 gm. Total gain in weight, (E) 156 gm., (F) 114 gm. Total consumption of food: Bread, (E) 2074 gm., (F) 1949 gm.; potato, (E) 1350 gm., (F) 1355 gm.; cabbage, (E) 3672 gm., (F) 3672 gm.

In this experiment the imperfect feeding was continued throughout, the green food being limited to 36 gm. per diem. And, in addition to eighteen days of severe rationing of bread, potato, and cabbage for both (E) and (F); (E), the inhalor, whose appetite was larger than that of (F), was allowed for the rest of the sixty days only the same amount as was eaten by the latter. By this arrangement the inhalor was hungry during the whole of the first sixty days, and the control only for eighteen days of this period.

Chart III shows no evidence of the influence of the inhaling during these sixty days, though seventeen of the twenty-one visits had been paid to the inhalatorium. But at this date, unlimited bread being allowed, and the inhalor's appetite satisfied for the first time, the lines begin to diverge from the increased gain in weight of this animal.

Experiment IV.

For Experiment IV the guinea-pigs (E) and (F) which were used in No. III were employed. It was desired to ascertain if a very delicate thermometer in the rectum

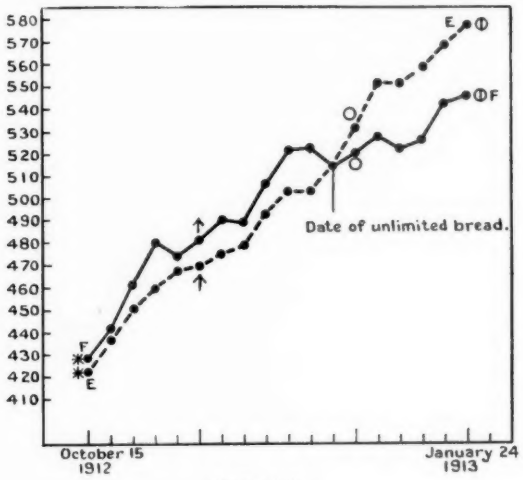


CHART III.

would show a modification of the body temperature through dry-inhalation of sodium chloride; but a sudden struggle on the part of (E) broke the instrument and temporarily incapacitated the animal. As (F) was acting as inhalor at the time, the experiment was continued and (F) thereby fitted to act as a special control in the next experiment.

Duration of experiment, ninety-six days. Ante-inhalation stage, thirty days. As it was necessary for me to finish the experiments by a given date the post-stage of Experiment III was utilized as the ante-stage of the present one. Inhalation stage, thirty days. Post-inhalation stage, thirty-six days.

Diet consisted throughout of unlimited moistened bread with 36 gm. cabbage daily.

Chart IV shows that (E) did not recover the weight it had at the time of the accident till thirty-six days later. It shows also that (F) in the last three periods (eighteen days) of the inhalation stage gained only 1·8 gm. in weight = daily mean of 0·1 gm., while the daily mean for the whole inhalation stage is 0·9 gm. The explanation of this will be given in connexion with the question of the fitness of guinea-pigs for experiments on metabolism.

Experiment V.

My purpose in this experiment was to test the effect on the surplus-gain of the inhalor, when, instead of a normal control, one which had recently inhaled was employed. (E) Inhalor (F) control. Duration of experiment ninety days. Ante-inhalation stage twenty-four days. Inhalation stage forty-four days, with a break of eight days, due to my illness. During this time the animals were put together in a

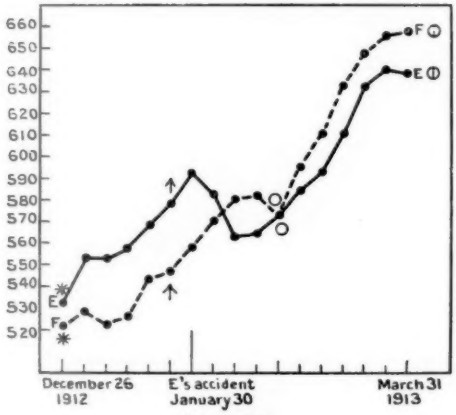


CHART IV.

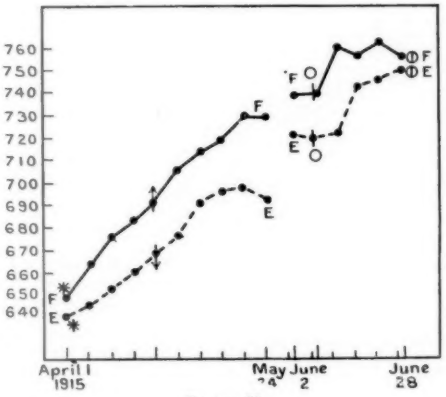


CHART V.

large cage, with bedding of pine shavings, and fed with unlimited moistened bread and cabbage. After return of the guinea-pigs to their separate cages this diet was continued to the end of the experiment. Before the interruption, unrestricted bread with 36 gr. cabbage had been allowed to each daily.

Of thirteen visits to the inhaling chamber nine preceded and four followed occupation of the same cage. Post-inhalation stage lasted twenty-two days.

At the beginning of the experiment the inhalor weighed 8·5 gm., and at its end 5·5 gm. less than the control; so that after ninety days, and after inhaling in the chamber thirteen times, the inhalor gained only 3 gr. more than the control.

The surplus gain in the other experiments was as follows: No. 1, 40 gm.; No 2, 36 gm.; No. 3, 42 gm. The joint occupation for eight days of one cage made of course a knowledge of the total amount of food eaten by each animal impossible. In Chart V the lines diverge from the start up to the point at which the eight-day break began, and the green food ceased to be rationed. (F), during this time, thanks to its recent inhaling, gained in weight more than (E) the inhalor, though, as already mentioned, this had been already nine times to the inhaling chamber. From the date of unlimited cabbage, however, the reverse occurred, and the lines converge.

It may now be asked if the experiments above described are invalidated through the use of guinea-pigs; for it can be correctly said that if Experiment V had terminated eighteen days earlier than it did, the control would have increased in weight more than the inhalor: and that the final change was brought about less by improvement in the rate of the inhalor's increase than through the reduction of that of the control, which in the period mentioned instead of adding to its weight lost 3·1 gm. To the first of these objections it must be replied: That to have discontinued the experiment, at the date named, would have been to do so before allowing time for a manifestation of the full effects of the treatment. In connexion with the second objection it may be pointed out that in none of the experiments is the increase of weight continuous; and that though, in the last three periods of Experiment V, (F), instead of gaining, lost weight (3·1 gm.), there are in the same experiment three similar consecutive periods in which (E)'s total gain was only 2 gm. There are also in the same record two shorter breaks of six days each, in which (E) in the twelve days following the eight-day interruption gained only 1 gm. while (F) lost 0·1 gm.

These breaks in the otherwise continuous increase of weight in the growing pigs own certainly two factors: imperfect feeding and emptying of the bowel. In Experiment II (D)'s continuous fall from 398·5 gm. to 388 gm. may be attributed —at any rate partly—to the first of these causes, but in most of the breaks in the other experiments it is the evacuation of the bowels which produced the arrest, or lessening, of gain in weight. Such emptying of the bowel, however, merely brings the guinea-pig, for the time being, in respect of its weight, into line with a carnivorous animal: being the removal of an accumulation, which, in the latter, would have been voided gradually, prior to the break. The end of such a break is therefore the best period for ascertaining what may be called the real weight of the animal. Examined in the light of such facts, the fluctuations in the weight-records of the above experiments will not be found to prevent a correct interpretation of them. On the other hand they show clearly the imperative necessity of extending each experiment over a considerable period.

Conclusion: (1) The dry-inhalation of sodium chloride in some unknown way increases the appetite and weight of growing guinea-pigs, provided that the food is good in kind and abundant.

This result accords with that observed by Condulmer and Calcaterra,[1] who found that of forty-five children in the primary schools of Bologna, treated with dry-inhalation of Salzomaggiore water, thirty-two, according to the tables of Paglione, exceeded the normal in weight.

[1] *Bollettino delle Scienze mediche*, 1912, lxxxiii, ser. 8, xii, Bologna.

(2) When the diet is defective the good effect of dry inhalation may, according to the severity and duration of the rationing, show itself quite early, or it may be postponed till the feeding is improved. See Experiment II for the former and III and V for the latter result.

It remains to inquire respecting the *modus operandi* of the dry-inhalation of sodium chloride in promoting appetite and growth in young guinea-pigs. On *a priori* grounds it may be thought unlikely that the maximum amount of sodium chloride which could be introduced by Körting's method of dry-inhalation, in the case of an organism which secretes, excretes, and contains so much of this salt as does that of the guinea-pig, would seriously affect the metabolism; but it must be remembered that the process is not one of mere deposition, but of aspiration of the salt. It is further possible that the blood, in consequence of the drain on it by the secretions, may be poor in sodium chloride when it enters the lungs and seize with avidity any small quantity coming from an unusual source.

I would suggest, therefore, that inhalation of dry sodium chloride may act beneficially (1) by increasing the supply of chlorine to the stomach, and (2) by helping to restore and maintain saline equilibrium in the body, as the fundamental work of Ringer, confirmed by that of Loeb, has shown conclusively the necessity of saline equilibrium for the efficient exercise of the functions of animal tissue. And if the food of my guinea-pigs should have contained a sufficient excess of potassium to derange that equilibrium, a portion at least of any absorbed sodium chloride may be expected to have helped in restoring it. Further, as the balance is disturbed by excess or deficiency of any one of the cations of potassium, sodium or calcium there must be occasions when the introduction of a salt of potassium or calcium, or of both these, would be preferable to that of sodium.

As I was prevented from making the necessary carbonic acid- and nitrogen-estimations, it is unfortunately not possible to say if there was a better utilization of the food as well as a larger intake of it; but the interest of the subject will perhaps induce some one else to determine this point.

Professor Peri's prediction that dry-inhalation would prove a valuable therapeutic method seems to have been fulfilled in Italy, for he writes that these inhalatoria are now scattered over Italy, even some of the smaller towns possessing one.

Section of Balneology and Climatology.

President—Dr. CHARLES W. BUCKLEY.

DISCUSSION ON DIURESIS.

Dr. CHARLES W. BUCKLEY (Buxton).

President's Address and Introduction to the Discussion.

I THANK you sincerely for the honour you have done me in electing me as your President for the ensuing year. I have been a member of the Section and of the old Society of Balneology and Climatology for over twenty years, and have seen many changes both in the membership and the work of the Society and of the Section. While some of us may look back with regret to our old Society and with a recollection of the social and fraternal spirit which permeated it, and perhaps even more to our "Journal" which served as a link between the members who were often unable to get to the meetings, it is evident that we have gained something perhaps in dignity and importance, and of late years especially I think we have made definite steps in the direction of advancing the science of hydrology, while climatology has not been altogether neglected. The programmes of the last year or two have been distinguished by discussions on subjects of interest to those who practise in health resorts and whose special province is the study and treatment of chronic disease; their value and interest have been proved by the large attendances. Distinguished workers in these fields have taken a prominent part, and to them we are much indebted. This feature of our work will, I hope, continue. It is the duty of every Member to do something to further the science which we practise, which has been too much neglected for many years, and discussion, criticism and co-operation at our meetings will do more than anything to stimulate research, which is at the root of progress. We who practise medicine in the spas make use of therapeutic measures which differ in many respects from those comprised in the pharmacopœia, and they have received too little attention from the professors of pharmacology and therapeutics from whom we derived what scanty knowledge of treatment we possessed when we emerged from the stage of medical student into that of qualified medical practitioners.

The gross effects of mineral water-drinking, purgation, diuresis, &c., may be obvious, but how and why these effects are produced it is less easy to demonstrate. In addition to these obvious effects and underlying them are far-reaching influences on metabolism, the study of which is likely to throw light on the pathology of many obscure diseases.

The subject we are to consider to-night is "Diuresis" as an effect of the administration of mineral waters. The object in view in promoting diuresis by this means is different, it is to be noted, from that for which the usual

diuretic drugs are administered. We do not seek to remove excess of water from the system as in œdema, but to remove excess of solid matter of a harmful nature, usually resulting from disordered metabolism. For the removal of such matter an increase is necessary both in water imbibed and urine excreted, and if the fluid ingested is to serve a useful purpose it must itself be free from substances in themselves harmful, which excludes many popular beverages, and yet contain elements capable of stimulating excretion and influencing metabolism. For this latter reason distilled water often used for flushing purposes is inferior to some waters with a certain mineral content. There is ample evidence that distilled water only produces diuresis not exceeding the amount taken, whereas many diuretic mineral waters definitely stimulate the system to an excretion in excess of the amount ingested. At the same time it is to be noted that this excessive excretion can only go on for a limited time, or the body would become dehydrated, with loss of weight, thirst, and other undesirable effects. Therefore the most important object is to secure what may be called a diuresis of solids, and only so much extra excretion of liquids as will usefully serve this purpose. Many of the experimental observations on diuresis extend only over a period of a few hours, and show an increased water excretion in this time, but the total excretion of the twenty-four hours may be unchanged so far as water is concerned, though profoundly modified in respect of solids. The effect of the water is not always limited to the hours immediately following its ingestion. I have frequently noted that the Buxton water has a potent effect during the night on some patients past middle life; the usual habit of rising once in the night is insufficient, and three or four urgent calls to micturate may be experienced, though the mineral-water drinking may have been limited to the earlier part of the day.

In considering the effect of mineral waters in this respect, we must first study the effect of plain water drinking on diuresis. The work of Marshall [1], confirmed by Carr [2], as well as that of earlier observers, indicate that excretion of chlorides is apparently increased, but the increase is variable and generally less marked than that of urea, and at the height of diuresis the chloride elimination generally decreases; creatinin is little affected. It appears to me that these observations are open to the criticism in respect of the chlorides that no fluid being given in the control period the chloride excretion must be limited to a subnormal amount, there not being sufficient water available for its excretion. I should attach more importance from our point of view to a control in which the normal kind and quantity of fluid had been taken, and in the experiment, the same fluids plus a fixed amount of water. Another point is, that I cannot discover whether distilled water was used or an ordinary "tap" supply. That this is not unimportant is shown by the observations of Keller [3], which showed that "hard" water had a greater diuretic power than distilled water, in respect of water, chlorides and urea. The amount of lime salts would be small in the hard water, but evidently they had a very decided effect, and it is unfortunate that no analysis of this water is given in Keller's paper.

The amount of the different salts contained in an ordinary dose of a mineral water is very much less than would be required to produce a diuretic effect if administered in artificial solution. Two explanations have been offered, first, that the mineral elements are more active in the "natural" form, being largely in the form of free ions; secondly, that the important matter is the combination of mineral constituents, one acting as adjuvant to another, or it may

be as opponent in some instances. Both explanations are probably right. That certain constituents act as adjuvants to each other is shown by the work of Schloesser [4] who found that the sulphates of magnesium and sodium injected intravenously with caffein caused a more intense diuresis than would correspond with the sum of the single effects. Caffein with sodium salicylate and theocin in combination produce a diuretic effect, which is not greater than that which would correspond to the sum of the single effects. Observations on the same lines upon other salts would be useful.

The action of mineral waters in producing greater excretion of water and of urinary solids is upon the renal cells, but also, as Blum [5] has shown, upon the tissues generally. He defines, therefore, a group of "interstitial" diuretics in addition to the older classification of cardiac and renal. This is based upon his work on calcium chloride as a diuretic. This salt acts by dehydrating the tissue colloids as do other salts, notably those of potassium, thus setting free water which is excreted. Sodium chloride acts in the opposite way by hydrating the colloids and thus locking up water. This action is mainly due to the kations, Ca and Na, but the Cl is not without some influence, since calcium lactate has no diuretic action. Blum [5a] uses CaCl as a diuretic in œdemas with considerable success, but has found that it is essential to place the patient on a salt-free diet. He uses large doses and thus his results cannot apply to mineral water therapy without modification, but they are of extreme interest in the light they throw on the antagonism of Na and Ca, two of the commonest constituents in our mineral springs. Dunn [6], working on different lines, shows that lactic acid which is produced in the body in certain conditions has a similar action to Na in causing what he terms "swelling of the body colloids." He uses various salines to combat this with varying success. It would seem that calcium was the obvious antidote, and in view of the part which lactic acid appears to play in some kinds of rheumatism, some light is thrown on the action of calcium-containing mineral waters, so successful in its treatment.

Three well defined types of mineral waters possess diuretic action, namely the sulphur waters, including the sulphuretted salines, the calcareous and the radio-active thermal waters of low mineralization. I may safely leave the discussion of the action of sulphur to others who understand it better, but when sulphur is associated with NaCl, as in the sulphuretted salines which are widely represented in this country, its action presents a problem of interest in determining to what factors the diuretic effect is due. Recent observations made by Keller at Rheinfelden and Violle [7] at Vittel indicate that sodium ions are strongly inhibitory of diuresis. Violle compared the effect of demi-decinormal solutions of NaCl with equivalent solutions of $CaCl_2$, and also with the natural water of the Grande Source of Vittel, in which both are present in considerably less amount than in his artificial solutions, the amount of Na being infinitesimal. He found that the solution of NaCl was definitely inhibitory of diuresis—that of $CaCl_2$ induced definite diuresis, while the natural water proved powerfully diuretic. He found that the addition of a quantity of NaCl equivalent to half that of the Ca in the artificial solution completely neutralized the diuretic effect. He sums up his conclusions as follows :—

"Each of the elements conceived as ions, entering into the composition of a diuretic mineral water, has its own peculiar action. Each of them contributes to the recognized properties of the water in question or, on the other hand, diminishes it by a contrary action. Therefore there should be in existence certain relations between the constituents of the water. If these relations do not exist, the water does not

possess any physiological property, and in consequence any special therapeutic quality. It is not so much in the variety of the elements and in their respective quantities, but in the relative proportion of those elements that the property of a mineral water resides. The contrary effects must be neutralized in order that the whole of their value may appertain to certain elements. And as these relative proportions are met with comparatively rarely, it is obvious that among the considerable number of mineral springs to be found on the surface of the globe it is only some that possess any therapeutic properties."

These conclusions to which Violle has come are of great importance though perhaps not entirely new, but they indicate the reason why two waters of apparently similar composition often differ widely in effect, and they also show that clinical observation, supported afterwards by experimental demonstration, is the only sound policy by which to investigate the value of a water; little can be discovered by chemical analysis alone.

I think it would have been an advantage if the solutions used in these experiments had approximated more nearly to the actual strength of the mineral water against which they were tested. The weaker the solution the higher the degree of ionization, other things being equal, and it may easily be the case that the diuretic power of the weaker solutions would be greater than that of the stronger. This question will no doubt receive further consideration in the future. Violle used distilled water diuresis as the standard of comparison and limited his observation to the six half-hours after the water had been taken —a method which may serve for comparative effects, but is inadequate for the proper investigation of the excretion of solids as well as of water. His papers are worthy of careful study and show that we must investigate the interaction of the mineral constituents of our waters, and much work needs to be done on other salts than those of Ca and Na. For example, we note from these researches that the effect of Ca in inducing diuresis may be neutralized if half the quantity of Na ions are present or added. Yet diuresis is claimed as an effect of many of the sulphuretted saline waters, and in those of this country the sodium is much in excess of the calcium salts. The proportion of Ca to Na is highest in the waters of Llandrindod, slightly lower in the other Welsh spas and by far the lowest at Harrogate. Some comparative experimental work on these waters would throw more light on the problem than work with artificial solutions. There must be other salts than CaCl and NaCl at work and evidently sulphur is one of these, while the action of the salts of magnesium calls for investigation. It may be that when Na is in excess the action of Ca is modified, for you will recollect that Blum only obtained a diuretic effect with CaCl when the patient was placed on a salt-free diet. Zeller [8] found that diuresis resulted from solutions of NaCl, administered intravenously and also orally, but in the latter case the diuresis appeared promptly and quickly ceased. The problem is one of great complexity and may be much affected by the amount of salt in the diet and the amount of fluids other than mineral water ingested before and after the experiment. We must remember that diuresis of water is chiefly referred to in these experiments, but the solids are more important, and from Keller's work it is seen that NaCl notably increases the excretion of urea. Baird and Haldane [9] administered hypertonic salt solutions to be followed by copious water drinking; and they showed that the diuresis which resulted was independent within wide limits of the amount of water ingested. They state that the excess of salt is stored probably in the skin and connective tissues.

There is not time to discuss the action of different salts upon the kidney

tubules, but the observations of Hamburger [10] are noteworthy. He shows that the endothelial cells of the kidney as well as of other organs are very sensitive to any change in the calcium content of the plasma; a very slight change considerably alters the cellular permeability.

The calcareous mineral waters, of which the best examples, if the quantity of Ca present is to be our standard, are the waters of Contrexéville, Wildungen, &c., which are noted for their action on the kidney. Keller, of Rheinfelden, has investigated the action of two springs of this class comparing them with artificial solutions and ordinary hard water, the latter proving definitely more diuretic than distilled water, in respect of water, chlorides and urea. The Kapuzinerberg source containing bicarbonate and sulphate of calcium proved strongly diuretic, the Magden source containing sulphates of calcium and magnesium was definitely less effective in respect of excretion of water but caused a notable increase in the excretion of chlorides and urea in the five hours following its ingestion. Tarasp water was also included in the scope of his investigation: it contains NaCl and Na_2CO_3 and it showed the inhibitory power of Na upon diuresis. An important detail is that the natural mineral waters produced a greater diuretic effect than an equimolecular solution of NaCl.

While discussing diuresis one must not forget that this obvious result of the ingestion of water may be less important than the action on metabolism. One important effect of calcium has been pointed out from time to time by different workers without its attracting perhaps the attention it deserves. Kionka [11], in 1900, recorded experiments showing that Ca tends to diminish the amount of uric acid excreted in the urine. Miyadera [12], in 1921, stated that the ingestion of Ca in the form of easily absorbable compounds does not influence the total nitrogen metabolism but reduces the amount of uric acid in the urine. This reduction, he suggests, depends on diminished production of uric acid in intermediary metabolism. Observations upon the blood uric acid indicate that this is progressively diminished under the influence of certain mineral waters containing Ca.

Another detail may be of importance in the investigation of the calcareous mineral waters; they differ widely in the proportion of $CaCO_3$ and $CaSO_4$ they contain. It has been shown that the carbonate is rapidly converted into the easily absorbed chloride in the stomach but there is evidence that the sulphate is much more difficult of absorption, which indicates that the carbonate is the more useful salt. In the waters of low mineralization, however, in which the Ca is almost completely ionized this point is perhaps of less importance.

The third group, the radio-active waters of low mineralization have as their chief solid constituent, Ca together with a large number of other salts often in very minute quantity, making it a matter of great difficulty to determine which constituents are responsible for their characteristic activity. The degree of ionization is almost if not quite total, and they are charged with considerable amounts of nitrogen, CO_2, argon, helium, &c., and are also radio-active to a marked degree, owing to the emanation or "niton" they contain.

I am not aware of any experiments having been published on the diuretic effect of waters of this type but the clinical evidence is overwhelming. We know that distilled water is negative, and that some "hard" waters are diuretic, but experiments which will be mentioned by a later speaker show that the diuretic effect of the Buxton water which is typical of this class is 25 per cent. higher than that of distilled water.

The effects of the gaseous constituents and of niton call for investigation, and to know their influence upon diuresis, if inhaled, would be interesting. The action of radium is generally admitted to be that of a stimulant to nutrition and metabolism, but many of the published investigations are contradictory and far from convincing as to the other effects possessed by radium emanation when used internally. Gudzent [13] and others published experiments with thorium X as a radio-active substance showing that the emanation temporarily increased the uric acid in the blood and urine and that Ca increased that in urine and diminished that in blood. McCrudden and Sargent [14] found no effect from artificial radium water on either blood, uric acid, rate of uric acid excretion, or of total N, or water. The work of other observers on Ca to which I have referred is also opposed to Gudzent's findings.

I do not think that experiments with artificial radium water can be regarded as throwing any light on the action of natural radio-active waters in which in my opinion an important effect of the radium is the action it exerts on the mineral constituents, whether by ionizing them or as an adjuvant to their action. There is clinical evidence that the radio-active waters lose much of their characteristic effect unless taken fresh at the spring, and since the gases and niton are the elements which disappear it would seem that to them must be attributed important effects.

The question as to how mineral waters produce diuresis has received some consideration in the light of their action on the tissue colloids, but I do not think this is adequate to explain the diuretic effect of a mineral water the Ca content of which is less than a grain in an ordinary tumbler and the total solids of which may not be more than two grains, the gaseous constituents being the most notable. Some observations made for me on normal individuals showed, in addition to the diuretic effect of the Buxton mineral water, a slight but definite and fairly constant fall of blood-pressure within a very short time of drinking the water, and this is suggestive.

I venture to put forward for your criticism the idea that the explanation may lie in the action of the pituitary gland. That this gland plays an important part in regulating diuresis is supported by much evidence. Schafer states that diuresis may occur from the action of the posterior lobe without corresponding rise in blood-pressure. Houssaye, Galan, and Negrete [15] found that intravenous injection of pituitary produced diuresis in the dog but a decrease in the urine of the rabbit, but in each case with a constant volume of water ingested the diuresis in twenty-four hours remained constant. Konschegg and Schuster [16], however, state that intravenous administration in rabbits led to increased diuresis if the dose were not too high, but with larger doses the increase gave place to a decrease.

Farini and Cessaroni [17] found that extracts of the posterior pituitary had the property of increasing diuresis and diminishing arterial pressure.

Stoland and Korb [18] found that injection of pituitrin into dogs seemed to act as a stimulant to the kidney in that it produces such a marked secretion of urine that the N-content of the blood falls far below normal. This seems to indicate more than mere water excretion. Douglas Cow [19] showed that diuresis resulting from perfusing the kidney with saline extracts of the intestinal mucosa, previously passed through various parts of the body, was dependent on whether the saline had passed through the pituitary circulation or not.

Sylvestri [20] sums up his researches in the statement that the pituitary stimulates or checks the activity of metabolism in general, including the production and destruction of uric acid.

To sum up, slight stimulation of the posterior lobe causes diuresis and fall of blood-pressure; increased stimulation reverses this effect; metabolism generally and the production and destruction of uric acid and also the excretion of chlorides, are affected.

These are precisely the most notable effects resulting from the ingestion of mineral waters of the group we are discussing. Further, one notices occasionally that a patient instead of passing more urine after drinking the water, passes less, and it seems possible that this may be due to over-stimulation of his pituitary and that much smaller doses are indicated in such cases.

In advancing this theory that the action of certain mineral waters, and notably the simple thermal radio-active group, is primarily as a stimulant to the pituitary, I am conscious of the slenderness of the evidence in support, but I think there is sufficient to induce further investigation which may bring to light important facts.

In putting forward these remarks to open the discussion I have not attempted to cover the whole field or to make any original contribution to the bio-chemistry of mineral waters, but simply to review the subject and to bring into prominence some problems which urgently call for solution.

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Mr. J. RACE (Devonshire Hospital, Buxton).

The subject under discussion to-day is a very important one. Its many phases have been investigated on numerous occasions, but it must be admitted that, with the exception of a few general principles, the process stands upon an empirical rather than upon a scientific basis. It is true that diuretics have been classified according to their mode of action and the type of diuresis produced, but physiologists are still divided between a modified form of the mechanical theory of Ludvig and the vital theory of Bowman. The latter is probably more generally accepted but even this theory fails to explain in any way the ability of the kidneys to break the second law of thermodynamics by occasionally excreting a urine of higher osmotic pressure than that of blood. And if one law can be broken is there any reason why all other so-called laws should be obeyed? It must be remembered that scientific laws are usually established by the investigation of inert matter. At present they are often inadequate to explain the mechanism of the living cell and are practically of no value in accounting for the variability in the properties of what are apparently identical cells. This variability in the response of the cell, and of the aggregate of cells, &c., constituting the human body, is often so marked that it is impossible to draw accurate conclusions from experiments made upon

one individual. A person may be apparently normal and yet exhibit an idiosyncrasy in some particular direction. To overcome this difficulty the series of experiments made at Buxton on the diuretic effect of water has been made on a number of cases and I propose only to give the average results, as these, when the number of cases is sufficiently great, practically eliminate the variations due to the personal idiosyncrasies.

The particular phase of diuresis under investigation was the comparative effect of distilled water and the Buxton thermal water on cases of tophaceous gout. The method selected was to place the patient on normal hospital diet, which contains 50 oz. of fluids, for several days, until a general equilibrium was established and to collect the urine over twenty-four-hour periods; in the second period the fluids were supplemented by 20 oz. per diem of distilled water divided into three equal portions; in the third period the distilled water was replaced by mineral water. In many cases (fourth period) the diuretic effect of mineral-water baths was determined by giving this form of treatment on alternate days in addition to the mineral water taken internally.

The use of a diet of slightly variable composition is admittedly open to criticism but in practice it was found that the total nitrogen excreted in the urine was fairly constant, and the method possesses the advantage that the diet is a normal rather than artificial one and is consequently less prone to cause digestive disturbances. This is an important desideratum when the products of intestinal decomposition are under observation.

In sixteen cases the following average volumes of urine were obtained:—

TABLE I.

Period I	(no extra fluids)	...	...	...	1,066 c.c.
Period II	(distilled water)	...	...	...	1,349 c.c.
Period III	(mineral water)	...	...	...	1,626 c.c.

In two cases (12·5 per cent.) distilled water failed to produce a diuresis and in only one case did the mineral water fail to produce a greater diuresis than distilled water (distilled water, 1,253 c.c.; mineral water, 1,245 c.c.).

In eleven cases that were tested as regards the diuretic effect of the baths the following figures were obtained:—

TABLE II.

Period I	(no extra fluids)	...	...	...	1,113 c.c.
Period II	(distilled water)	...	...	...	1,409 c.c.
Period III	(mineral water)	...	...	...	1,693 c.c.
Period IV	(mineral and baths)	...	...	...	1,745 c.c.

In three cases (27·2 per cent.) the baths failed to produce an increased diuresis over that obtained by the use of the mineral water taken internally.

From these figures it is evident that distilled water did not, in the cases under observation, produce a diuresis equal to the volume of water ingested. The increase in the volume of urine excreted is only 283 c.c., or 50 per cent. of the water taken. With mineral water the increase in volume is 560 c.c., or 98 per cent. of the water taken.

The difference between these increases leaves no room for doubt as to the superior diuretic effect of the mineral water on these cases of gout. The difference is both definite and appreciable.

The increased volume obtained by the use of mineral-water baths at an indifferent temperature (96-98° F.) over that obtained by the ingestion of mineral water is only 52 c.c. on the average (3 per cent.). So small a figure,

combined with the fact that there was no increase in three cases shows that, at this temperature, the diuretic effect of the mineral-water ingestion is not materially altered. The effect of mineral baths alone, i.e., without mineral-water ingestion, is now under investigation.

The average diuresis produced in the twenty-four-hour period was slightly under 100 per cent., i.e., the increased flow of urine was slightly less than the volume of mineral water ingested. I am inclined to doubt if a diuresis of over 100 per cent. can be obtained in experiments lasting many days unless there is a previous condition of hydræmic plethora. Diureses of 125 per cent. have been reported in experiments lasting about five hours, but it is very probable that the subsequent periods would have recorded a compensatory deficiency. A sustained diuresis of over 100 per cent. could only be obtained at the expense of the water in the tissues, &c., and if these only contain the normal amount the balance is disturbed and the deficiency would be replaced by a greater ingestion in order to satisfy thirst.

The excretion of sodium chloride was as follows :—

TABLE III.

	16 cases.	11 cases.
	grams per diem	
Period I	7·3	7·4
Period II	8·2	7·8
Period III	8·4	8·2
Period IV	—	9·5

The distilled water slightly increased the elimination of sodium chloride but its effect was not so great as that of the thermal water. Baths appear to have an even greater effect, as the increase is larger and occurred in every case. The increased salt elimination is comparatively small (about 1 grm. per diem) but larger increases could scarcely be anticipated without appreciably reducing the salt content of the blood. The removal of an excess of salt from the blood is important on account of the depressant action of the sodium kation on the solubility of sodium urate by what is usually known as the common ion effect.

One of the most common features, in my opinion, of diuresis, is the tendency to reduce the hydrogen ion concentration of the urine and as this is probably controlled by the hydrion content of the blood it follows that diuresis tends to increase the blood bicarbonate. Henderson and Palmer, and Carr, have also found that the PH of the urine increased with an increase in the volume. The alkalinity of the ingested water is quite inadequate to effect this change *per se* and it must be assumed that it acts indirectly by altering the utilization of the bases in the diet.

The hydrion concentration of the urine of the gout cases admitted to the Devonshire Hospital is invariably excessive, the median during the past four months being PH 4·8, as compared with a normal median of 5·95 found by Henderson and Palmer on a series of cases.

A reduction in the hydrion concentration is probably important in several directions. It would increase the solubility of sodium urate in blood to an appreciable extent, for although possible changes in the PH are quite small, an increase from 7·25 to 7·35 increases the solubility by as much as 18 per cent. The urine would be affected similarly, but not to such a large extent if the PH is low, owing to the flat nature of the solubility curve for uric acid below PH 6.

Mineral water diuresis, possibly through its effect upon the hydrion concentration, also tends towards more complete nitrogen metabolism. In nine of

the cases investigated the average amounts of urinary nitrogen appearing as urea and uric acid were the following:—

TABLE IV.

	Urea.	Uric acid. grams of N per diem	Ratio.
Period I	7·65	0·128	59/1
Period II	7·72	0·117	66/1
Period III	7·85	0·113	69/1
Period IV	7 83	0·114	68/1

The increased amounts of nitrogen excreted as urea are small but quite definite, and are not due to variations in the total nitrogen. Complete analyses show that the percentage of the total nitrogen appearing as urea increases about 3 per cent. between periods I and IV.

The excretion of uric acid diminished with increased diuresis, a result that was quite unexpected, but which has also been reported by Garrod, who also quotes Boeker and Genth to the same effect.

In these cases there was no increase in the uric acid content of the blood—the average amount of uric acid was reduced by 25·5 per cent.—so that there was either a reduced absorption of uric acid precursors from the intestines, an increased destruction of uric acid, or a reduced production of uric acid in intermediary metabolism. Possibly all three actions occurred. I am inclined to attach considerable importance to this indirect effect of the mineral-water diuresis on gout cases, as it appears to get to the primary cause of the disturbances producing this disease.

The results obtained with the Buxton mineral water appear to justify the following conclusions:—

(1) That the mineral water has a definite and specific diuretic effect over and above that of distilled water.

(2) That it increases the elimination of sodium chloride.

(3) That it decreases the hydrion concentration and favourably influences nitrogen metabolism by increasing the percentage excreted as urea and reducing the percentage excreted as uric acid and undetermined nitrogen.

Dr. FORTESCUE FOX (London)

said that he welcomed a serious investigation and discussion on diuresis, which was fundamental to the actions of waters, but which, he believed, had not been formally discussed since the foundation of the Balneological Society. It was appropriate that the opener should come up from Buxton, which was the chief diuretic spa of Britain. A condition of toxæmia was now recognized in the majority of cases submitted to spa treatment, and therefore the study of renal elimination became essential. Observations from Switzerland and France had been published in recent numbers of the *Archives of Medical Hydrology*. He (Dr. Fox) suggested that the period immediately following the ingestion of waters ought to be included in the investigation—as diuresis often continued for hours or even days. As regarded the effects of baths, they would probably be found to depend chiefly on temperature, the cooler baths checking the action of the skin and favouring that of the kidneys, according to the Dastre-Morat law. All spa practitioners were familiar with cases in which exceptional diuresis followed small doses of waters. These cases needed careful study. He recalled the effect of a strong sulphuretted water containing calcium sulphate, which was given in wineglassfuls in the early morning. The diuretic effect of this water was proverbial and often inconvenient.

Dr. J. F. HALLS DALLY (London)

said that contrary to general belief recent work had shown that the ingestion even of large quantities of water failed to raise the blood-pressure of normal persons, and that there were reasonable grounds for belief that various portions of the kidney tissue exercised a selective action in the elimination of different substances, e.g., salts and nitrogenous bodies.

Dr. J. B. BURT (Buxton).

For many generations we have had clinical proofs of the specific action of mineral waters. Mr. Race's contribution to the discussion has given us the much desired scientific proof.

An interesting question arises from a consideration of his first and second tables (p. 18): Is the calcium ion in the Buxton water the only factor in producing the water diuresis? Dr. Violle has studied this question with regard to the calcium waters of Vittel and finds that the waters of the Grande Source produce seven times the diuresis caused by an artificial mixture of the same amount of calcium and distilled water.

In the third table (p. 19) the most interesting fact forthcoming is, in my opinion, the increased excretion of sodium chloride produced by the baths as compared with drinking the waters only. Unless we assume that the gases inhaled from the surface of the bath are the cause of this, we must consider that the baths have a physical effect on the nerve-endings of the skin, possibly by way of stimulating the posterior pituitary gland through the spinal cord. With regard to the fourth table (p. 20) the alteration of the urea uric acid content is extremely important. The production of an increased oxidation of waste products is of great value in the treatment of such conditions as old age and gout. Again the question arises as to the importance of the calcium ion. Blair Bell has shown the special importance of calcium in relation to the ovaries, and other writers have proved a definite relation between the parathyroid and calcium. It seems quite reasonable to suggest that the action of the posterior pituitary glands may also be stimulated by calcium.

There are several grounds for suggesting that the extremely active calcium ion found in some thermal waters is responsible for many of the beneficial results experienced by patients which take them. Against this, however, it is the fact that a number of mineral waters which contain no calcium benefit the same class of case.

Dr. W. EDGECOMBE (Harrogate).

The problem of determining whether a given mineral water has a true diuretic effect appears at first sight simple; on critical examination, however, it becomes very complicated. The conditions of experiment may be laid down as follows:—

The subject should be on a fixed diet in a state of nitrogenous equilibrium; the fluid intake must be carefully measured; the salts in the diet should be estimated with the same care as the salts present in the mineral water ingested; the output from lungs and skin must be measured as well as that from the kidneys; the temperature and moisture of the atmosphere must be kept uniform; the subject must be kept at rest, or the amount of muscular work rigidly controlled; mental and nervous disturbances must be eliminated in view of their effect on blood-pressure; and, lastly, the weight of the subject must

be carefully observed. These conditions having been regulated as far as is possible, the problem then is to determine: (*a*) Whether diuresis occurs; (*b*) if so, is the plus secretion merely temporary in rate and quantity; is there a period of reaction and a decrease afterwards? (*c*) is the total water and salts balance affected; that is to say, is the system richer or poorer in water and salts after prolonged use of a mineral water; and, if either, what are the mineral substances that produce the observed result?

From the practical point of view of treatment, the further questions arise: (*a*) The extent to which the kidneys eliminate the mineral water ingested; (*b*) whether the diuresis excites the elimination of the substances.

Lastly, there remains the problem of bio-physics: (*a*) The rôle played by the rate of blood-flow through the kidney, conditioned by changes in the volume of blood and in blood-pressure; (*b*) the part played by the salts in their action on the kidney epithelium.

The complete investigation, therefore, is one of great complexity and so far as I am aware has never been undertaken with respect to any British mineral water.

From time to time during past years observations have been made by different workers (Bain, Edgecombe, David Brown and Woodmansey) on the effect of the Harrogate mineral waters. The conditions of experiment, though falling short of the requirements above stated, were partially controlled, the subjects being on a fixed diet. The waters examined fall into two groups: the strong saline-sulphur water having a definite aperient action; and the mild saline-sulphur, considered clinically to act on the kidneys as diuretics and having no aperient effect. The results show that the stronger aperient waters have a diuretic effect, varying inversely with the drain of fluid from the bowel; the diuretic effect being apparently greater relatively than that produced by the milder saline-sulphur waters. These latter produced a diuresis in the experiments on normal subjects under consideration less than was anticipated or than is observed clinically. No attempt was made to determine the salt or salts that produce the effect. Both groups are complex solutions containing many substances, the predominating constituent being sodium chloride. Inasmuch as a greater relative diuresis was observed with the waters having the higher sodium chloride content, it would appear that this substance in the waters used has no marked inhibitory effect on diuresis from the normal kidney.

Clinically, it is found that, in patients with healthy kidneys, if the aperient action of the stronger water fails, a profuse diuresis is set up. If the kidneys are defective, as evidenced by a persistent polyuria of low specific gravity, a condition of hydræmic plethora is readily induced with chloride retention, hydræmia of the tissues and a consequent rapid increase in weight. The same takes place to a less extent with the milder saline-sulphur waters, and both should therefore be used with caution when renal inadequacy is suspected. Also, in cases of obstructed micturition from mechanical causes, such as enlarged prostate, diuretic waters should be withheld.

Lastly, the question must always be borne in mind: Is diuresis in a given case necessary or beneficial? That diuresis induced by mineral waters is accompanied by an increased output of nitrogenous end-products has been repeatedly shown; but that the increase is more than would be accounted for by the flushing out of waste products accumulated in the tissues has not been proved. No reliable evidence has been adduced, so far as I am aware, that the ingestion of mineral waters can provoke active tissue changes. More light is needed on the subject. The laborious character of the investigation unhappily

renders its performance by spa physicians in active practice a matter of the greatest difficulty.

Dr. BUCKLEY (in reply)

said that he agreed that it was necessary to emphasize that diuresis, if it concerned water alone, was not an object to be aimed at, but if the increased excretion of water was accompanied by an increased elimination of waste products in solution a very useful end was attained, and even more important was the associated effect upon metabolism indicated by Mr. Race's experiments. He did not attach any special importance to the increased excretion of urea in itself, but he did to the variation in the urea-uric acid ratio, in respect of which the criticisms as to the influence of variation in diet, &c., had not the same force. He was strongly in agreement with Mr. Race that the difficulty in obtaining ideal experimental conditions and the prejudicial effect of such an abnormal habit of life upon the subjects of experiment rendered observations on a number of individuals under normal conditions preferable, and that the errors were largely eliminated by the average of a number of observations. He thought it would be worth while to investigate the effects of the saline waters on these lines; his remarks on their action had not been intended to belittle their value but to indicate the trend of modern criticism as to the mode by which they produced their beneficial effects.

Section of Balneology and Climatology.

President—Dr. Charles W. Buckley.

The Causes of Brachialgia.

By J. Barnes Burt, M.D. (Buxton).

The Causes of Brachialgia.

In this paper the term brachialgia is used with reference to pains radiating along the nerves of the brachial plexus, just as the term sciatica is used with reference to pains radiating along the sciatic nerve.

In England the majority of people call all cases in which pain radiates along the nerves of the arm brachial neuritis, even when no organic changes are found in the nerve in any stage of the disease.

Genuine brachial neuritis in the earlier stages may begin as a brachialgia before any microscopic changes can be found in the nerve tissues. I must, therefore, mention the causes of brachial neuritis as among the possible causes of brachialgia, but this paper chiefly concerns the big group of cases in which no organic changes in the nerves are found in any stage of the disease, and for this group the term brachialgia ought to be reserved.

Recognized Causes.

(1) *Toxic.*—Diabetes, arsenic, lead, gout, influenza.

(2) *Injury.*—Crutch palsy, Saturday night arm, obstetric arm, pressure on brachial plexus, carrying of heavy weight on the shoulders, tight braces.

(3) *Secondary Symptoms.*—Cervical rib, osteo-arthritis cervical vertebræ, malignant glands in axilla, syphilis of spinal meninges.

(4) *Spread of inflammation* from surrounding tissue into the nerves, as in subacromial bursitis, inflammation of the shoulder-joint, spread of fibrositis from a muscle into the tissue of the nerve.

(5) *Referred pain* as in certain cardiac conditions.

This long list quite fails to explain a large number of cases of brachialgia.

Several years ago I noticed, like several other physicians, that many patients complaining of brachialgia had pain in the neck and scapula muscles, and in the case of all patients consulting me for pain down the arm I have made careful notes with regard to the condition of these muscles.

The results of this study are shown in a series of Tables made from an analysis of my last 100 cases of brachialgia occurring in private and hospital patients. All cases of muscle wasting and anæsthesia have been excluded from the analysis. It must be remembered that the majority of these patients have passed through the hands of other doctors before seeing me, and hence cases,

the result of recognized causes such as cervical rib and diabetes, will be below the average. For purposes of comparison the cases are divided into three groups:—

Group 1.—Brachialgia due to well recognized causes such as cervical rib, trauma and subacromial bursitis.

Group 2.—Brachialgia associated with fibrositis of the supra- or infraspinatus muscles.

Group 3.—Brachialgia in which physical signs were indefinite.

TABLE I.—SHOWING NUMBER AND SEX OF PATIENTS IN PRECEDING THREE GROUPS.

	Cases	Female	Male
Group 1	36	19	17
,, 2	53	38	15
,, 3	11	8	3
	100	65	35

Of the 53 cases 32 had fibrositis of supraspinatus only.
10 ,, ,, ,, infraspinatus only.
11 ,, ,, ,, both supra- and infraspinatus.

TABLE II.—SHOWING SIDE AFFECTED IN PRECEDING THREE GROUPS.

	Right	Left	Right and Left
Group 1	16	16	4
,, 2	24	23	16
,, 3	2	5	4
	42	44	24

TABLE III.—SHOWING NUMBER OF CASES ASSOCIATED WITH FIBROSITIS ELSEWHERE AND PROPORTION OF CASES IN WHICH ABNORMAL SENSATIONS SUCH AS TINGLING, PINS AND NEEDLES OCCURRED.

	Associated with fibrositis elsewhere		Tingling, &c.	
Group 1	8	22 per cent.	8	22 per cent.
,, 2	35	66 ,, ,,	26	50 ,, ,,
,, 3	7	63 ,, ,,	4	36 ,, ,,
	50		38	

The most striking point in these Tables is the large number of cases associated with fibrositis of the supra- and infraspinatus muscles. Until I made this analysis of notes which extended over a period of five years, I had no idea it was so common.

Is it possible that my prejudice in favour of fibrositis of the spinatus muscles blinded me to other causes? I think not, for a study of the question shows that in women of the upper and middle classes fibrositis of the spinati muscles is commoner than fibrositis of any other muscles, with the possible exception of the trapezius.

The reasons for this are the following:—

(1) The present fashion of wearing heavy coats and furs which hang straight from the shoulders.

(2) The wearing of evening dress, the neck muscles after being warmly clad all day are exposed to draughts and chills without any protection.

(3) The craze for knitting which started in the war and continued up to 1921. This is an important factor in causing fibrositis of the supraspinatus muscles, for it is this muscle which starts abduction of the humerus before the deltoid comes into play.[1]

[1] Cathcart, *Journ. Anat. and Physiol.*, xviii.

A study of my notes supports these observations, only fifteen men affected as against thirty-eight women—it is very much rarer in hospital practice than in private practice. I had a larger proportion of cases of brachialgia in 1919, 1920, 1921, but in the last year or so they have diminished, probably because the knitting craze is dying out.

An important point to study is the exact distribution of the pain and tingling.

With regard to the distribution of pain, this is difficult, as most patients appear to be unable to localize the pain to definite areas. The pain of brachialgia is often variable in distribution and character, sometimes extending to the elbow, at other times to the wrist; it is diffuse in character, and the whole arm may feel tired and heavy.

Cases of brachialgia due to subacromial bursitis form an exception to the above. Amongst my own cases 77 per cent. of the patients localized the pain to the back of the arm and forearm.

With regard to tingling, numbness, and pins and needles, it was possible to get more accurate answers, and the following Table has been drawn up from thirty-four cases:—

TABLE IV.—SHOWING DISTRIBUTION OF TINGLING, PINS AND NEEDLES, &c.

	In all fingers	In fingers supplied by median nerve only	In fingers supplied by ulnar nerve only
Group 1 ...	50 per cent. ...	10 per cent. ...	40 per cent.
,, 2 ...	66 ,, ,, ...	30 ,, ,, ...	4 ,, ,,
,, 3	Answers too indefinite to classify.		

These figures are too small to be of very much value, but, roughly speaking, pain confined to the back of the arm suggests subacromial bursitis, pain along the front of the arm, most intense on the radial side, with tingling in the fingers supplied by the median nerve, suggests the spinatus group. Pain along the front of the arm, especially along the ulnar side, with tingling in the fingers supplied by the ulnar nerve, suggests cervical rib.

The question now arises, how does fibrositis of the spinati muscles set up brachialgia? You will remember that the suprascapular nerve passing through the suprascapular notch sends a branch along the surface of the bone to supply the supraspinatus muscle, the nerve then winds round the spine of the scapula to supply the infraspinatus muscle. These two muscles are bounded on two sides by bone, on the third side by a strong aponeurosis, so firm that fibres of the muscle rise from it. Thus, any inflammatory change in the muscle not necessarily near the nerve exerts a definite pressure on the nerve, squeezing it against the bone. The circulation is also interfered with. The suprascapular nerve comes from the outer cord of the brachial plexus, which derives its fibres from the fifth and sixth cervical nerves; and it is the pressure on the suprascapular nerve which gives rise to pain in the skin supplied by the musculocutaneous and median nerves, viz., front of the arm, on the outer side; hand and the tips of all fingers except the little finger.

I have shown that in many of the cases it is impossible strictly to localize the pain in these areas but exactly the same thing occurs in neuralgia of the fifth nerve, a small hole in an upper molar may set up a pain along the supramaxillary division only, the supra- and infra-maxillary, or all three branches; it may even set up pain in the supra-orbital division without pain in any of the other divisions.

Thus, on both anatomical and clinical grounds, I believe that the commonest cause of brachialgia is fibrositis of the supraspinatus muscle and infraspinatus

muscle, the result of pressure on the suprascapular nerve, and not the result of the spread of inflammation from the muscle into the nerve.

Of course it is possible for the inflammation to spread from a muscle into the nerve, but this event is rare and when it does occur it will explain the wasting of these muscles.

Fibrositis of these muscles affords an exact explanation of many of those cases of brachialgia which are associated with gout or septic foci; and it will also explain some of those traumatic cases caused by carrying weights on the shoulder or wearing tight braces.[1]

Dr. Williamson has explained these traumatic cases as being due to pressure on the brachial plexus; I think they are far more likely to be due to pressure on the supraspinatus.

I consider that brachialgia associated with fibrositis of the spinati muscles forms a definite clinical entity. The cases frequently begin with stiff neck or pain in the shoulders; this is followed by pain down the front of the arm, chiefly on the radial side, later, tingling or pins and needles in all fingers or fingers supplied by median nerve, most common in women and often associated with fibrositis elsewhere. Sufferers from gout or septic infection are predisposed to this type of brachialgia. The important diagnostic points are the condition of the spinati muscles, the mode of onset, and distribution of the pain and tingling.

The treatment is comparatively simple, and the much discussed question as to the value of massage answers itself. Deep massage to the arm is harmful, it only serves to irritate nerves already irritated. Massage applied to the spinati muscles in most cases cures the brachialgia. Where these muscles are too irritable to be massaged, as is generally the case in the early stages, cataphoresis or diathermy is valuable in preparing the muscles for massage. The final cure, however, is generally brought about by massage.

Summary.

(1) In spa practice more than half the patients complaining of brachialgia are suffering from fibrositis of the supraspinatus muscle or infraspinatus muscle.

(2) These muscles are especially exposed to trauma and to chills. Inflammation in these muscles will cause pressure effects on the suprascapular nerve, because the nerve lies on bone and the muscles it supplies are bounded by rigid walls.

(3) Pressure effects on the suprascapular nerve will account for a large proportion of cases of brachialgia.

DISCUSSION.

Dr. C. W. Buckley (President) said that Dr. Burt had brought an important and interesting subject before the Section, and that he agreed with most of his views on the subject. The wearing of furs was a very important cause of fibrositis of the shoulder; they caused undue warmth, which was relieved by throwing them back and the result was often a severe local chill. He thought that a definite extension of fibrositis from the muscles to the adjacent nerve trunks was more frequent than a pressure-effect on the suprascapular nerve and bore a closer analogy to sciatica. Teno-synovitis in the bicipital groove was a cause of brachialgia which was frequently overlooked. In one group of cases, pain referred to the inner end of the supraspinous

[1] Williamson, *Brit. Med. Journ.*, June, 1919.

fossa and tenderness at the same spot was a well-marked feature. He laid stress on the necessity for specific directions in ordering massage; the exact area to be treated and the kind of massage to be used, should be prescribed, or the results would be disappointing.

Dr. VINCENT COATES (Bath) said that in his opinion it was often possible to diagnose fibrositis of the supraspinatus muscle by finding fibrositic nodules. Treatment of these relieved the subjective symptoms in the arm and fingers.

With regard to the causation and frequency in women he considered that, arguing on the analogy of lumbar fibrositis, it would appear that damp played a part. In this latter condition the close approximation to the skin of garments moist with sweat to which no air had access, was an important point. In fibrositis of the neck and supra-scapular regions the turning up of a fur collar wet from rain produced an exactly similar state of affairs. It was interesting to note that in pathological conditions of the shoulder-joint one of the first muscles to waste was invariably the supraspinatus.

Dr. G. L. KERR PRINGLE referred to cases of brachialgia caused by some sub-luxation or very slight alteration in the position of one of the cervical vertebræ. These cases were frequently caused by hunting accidents and sometimes the alteration could be demonstrated by X-rays. He referred specially to one case in which the pain radiated down both arms and the X-ray showed a definite alteration in the alignment of the cervical vertebræ. After about half-a-dozen electric hot-air baths applied to the cervical region the patient experienced a slight "click," and a second X-ray showed that the subluxation was reduced. He was not prepared to say how far it was justifiable and prudent to attempt to reduce these slight displacements by manipulation.

Section of Balneology and Climatology.

President—Dr. CHARLES W. BUCKLEY.

The Biochemical Aspect of the Rôle of Uric Acid in Gout.[1]

By JOSEPH RACE (Buxton).

ALTHOUGH the presence of uric acid in gouty concretions was discovered by Wollaston in 1797, twenty-one years subsequent to the discovery of the acid by Scheele, the association of the acid with gout did not receive much attention until 1847 when Sir Alfred Garrod demonstrated the presence of sodium urate in the blood of gouty persons. This discovery led to much controversy as to whether the urates were the cause or the result of gout. The adherents of the former theory, in their turn, were in disagreement as to the manner in which the urates effected their deleterious action: some maintained that the urates produced a toxic action whilst still in solution in the blood and others that no action occurred until the urates had crystallized out from the blood and had become deposited in the affected tissues. That uric acid in solution produces no toxic effects, has been demonstrated on many occasions so that this theory will not be considered. A modified form of the other theory still has its advocates.

After the thread test, introduced by Garrod for the approximate determination of the amount of uric acid in blood, more refined processes were used and it was soon found that uric acid was present in normal blood but was in abnormal quantities in the blood of gouty subjects. Examination of the blood in other pathological conditions showed, however, that a hyperuricæmia is not confined to gout but is found also in leukæmia, polycythæmia, interstitial nephritis, and various febrile conditions.

These, and other results, led to the disparagement of the rôle played by uric acid in gout. Uric acid was regarded merely as incidental and other theories were developed.

With these other theories we are not concerned at the moment and I propose to confine my remarks to the subject indicated in the title, viz., the rôle of uric acid in gout.

Since 1904, when the chemical pathology of gout was discussed at the meeting of the British Medical Association, no general résumé of this subject has appeared and in view of the amount of research work on uric acid published since that date it was considered desirable that the subject should be reviewed. The introduction of micro-methods of analysis in the last decade has proved a stimulus to further work on uric acid in blood and urine, and many of the older conclusions have been shown to require reconsideration.

[1] Read at the Annual Provincial Meeting of the Section at Buxton, May 24, 1924.

The fundamental fact in connexion with the rôle of uric acid in gout is that, so far as is known, gout is the only clinical entity in which a deposition of urates in the tissues occurs. This is, of course. the tenth proposition of Garrod. Fink, of Vittel [1], has lately drawn attention to the fact that joint tophi contain considerably larger amounts of calcium carbonate and phosphate than uric acid, and he states that it is the lesions due to osteitis which bring about the formation of tophi. There is little doubt, however, that the view of Garrod and Luff is the correct one, viz., that tophi originally consist of sodium biurate and that the presence of calcium carbonate and phosphate is due to secondary deposits. The deposition of urates is a unique feature of typical gout, the only condition comparable to it being the so-called guanine gout of hogs in which free guanine is found in the concretions round the joints.

Another connexion between gout and uric acid is the hyperuricæmia that is a concomitant of the disease. Hyperuricæmia, as mentioned above, is also found in leukæmia, nephritis, and various febrile conditions and cannot, therefore, be regarded as indicative of the gouty state until the other conditions known to cause hyperuricæmia have been excluded. Evidence is accumulating that the condition of hyperuricæmia is more common than is generally supposed. Schamberg and Brown [2], using the Folin-Wu method of estimating uric acid, found that of 163 patients presenting themselves with various dermatoses, eighty-eight, or 53·6 per cent. had more than 3 mg. of uric acid per 100 c.c. of blood; fifty, or 30·5 per cent. had 4 mg. or over; eighteen, or 11 per cent. had 5 mg. or over; and seven, or 4·6 per cent. had 6 mg. or over. In a private communication Dr. Schamberg mentions that in a recent case of severe eczema, exhibiting no signs of gout, the patient showed 10·7 mg. of uric acid per 100 cc. of blood. Such results would appear to depreciate still further the value to be attached to the finding of hyperuricæmia but it must be remembered, as Schamberg and Brown [3] have pointed out that "the older writers, particularly among English and French dermatologists, laid considerable stress on the relationship of gout and eczema." Luff [4] indeed states that "amongst those who have inherited a gouty tendency the skin affections may constitute the only manifestation of gout."

The patients admitted to the Devonshire Hospital, Buxton, and suffering from chronic arthritis and allied diseases, form another class in which, judged by the usual standards, there is an abnormal percentage of hyperuricæmia. But here again there seems to be some bond of relationship with gout, and the differential diagnosis is far from being simple in some cases.

The results of the examination of a more normal hospital population are given by Feinblatt [5]. Of 1,500 patients, only fifty-seven, or 3·8 per cent. gave uric acid values in excess of 3·5 mg. without a concomitant retention of urea or creatinine; 20 mg. of urea nitrogen and 2 mg. of creatinine per 100 c.c. of blood being considered the commencement of pathological values.

Of the multitude of hypotheses that have been advanced at various times concerning the rôle of uric acid in gout, very few have survived modern criticism and of these only the following will be discussed: (1) that uric acid is merely incidental; (2) that gout is a form of nephritis; (3) that gout is due to excessive production of uric acid; (4) that gout is due to diminished destruction of uric acid.

(1) That Uric Acid is Merely Incidental.

The advocates of this negative hypothesis direct attention to the lack of toxicity shown by uric acid; to the various conditions mentioned above in which there is evidence of hyperuricæmia; and to the possibility that local

deposition may be secondary to inflammatory conditions. Uric acid is admittedly non-toxic in solution, but it must be remembered that an accumulation of this end-product of nuclein metabolism may exercise a general effect by retarding the rate of this form of metabolism. The uric acid may be incidental, but it is the only index of this process known at the present time. An analogous case is the use of the urea content of blood as an indication of the condition known as uræmia.

The absence of the symptoms of gout during the hyperuricæmia found in other pathological conditions is due probably to varying causes: in febrile diseases the condition is too transient and the blood circulation too rapid to permit of the deposition of urates; and in leukæmia and anæmia the increased proportion of plasma would tend to increase the saturation point for urates of the whole blood. The hyperuricæmia of nephritis will be considered later.

The question of local deposition of urates is admittedly exceedingly controversial and has been discussed so often that the older arguments for and against need not be repeated here. In connexion with the question as to whether deposition of urates precedes, or is subsequent to inflammation, it is necessary to remember that tophi in the helix of the ear, and in other parts of the body also, often develop without inflammation and pain. Inflammation is not, therefore, a *conditio sine qua non* for uratic deposition. Many observers have suggested that the inflammatory condition during an acute attack is due, not to uratic deposition, but to resorption of such deposits. It is stated that if an attack of gout were due to the precipitation of urates, the uric acid content of the blood would diminish and not increase as is usually found to happen: that the blood is not saturated and should therefore not deposit urates; that tophi almost always diminish with the onset of an attack and frequently disappear after a pronounced and violent attack; and that the blood during an attack is more alkaline and tends towards solution rather than deposition of urates (Fink [1]).

Whilst there is usually an increase in the uric-acid content of the blood during an attack of gout, this is due to a rise in the kidney threshold for this substance. If the kidney threshold remained constant and resorption occurred, the blood content would remain approximately level and there would be an increased excretion of uric acid in the urine. Most observers have found that the volume of urine and the uric acid output are reduced in the period immediately preceding an attack, and also during the early period of it. This is in accordance with the results obtained at this hospital during the past twelve months. The alteration in the threshold value of the kidneys invalidates any conclusions being drawn from either the amount of uric acid excreted or the uric acid content of the blood. This line of investigation offers little hope of producing a solution of this problem until the kidney threshold can be controlled, and even then it is probable that the amount of acid precipitated or dissolved would be within the limits of normal variations. The influence of the accompanying pyrexia introduces further complications.

Indirect evidence favours the precipitation theory. Uratic deposits occur in the helix of the ear and in the extremities of the body where the blood flow is more sluggish. If deposits tend to occur *where* the blood flow is most sluggish, they might also be expected to be most prone to occur *when* the flow is most sluggish, viz., during the early hours of the morning when the temperature of the body is at a minimum. Is it merely a coincidence that this is the period during which the attacks of gout develop, or is it that attacks are most liable to occur when conditions are most favourable for the deposition of urates?

The acidity of the urine is also at the maximum during the night, but the amount of uric acid excreted is at a minimum, which would indicate either minimum production or a raised threshold. There is some evidence, however, that other factors may be involved. Unpublished results obtained in this hospital show a marked increase in uric acid excretion immediately on rising in the morning as compared with the average of the night sample without an accompanying increase in the excretion of phosphoric acid. If the increase were due to general cellular activity resulting in greater nuclein metabolism, a parallel increase in both of these end-products might be anticipated. The absence of a phosphate increment appears to indicate that the uric-acid increase is due to the elimination of material stored in the body, but the whole problem of nuclein metabolism is so imperfectly understood that any conclusions drawn from the rate of excretion of its end-products may be accepted with reservations. The results referred to also indicate that there is no definite relation between the uric acid and phosphoric acid curves either during short fasting periods or after ingestion of food, and no satisfactory explanation of this apparent lack of correlation in the hourly rates of excretion can be offered at the moment.

The consideration of the degree of saturation of the blood with uric acid during an attack of gout does not lead to any definite conclusions. During such attacks the general venous blood is usually, though not invariably, below the point of saturation which, for stable solutions, is stated by Gudzent [11] to be 13 mg. of sodium urate ($C_5H_3N_4O_3NaH_2O$) per 100 c.c.; this is equivalent to 9·7 mg. of uric acid per 100 c.c., and accords with the results obtained in this hospital. Two samples of blood, when proper precautions were taken to prevent loss of carbonic acid, gave stable solutions at 37° C. containing sodium urate equivalent to 10·0 mg. and 10·3 mg. of uric acid per 100 c.c. of blood. Gudzent's finding, that unstable supersaturated solutions containing larger amounts can be obtained, was also confirmed.

Although several cases of tophaceous gout admitted to this hospital have yielded results between 10 mg. and 11 mg. of uric acid, corresponding to stable saturation, the average content in males is approximately 6 mg. or about 60 per cent. saturation. The blood is therefore usually unsaturated, but this does not preclude the possibility that local conditions may lower the point of saturation of the blood, or, more probably, of the lymph. An increase in the acidity due to lack of oxygen may be sufficient. Even local saturation may be unnecessary if the local conditions so alter the tissues as to disturb the equilibrium with the surrounding lymph. Gudzent [11] regards gout as a specific tissue disease (spezifische Gewebserkrankung) which leads to an anomalous distribution of uric acid, and he has found that the fluid obtained by joint puncture contains more urate than the blood; this amount may even exceed the quantity required to form stable saturated solutions.

An analogous case is the process of calcification and for the explanation of this the saturation of the whole blood supply is not considered essential. Blood samples taken in the usual manner from the cubital fossa represent the average systemic supply and therefore do not indicate in any way the extreme variations that might be produced locally. In the same way it is possible for a severe epidemic of some disease to occur in one area of the country without material effect upon the average morbidity rate for the whole country.

The reduced size of tophi sometimes observed after an acute attack of gout cannot be regarded as conclusive evidence of the resorption of urates being the cause of the attack, as the phenomenon might equally well be ascribed to the

pyrexia which accompanies it. The increased circulation of blood and lymph, together with the higher metabolic rate, would tend to solution rather than deposition.

The question of changes in the alkalinity of the blood during an attack requires reviewing with the aid of modern methods of analysis. The older methods are very unsatisfactory and the results obtained with them do not warrant any definite conclusions being made.

(2) That Gout is a Form of Nephritis.

According to this hypothesis the disease is caused by the retention of uric acid in excess due to renal inadequacy. This aspect was suggested many years ago and was stated very lucidly by Sir A. E. Garrod at the meeting of the British Medical Association in 1904 [6]. In more recent years attempts have been made, particularly in America, to associate gout with nephritis. Myers, Fine, and Lough [7] stated that

"The blood pictures in early interstitial nephritis and gout are strikingly similar, particularly as regards the increase in uric acid. In view of the other clinical symptoms in common, it would seem that this similarity must be more than accidental."

Fine [8] defined the problem very clearly in the following questions:

"(1) Is gout merely a stage in the development of interstitial nephritis, whose further progress may be indefinitely delayed? (2) Is early interstitial nephritis merely potential gout, in which the symptoms may or may not appear? (3) Is the uric acid retention of gout due to the specific condition gout or to a complicating early interstitial nephritis?"

In the biochemical aspect of this problem one feature stands out prominently, viz., owing to the insolubility of uric acid it might be anticipated that a reduced efficiency of the kidneys to excrete this substance would be the first symptom of a reduced function of these organs. This is in agreement with the results obtained and it is to be noted that this reduced efficiency takes the line of a raised threshold rather than a limitation of the amount of acid excreted. This accounts for the fact that, after the ingestion of uric acid, purin bases, or food rich in nuclein, a gouty person excretes the same quantity of acid as a normal one. Any difference observable is in the rate of elimination rather than in the total quantity. The raised threshold which results in the hyperuricæmia must, as mentioned above, lower the rate of nuclein metabolism by accumulation of the end-products of such metabolism. This would account for the retarded rate of elimination of ingested acid by gouty subjects and also for the reduced elimination of endogenous uric acid noted by various observers.

The fundamental question in the relationship of gout and nephritis is whether the uric acid excretory function of the kidney is an independent one or merely part of a general function. In other words, is the uric acid function capable of separate activity and stimulation? The only data relevant to this problem are rather inconclusive. The effect of the so-called uric acid eliminators such as phenyl-cinchoninic acid, sodium salicylate, and sodium benzoate, upon the hyperuricæmia of gout is well known; a marked reduction in the uric acid content of the blood occurs. Myers and Killian [9] and Fine and Chace [10], from a study of the effect of atophan and allied derivatives on nephritics, have obtained results which indicate that the uric-acid function is not a separate one and that these drugs also stimulate, to a lesser degree, the elimination of other metabolites. The blood-samples used by these observers were usually taken after about three days' medication and under these conditions similar results have been obtained in this hospital. After only one day's

treatment the results are rather different. The urea nitrogen is increased and the reduction in the uric acid content of the blood is much more marked in the plasma than in the corpuscles. The study of the rate of excretion of waste products by a normal person after a single dose (1 gram) of atophan or salicylate supports the hypothesis of a separate functional activity. In each case the increased rate of elimination of uric acid was accompanied by a diminished excretion of urea but it was observed that the latter was eliminated later in abnormally large amounts. The excretion of chlorides was somewhat irregular but that of creatinine remained fairly normal throughout the whole period. These results are not conclusive but they do seem to indicate that, at least in the normal individual, the uric acid and urea excretory functions may be separately stimulated.

Another problem awaiting solution is whether it is possible to produce gouty symptoms in a nephritic person by local damage to tissues or in some other manner.

Until these and other questions are answered, the relationship of gout to nephritis must remain indefinite.

(3) That Gout is due to Excessive Production of Uric Acid.

The uric acid produced in the body is usually regarded as being derived from two sources, one being the exogenous acid derived from ingested nuclein and purins, and the other, the endogenous variety, produced by the katabolism of the nuclein material of the body cells.

Whilst it is undoubtedly true that the kidneys have a much greater capacity for excreting uric acid than they are usually required to perform, there are no observations recorded as to the effect of continued passage of abnormal amounts. If continuous operation beyond the normal capacity once results in a raised threshold, the tendency will be towards a reduced capacity and further ingestion of an excess of purins will result in a gradually accentuated hyperuricæmia. A reduced purin ingestion leads to a corresponding reduction in the uricæmia both in gout and in nephritis, but in the recorded cases the new levels are still above the normal. In pneumonia, the leucolysis following the crisis causes excessive production of endogenous acid, but as this condition is only transient the threshold of the kidneys for uric acid is gradually lowered until normal values are re-established. In the more prolonged leucolysis of leucæmia, the threshold is probably permanently raised. Excessive general metabolism that includes nuclein metabolism should also tend, in chronic cases, to a higher uric acid threshold. Thus it might be anticipated that cases of exophthalmic goitre would show hyperuricæmia. This is quite hypothetical, but, if substantiated, might afford a rational explanation of those cases of gout in which there are general indications of an inborn error of metabolism.

Excessive production may occur in another manner. It is well known that only a fraction of ingested purins appears as uric acid in the urine, and it is not improbable that a portion of the amount lost is due to destruction by the bacteria in the intestines. Alterations in the bacterial flora, or in the absorptive mechanism, may lead to the absorption of a greater proportion of the ingested purins.

Excessive production of uric acid in intermediary metabolism might also be mentioned. Numerous observations have shown that exogenous purins are not essential for the production of nuclear material in the body, and it is therefore quite clear that the body possesses a mechanism for nuclein anabolism from non-purin materials. In this synthesis, uric acid may be an intermediary

product, and there is a possibility that a portion of it escapes conversion into nuclein. Such a process would account for excessive production, but it must be emphasized that it is quite hypothetical and lacks experimental verification.

(4) That Gout is due to Diminished Destruction of Uric Acid.

One method of purin destruction, by bacteria in the intestines, has already been mentioned. Whether uric acid or its precursors may be destroyed after absorption is still undetermined. The practical absence of allantoin from the urine of man, together with the failure to obtain uricase from his tissues, point to the absence of the uricoclastic property which is shown by most of the mammals. The very low purin coefficient of man suggests that the destruction takes another form, but for this there is no experimental support, and the whole question should be regarded as *sub judice*.

Relation of Gout to Endocrine Glands.—Although a possibility of some relationship between gout and the endocrine glands has been referred to indirectly under nuclein metabolism, it has not been specifically mentioned. It is obvious, however, that an inborn error of metabolism leading to excessive katabolism may be due to an endocrine disturbance. Later, when the excessive production of metabolites has raised the threshold of the kidneys, the tendency would be towards a gradual reversal of the balance, and excessive anabolism would result. This action of the kidneys would be a natural defence against the vicious circle mentioned by Langdon Brown [12] of "the sympathetic stimulating the thyroid, and the thyroid increasing the sympathetic response."

Diagram I, showing the relationship of the weight of the patients to blood uric acid, is not incompatible with this view although the results could doubtless be explained in other ways. Schamberg and Brown [2] have also noted a tendency for the blood uric acid to increase with weight in various dermatoses, and their results are included in the diagram for comparison.[1]

Another fact, indicating a possible endocrine origin in some cases, is that of the females admitted to this hospital during the past twelve months and diagnosed as gout, 97 per cent. were in the post-climacteric period of life. Mason [13] states that the menopause causes a complex syndrome in which signs of thyroid hyperfunction may be recognized. This would then initiate the cycle of events previously mentioned. If this hypothesis were correct, it might be anticipated that such a cycle would require several years for completion and that the greatest incidence of gout would be, not in the period immediately following the menopause, but several years later. An analysis of the cases in age-groups is shown in Diagram II, in which the similar groups for males are added for comparison.

It will be seen that the group 51-60 exceeds the total of all the other groups for females, a result that accords with the hypothesis suggested. It is hoped that, on some future occasion, a statistical study of a larger number of cases by biometrical methods may be presented.

In this paper, although emphasis has been laid on the rôle of uric acid as an indication of gout, and its possible connexion with the development of the disease, no suggestion has been made that uric acid is the primal cause of gout. The facts do not warrant any such suggestion although they do indicate that hyperuricæmia, or some condition invariably associated with it, is essential

[1] The uric acid figures of Schamberg and Brown, and those from this hospital are not strictly comparable. The former used the Folin-Wu method; the latter are obtained by Pucher's modification of Benedict's process. About 15 per cent. should be added to the former to make them strictly comparable with the latter.

DIAGRAM I
GOUT.

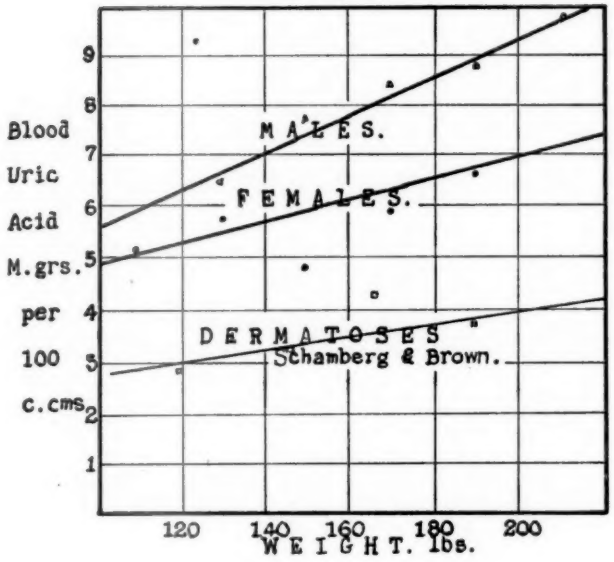
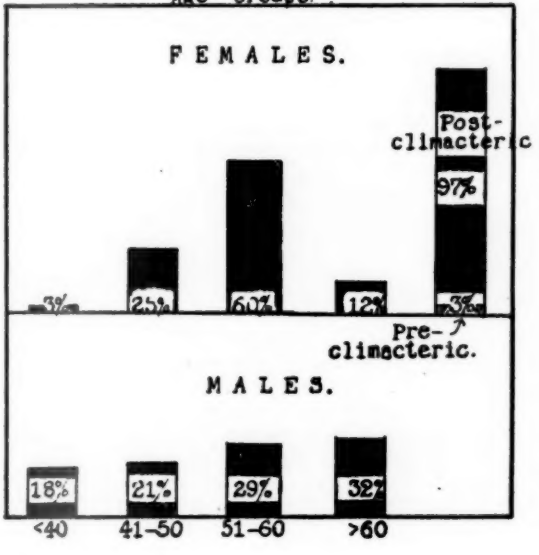


DIAGRAM II
GOUT INCIDENCE.
Age Groups.



for the development of gout. Deposition of urates is a concomitant of typical gout; in atypical gout no surface deposits are noted but they may occur in other parts of the body.

None of the various hypotheses that have been discussed offers a complete and satisfactory explanation of the facts and it is more than probable that more than one factor is involved. One great obstacle to progress in the study of gout is the lack of suitable animals for experimental work. The animals usually available are quite unsuitable because their uric acid metabolism differs so materially from that of man. The chimpanzee is suitable, but work with this simian type of primate could only be undertaken at an expense which no single institution in this country could possibly afford. Such work might be carried out most advantageously by the Government as part of an investigation into rheumatoid and allied diseases.

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DISCUSSION.

Dr. Buckley (President) said that Mr. Race had made a valuable contribution to the study of gout. Consideration of the various theories which had been so fully described would take up more time than was available but he hoped that some of those present who could speak with authority on different aspects of the disease and its chemistry would express their views. For his own part he believed that the theory of a variable kidney threshold furnished the most satisfactory explanation of the uric acid retention which was a feature of the disease. Whether it was the cause or not was a point which had yet finally to be settled.

Dr. George Pernet (London) spoke on the relations of gout and eczema to which Mr. Race had been alluding. He desired to point out that eczema was a term applied in a loose way to all sorts of morbid skin conditions, such as irritant dermatitis arising from various external causes, pityriasis and so forth, which had nothing to do with eczema. Dr. Pernet considered the name should be limited to an eruption, apparently spontaneous and of internal origin, characterized by numerous minute vesicles, which rapidly broke down, and were accompanied by serous exudation. The popular name of "weeping eczema" was an apt one. Gout also had come to cover many morbid conditions of a toxic nature. But if one limited the term "gout" to the old classic condition of podagra, then he (Dr. Pernet) must say that in his long experience, it was uncommon to see the association of true eczema, as defined, and classic gout.[1] Most patients with a skin rash, put the question: "Was it gouty?" and were disappointed if one said: "No!" They rather liked to think they were of gouty stock.[2]

Dr. Vincent Coates (Bath) said that Mr. Race had given an excellent résumé of the biochemistry of gout. He thought that the ultimate solutions of the abnormal metabolism obtaining in gout must come from biochemical researches. The necessity for careful clinical observation, however, could not be over-emphasized. It was

[1] See also Pernet, "The Alternation of Disease, especially from the point of view of the Skin," *Medical Press and Circular*, April 25, 1917, p. 348.

[2] An old tag concerning gout ran: *Dominus morborum et morbus dominorum.*

important for research purposes that only undoubted cases of gout should be investigated. It was his own practice not to classify a case as one of gout unless either tophi were present and a clear history of typical attacks elicited, or an acute attack was observed by him. He would like to be assured that the cases referred to by Mr. Race came into one of these categories. The theory that an acute attack of gout was an anaphylactic phenomenon was decidedly attractive in view of the close association of various dermatoses, asthma and urticaria, with gout, in which disorders eosinophilia might occur and in which protein sensitization was sometimes found to play a part. It might be that in some measure the site of election in which an anaphylactic reaction occurred varied with the susceptibility of the individual. In the instance of gout, presuming the condition to be one of anaphylaxis, this occurred in fibrous and other tissues poor in blood-supply.

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SESSION 1923-24

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SECTIONS OF STUDY OF DISEASE IN CHILDREN, NEUROLOGY, OBSTETRICS AND GYNÆCOLOGY, AND ORTHOPÆDICS.

(JOINT MEETING)

March 6, 1924.

DISCUSSION ON "BIRTH INJURIES, WITH SPECIAL REFERENCE TO INTRACRANIAL INJURIES WITH HÆMORRHAGE, AND TO NERVE INJURIES."

Dr. CUTHBERT LOCKYER (Chairman) (p. 1), Mr. EARDLEY HOLLAND (p. 2), Dr. H. C. CAMERON (p. 7), Dr. JAMES COLLIER (p. 9), Mr. H. A. T. FAIRBANK, D.S.O., M.S. (p. 13), Dr. THOMAS LUMSDEN (p. 15), Dr. GILBERT STRACHAN (p. 18), Dr. BERNARD MYERS (p. 19), Mr. D. MCCRAE AITKEN (p. 20), Mr. BRIGHT BANISTER (p. 21), Mr. EARDLEY HOLLAND (in reply) (p. 21), Dr. CAMERON (in reply) (p. 22), Dr. JAMES COLLIER (in reply to Mr. AITKEN) (p. 22).

The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

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Section for the Study of Disease in Children.

President—Mr. R. H. Anglin Whitelocke.

Case of Hemi-hypertrophy, Traced for Eighteen and a Half Years.

By J. P. Lockhart-Mummery, F.R.C.S.

The patient was first exhibited at the Society for the Study of Disease in Children in January, 1906, and this case was described by myself and the late Dr. George Carpenter[1] in the reports of the Society for that year.

The child was then 2½ years old, and the left side of his body and head was growing faster than the right side. He was originally brought to the hospital because he was walking lame. There was nothing else wrong with him; the mental condition was normal, and there was no abnormality detectable in the nervous system. The difference in length of the lower limbs was ½ in. The child was again exhibited at this Society in January, 1908.[2] At that time the left side had grown considerably more than the right, and the child walked very lame. He had to have a boot on the right leg 3½ in. higher than the left. X-ray photographs of the bones showed the epiphysis to be more advanced on the left side than the right. The growth on the left side of the face appeared to be two years in advance of that on the right, and the left side of the tongue, the left eye and left ear were all larger than the right.

The patient was lost sight of until the other day, when I came across him again at the age of 21 years. It was always a matter of interesting speculation as to whether the condition was one of hemi-hypertrophy or hemi-atrophy. I have always maintained, as did Dr. Carpenter, that it was hypertrophy, more particularly as the X-ray photographs seemed to show that the epiphyses on the smaller side were normal for a child of his age. So far as I am aware this is the only case that has been traced to adult life. The patient now shows only ½ in. difference between the two sides. We do not, unfortunately, know what the actual difference was at the age of 10 or 12 years, but at the age of 4½ years it was 3½ in. It is obvious that the small side is now catching up after the big side has ceased growing. The boy is to all intents and purposes now normal, and I have no doubt that all difference between the two sides will have disappeared in another couple of years.

The cause of the condition is a matter for speculation. There may be some centre in the brain which controls nutrition, and in these curious cases there

[1] *Rep. Soc. Study Dis. Children*, 1906, vi, p. 153.
[2] *Proceedings*, 1908, i (Clin. Sect.), p. 61.

may be same congenital abnormality or possibly an injury of one half of the centre.

The photograph of this child at the age of 4½ years is to be found in Sir James Purves-Stewart's book on "Diseases of the Nervous System," 1920, p. 290 (fig. 156).

DISCUSSION.

Mr. C. MAX PAGE asked whether Mr. Lockhart-Mummery knew of any case of hemi-hypertrophy in which the patient had reached adult life.

Dr. J. PORTER PARKINSON said this case was of very great interest, and he did not think it was likely that many had seen and had been able to observe a case of the kind for such a long time. Was the result seen in this case likely to occur in others as well? And it would be interesting, if the smaller side could be skiagraphed, to ascertain whether the epiphyses were likely to promise further growth. The smaller side had now practically caught up to the other, except in the face. The boy was right-handed, and had continued to be so, i.e., he had used the smaller side for his activities.

Mr. R. H. ANGLIN WHITELOCKE (President) said that he must express his gratitude to Mr. Lockhart-Mummery for having shown this case, which, as Dr. Parkinson remarked, was a rare condition to see at such an age. He (the President) had had an opportunity of seeing this patient when he was a child, and it was most interesting to have him under observation now that he was a young man, looking so well and fit.

Dr. F. PARKES WEBER said it was interesting that the late Dr. George Carpenter, who had a good deal of experience in abnormalities in children, remarked in January, 1906, when this case was shown, that he was not sure which was the abnormal side of the boy's body, whether it was the larger (left) side or the smaller (right) side. He (Dr. Parkes Weber) did not feel certain that the prognosis in cases of hemi-hypertrophy, in moderate degree, was bad. He thought there was need for more investigation before it would be possible to say whether the prognosis in a child with hypertrophy of one side, involving soft tissues and bones, without tumour formation, *quoad vitam*, was worse than in ordinary normal children.

Dr. DONALD PATERSON remarked that last year,[1] in conjunction with Mr. Reynolds, he had shown at the Section two cases of hemi-hypertrophy, and had quoted from an American observer, Arnold Gesell, who had collected forty cases up to 1921. Contrary to the statement just made by Dr. Parkes Weber, Gesell said that the right was the affected side in 70 per cent. of the cases. He also said that the condition was rarely seen in adults, though he did not say he had not seen an adult case; and he hinted that the difference between the two sides became less apparent as adult years approached.

Mr. J. P. LOCKHART-MUMMERY (in reply to Mr. Max Page) said there was a hemi-gigantism, but whether that was the same condition he did not know. In this case the interesting problem, to which he had hoped reference would be made, was the position of the lesion. He thought it must be a central nervous lesion. These hypertrophies were never crossed. Dr. Robert Hutchison had proved that the condition affected the internal organs because he showed a case in which, after the death of the patient at London Hospital, it was found post mortem that the organs were larger on the hypertrophied side than on the other. He would try to show the boy again when the hypertrophy had quite disappeared.

[1] *Proceedings*, 1922, xv (Sect. Study Dis. Child.), p. 50.

Case of Congenital Unilateral Dilatation of the Ureter, ending Fatally.

(Skiagram Shown.)

By F. J. POYNTON, M.D.

PATIENT, a boy, aged 6 years 10 months. He came into the hospital on December 21, 1922, with the history that he had had enuresis since birth. On examination, his bladder was found to be distended to the level of the umbilicus, and there was enuresis by day and night (d. 6-8, n. 3). A catheter was easily passed, and the urine was slowly evacuated. It was clear that the enuresis was incontinence with overflow. There were some grave symptoms, as he suffered from great thirst, and had frequent headaches. His blood-pressure was 135 mm. systolic, and his right kidney was easily felt. The blood urea measurement was 114 mgm. urea per 100 c.c. blood. The urine was alkaline, specific gravity 1006; there was a definite cloud of albumin and pus was present; sugar and acetone absent. He was in hospital for some time, and I asked Mr. Addison to see him. He agreed it was a risky case in which to try to do anything, because there was some urea retention and there was a tendency for the patient to become uræmic. The bladder improved under electricity treatment. He stayed out of hospital for some time, keeping a good deal better, and then he came back with a recurrence of the condition. Sodium bromide was introduced into the bladder, and a skiagram taken (now shown), and this revealed a greatly dilated left ureter at the saccular end of the bladder wall. Shortly afterwards he developed symptoms of uræmia, and died April 4, 1923. An autopsy was refused. The injection of sodium bromide may possibly be harmful, but of this I have no certain knowledge.

Case of Congenital Bilateral Hydronephrosis.

By F. J. POYNTON, M.D.

PATIENT, a girl, T. G., aged 10½ years. Admitted April, 1923, complaining of abdominal pain and sickness. The urine was found to contain many pus cells and the diagnosis was considered to be pyelitis, as the condition abated with the administration of potassium citrate, although the pus persisted to some extent on discharge. Cultures were made and the infection proved to be one of *Bacillus coli*.

Urea concentration test: 1·3 per cent., 6 a.m.; 0·95 per cent., 30 c.c., 7 a.m.; 1·35 per cent., 85 c.c., 8 a.m.

Blood urea, 68 mgm. per 100 c.c. blood.

Blood-pressure, 94 mm. Hg.

In August the pain and vomiting recurred and the child was readmitted with pus still present in the urine.

A cystoscopic examination of the bladder showed what was considered to be a diverticular orifice at the base rather to the right side. There was much trabeculation and sacculation; sodium bromide 20 per cent. was introduced into the bladder and an X-ray taken revealing the condition of bilateral

hydronephrosis. The patient has had no enuresis. She is still passing pus in the urine, and one does not know what to do for her.

This case is of a kind of which I have very little experience, and any hints as to treatment, and any further information will be of value. To those not well acquainted with such conditions in children, I may point out that enuresis with distended bladder is an unusual event, and that pyelitis, together with the condition of ureters revealed by the X-rays, is very instructive.

DISCUSSION.

Dr. FORDYCE asked for further details in the history. He thought a certain number of these cases, which were often given the name primary hypertrophy of the bladder, were connected with birth conditions, and showed nerve symptoms. He had wondered whether this girl's reflexes were normal. With regard to ætiology, he thought that in a certain number of cases one found there was an organic cause for such a condition, as was shown in this picture. But in a considerable number of cases no organic cause was found, and, as Shattock, and Still, and John Thomson showed many years ago, the condition was analogous to congenital stenosis of the pylorus. If that were so, one interest in the present case was its sex, for most of these cases seemed to have occurred in boys. What the significance of that was he did not know. Blum (in the *Wiener Mediziner Wochenschrift*, 1907, vol. xlvii, p. 2258) showed that in some instances of this condition there were found, post mortem, lesions in the sacral cord. Blum's theory was, that as a result of the birth difficulty there had been this lesion in the sacral cord, a spasmodic condition of the sphincter of the bladder resulting. He (Dr. Fordyce) did not think it was possible to suggest much in the way of treatment, except that he believed vaccines were useful; they helped to clear up the purulent condition.

Mr. MAX PAGE said that if these cases did not occur *in utero*, and were recognized early, division of the sphincter might prevent the development of the hydronephrosis. This would not be a severe or difficult operation, but it would lead to incontinence for a while. How far it would be justifiable on the same lines as when treating pyloric stenosis, he did not know.

Dr. F. PARKES WEBER asked whether, in regard to such hypertrophied urinary bladders in children, there had been proof of the spasmodic action of the sphincter of the bladder as a cause of the hypertrophy. He believed that in some cases it had been suggested that the hypertrophy of the urinary bladder was primary, and that the enlarged ureters were a result of obstruction at their orifices, due to the hypertrophy of the bladder-wall. Another theory in regard to the vesical hypertrophy was that it was analogous to the "megalo-colon" of Hirschsprung's disease and to congenital hypertrophic stenosis of the pylorus. The so-called congenital "mega-duodenum" or "megalo-duodenum" was a very rare condition, which might also be analogous.[1]

Dr. POYNTON (in reply) said that the girl was first in the hospital in May of this year (1923), and that there was a history of her having had some septic illness when she was a small child; but he could not say when pus began to appear in her urine. She now seemed to be wonderfully well except for the attacks he mentioned, but the outlook was surely very grave. He would have liked opinions as to the effect of the use of sodium bromide injections into the ureters as a help in the investigation of these cases. In this case it was of great value in showing up the condition plainly; but was it not dangerous to drive any substance up into the kidneys? The solution used was 20 per cent. strength. The boy, in the case first reported, succumbed shortly after the injection was given, but he had shown signs of uræmia at intervals long before that was done. Unfortunately, as already stated, a post-mortem examination was refused.

[1] Cf. the accounts of cases by Variot, R. Grégoire, Variot and Caillaud, and Roberts. The condition of "megalœsophagus" ("idiopathic dilatation of the œsophagus") was also possibly analogous. There was likewise a good deal of literature on primary congenital dilatation of ureters ("megalo-ureter").

Section for the Study of Disease in Children.

President—Mr. R. H. Anglin Whitelocke, F.R.C.S.

Specimen of Hair-ball of the Stomach.

By J. D. Rolleston, M.D.

The specimen is from a girl, aged 15, whom I never saw in life. She was an epileptic idiot at an asylum of which she had been an inmate for five years before her death in 1917. During her stay in the asylum she remained in good health apart from an attack of pneumonia in 1913 until a week before death, when she began to suffer from vomiting and diarrhœa. On examination a smooth tender swelling was found in the epigastrium rising and falling with respiration, and was taken to be the enlarged right lobe of the liver. The

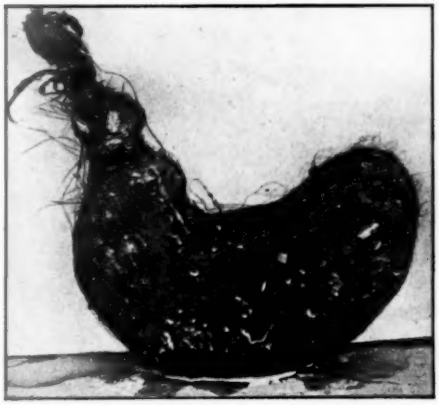


Fig. 1.—Hair-ball of stomach.

vomiting continued; on one occasion there was some hæmatemesis, and death took place on July 6, 1917. At the necropsy the stomach was found to be filled with a collection of hair of exactly the shape of the stomach, of which it formed a complete cast (fig. 1). It weighed 9 oz. and measured in its long axis 5 in., in circumference 6½ in., and along its greater curvature 10 in. The gastric mucous membrane was congested and showed several suppurating points. No hair masses were found in the intestine.

On inquiry from the mother it was learnt that the patient had been in the habit of eating her hair since she was a baby. No note of this habit had been made during her stay in the asylum.

The condition of hair-ball of the stomach, sometimes known as trichobezoar, is rare. No specimen of the kind has previously been shown at this Section or at the old Society for the Study of Disease in Children, but cases have been reported at the Clinical Section by G. F. Still [1] (1908) and Ivor J. Davies [2] (1921) and at the Electro-therapeutic Section by A. E. Barclay [3] (1915) and R. Morton [4] (1917). Davies, in whose paper a bibliography will be found, collected one hundred and eight cases since the first one recorded by Baudamant, [5] whose original description with engravings of hair-balls in the stomach, duodenum and jejunum I have brought from the library to show you (fig. 2).

Hair-balls in domestic animals, of which a fine collection may be seen in the Museum of the Royal College of Surgeons, were in former times supposed to possess antidotal properties against poisons, and were treasured as amulets

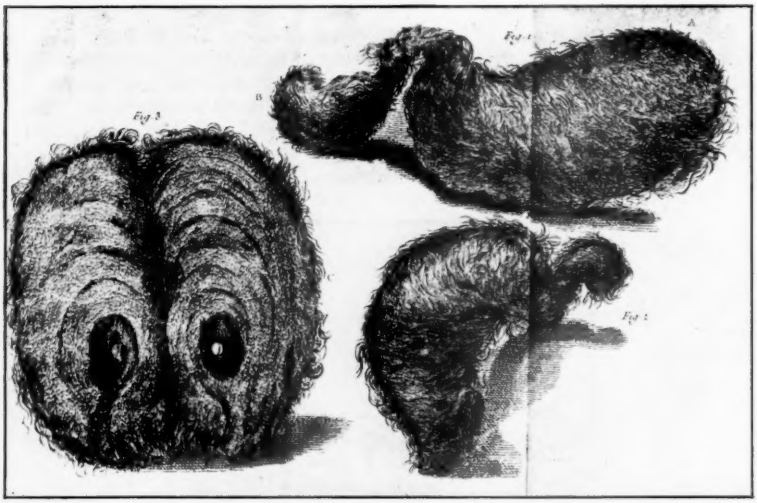


FIG. 2.—First recorded case of hair-ball of stomach (Baudamant).

i, (*a*) Hair-ball in stomach, (*b*) part which had passed through pylorus. ii, Second hair-ball in duodenum and jejunum. iii, Section of hair-ball in stomach, showing cherry-stone round which hairs had grown.

to protect the owner against plague, malignant fevers, epilepsy and other diseases.

The present case is typical in several respects. In the first place the habit had existed since infancy, as in 75 per cent. of the cases on record in which a history was obtainable (Butterworth). [6] Secondly, the symptoms were indefinite, and the patient's nutrition remained good until shortly before death, when wasting was noticed. Thirdly, the case was not diagnosed during life. In a recent paper by Monrad, [7] of Copenhagen, it is stated that only seventeen cases have been diagnosed during life—twelve in adults and five in children, including that reported by Still—and three only before operation. Hair-ball of the stomach has been mistaken for a variety of conditions, such as lympho-sarcoma, tuberculosis, tumour of the spleen, tumour of the kidney, sarcoma

of the ileum, mesenteric cyst, hydatid cyst of the liver, &c. The X-ray appearance, however, is so distinctive—the hair-ball appearing as a lighter area in the midst of the dark shadow of the bismuth—that, as Davies has pointed out, a correct diagnosis should now be the rule instead of the exception.

I would emphasize how interesting these cases of hair-ball of the stomach are, not only to the physician, the surgeon, the radiologist and the pathologist, but also to the medical historian. In the first case recorded by Baudamant the history of the patient was typical; he died at the age of 16, but, from his earliest infancy, the writer says, he had the deplorable habit of eating his own hair and that of his brothers and sisters; he even got up in the night, and took their hair from the dustbin and devoured it. He kept in fairly good health until he was 10 years of age, when he began to suffer from the effects of his bad habit.

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Three hair-balls removed from stomach (Mr. Fairbank's case).
The penny is inserted for comparison of size. The portions of the divided hair-ball described in text are shown at the bottom and right-hand side.

Mr. H. A. T. FAIRBANK showed a photograph of three hair-balls—one of which had been divided—removed from the ileum, just above the ileo-cæcal valve (see fig., p. 7). The patient was a girl 10 years of age, who came into Great Ormond Street Children's Hospital four years ago complaining of symptoms which had been persisting for five days only. There was pain in the abdomen, and she had been sick several times. She had rigidity in the right iliac region, and therefore it was thought likely she had appendix trouble. Her temperature was normal, pulse 104. A few hours after the examination she became worse, the pulse-rate increased to 124, and there was a slight rise in temperature. Some distended coils of ileum were noted, and therefore operation was done in the right iliac fossa region. The hair concretions were found and removed, and the patient got well. Though not an intelligent child, she could not be described as an idiot. It was really a case of subacute intestinal obstruction.

Case of Lymphangioma of the Foot.

By W. E. ROBINSON, M.D.

CHILD, aged 7 months. Brought to the Infants' Hospital, Vincent Square, for screaming. On investigation was found to be suffering from indigestion.

The left foot was noticed to be much overgrown in the region of the metatarsal bones and terminal phalanges. An X-ray examination, however, showed the bones to be perfectly normal. The ankle was not enlarged. After two months the foot was noticed to be getting larger.

This case is shown for suggestions as to its treatment.

DISCUSSION.

Mr. MAX PAGE said he felt sure that, ultimately, amputation of the foot would be necessary. A local operation for the purpose of dissecting out the lymphangioma, if such it were, would be out of the question; at least it would not yield a satisfactory result, and meantime it would cost untold sums to buy odd boots. He had seen a case of the kind affecting both feet, and in that case a double Syme's amputation had been done. That sounded drastic treatment, but if this were done early the child would obtain good use of the legs.

Mr. H. A. T. FAIRBANK said he was not convinced that this was lymphangioma, and asked what were the reasons for that view. There was hypertrophy of all the tissues, and he (the speaker) would have thought it included the bones, though the skiagram seemed to show the bones as normal. He did not see what there was to remove, except a portion of the foot in order to reduce the size of it. If it became unwieldy, a Syme's amputation would give the best result.

Mr. R. H. ANGLIN WHITELOCKE (President) agreed with the view that the case was one of hypertrophy, because the foot had grown in length, and he thought the bones must have increased in length too, even if they had not in width. He had not had much experience of these cases, but the feet tended to increase materially in size. He remembered one case for which a Syme's amputation was done, the patient being 11 or 12 years of age; and when seen the second time he was walking very well. He thought such cases were very rare.

Case of Precocious Puberty.

Shown for ROBERT HUTCHISON, M.D., by F. J. BENJAMIN.

THE child, aged 3½ years, commenced menstruation at the age of 17 months, and had continued menstruating at more or less regular intervals of one month up to the present date. The breasts were well formed and hair was present on the pubic region. Weight 52 lb. Height 44 inches. Skiagram showed bony

development of a child 13 to 14 years of age: in contrast to this, dentition was in every way normal.

Except for being extremely obstinate and very subject to fits of bad temper she was mentally normal and bright for her age.

Previous and family history revealed nothing unusual, and physical examination, apart from the excessive development mentioned, was quite negative.

The condition was thought to be due to ovarian hypersecretion either from tumour formation or simple hyperplasia. In either case the correct treatment was considered to be laparotomy and unilateral ovariotomy. Unfortunately this procedure had been refused by the parents.

Dr. F. PARKES WEBER, referring to the "pineal syndrome," which had also been called "macro-genito-somia præcox," remarked that a recent author found that such cases, which were first described by Marburg about 1907, had not been definitely proved to be due to tumour or other abnormalities of the pineal body.[1] That lent some support to what Dr. Benjamin had said. He wondered whether it would be possible to ascertain if a tumour of one of the ovaries existed in this child, either by the help of examination under an anæsthetic or by the help of artificial pneumo-peritoneum.

Case of Progressive Neural Muscular Atrophy (Peroneal or Charcot-Marie-Tooth type) in which the Upper Limbs are also affected.

By C. WORSTER-DROUGHT, M.D.

J. S., GIRL, aged 13 years. No family history of the disorder can be traced; the patient has one brother, aged 11 years, who is quite healthy; one other brother died at the age of 12, five years ago, from appendicitis.

Patient walked normally until the age of 2½ years; it was then noticed that she began to walk with her toes turned in and that she was always falling down. During the past four years the feet have become increasingly deformed, and the hands and arms have wasted considerably.

Present condition: Generally very thin and rather nervous; speech normal, quite intelligent; restless—constantly making wriggling movements of feet and legs, which consist chiefly of flexion and extension of knees and to some extent of wrists and fingers.

C.N.S.—Cranial nerves: Pupils react normally. No nystagmus. Lower lip pulled slightly more down to right side than to left when showing teeth, but no signs of facial palsy. Tongue protruded centrally.

Upper extremities: Tendency to bilateral *main-en-griffe*. Long thin fingers with coarse tremors. Considerable wasting of interossei, hypothenar and thenar eminences, especially abductor pollicis, in both upper limbs. All arm-jerks absent.

Lower extremities: Very pronounced talipes equino-varus of both feet. Marked weakness of quadriceps extensors on both sides. The peronei are atrophied and in a state of paresis; the tibialis anticus and externus longus hallucis of both sides have fair power. The remaining extensors of the toes

1 Cf. F. K. Walter, "Weitere Untersuchungen zur Pathologie und Physiologie der Zirbeldrüse," *Zeitschr. f. d. ges. Neur. u. Psychiatrie*, Berlin, 1923, lxxxiii, pp. 411-463.

are inactive. The tibialis posticus and gastrocnemius of each side contract voluntarily, the latter feebly. The great toes are hyperextended at the metatarso-phalangeal joints and flexed at the interphalangeal with considerable wasting of intrinsic muscles of foot. Glutei active. Marked scoliosis to right in lower dorsal and upper lumbar regions. Knee-jerks and ankle-jerks absent; plantar reflexes flexor.

Sensation: No sensory loss can be detected in either the upper or lower limbs.

Electrical reactions: To faradism—reaction absent in all interossei of hands excepting 1 and 2 (very slight); in flexors and extensor of wrist very sluggish. Biceps and triceps fair but more sluggish in left than right. Legs: All faradic reactions absent in muscles below knees, with exception of faint reaction in gastrocnemius and tibialis anticus. To galvanism—sluggish with A.C.C. greater than K.C.C. in intrinsic muscles of hands; fair reaction with K.C.C. greater than A.C.C. in other upper limb muscles. No reaction obtainable in small muscles of feet; use of muscles, fair reaction with K.C.C. greater than A.C.C.; anterior tibial group sluggish with K.C.C. greater than A.C.C.

The case has been treated by manipulating the feet into correct position and putting them up in plaster, followed by the wearing of suitable boots with a double support.

It is difficult at the present time to draw a hard-and-fast line between certain neural atrophies, nuclear atrophies and muscular dystrophies. The peroneal type of Sainton and the arm type of Haenel appear to merge into the type illustrated by this case—the Charcot-Marie-Tooth type—and may possibly be early or aberrant forms of the same disease. Heredity is the chief ætiological factor recognized, but in this case no other example can be traced in the family. Sensory changes are said to be present in the majority of examples of this type; in this case no anæsthesia or analgesia can be demonstrated.

Pathologically, in most of the relatively few cases that have come to autopsy, degenerative changes have not only been found in the muscles and peripheral nerves, but also in the spinal ganglia, anterior horn cells and posterior and lateral columns of the spinal cord.

DISCUSSION.

Dr. N. HOBHOUSE said that, being interested in this type of muscular atrophy, he had intended to show a case of the kind, but the patient had been taken ill. In his case there were no features suggestive of poliomyelitis, except that there was a definite diminution in the size of the femur on the side in which the chief atrophy occurred. He had neither seen nor read of a case of chronic neural atrophy in which there was a diminution in the size of bone; could Dr. Worster-Drought give him any information on the subject?

Dr. WORSTER-DROUGHT (in reply) said that bony dystrophies were not infrequent in various myopathies; the long bones often became thinner and those of the hands and feet shortened. As degenerative changes were found in the anterior horn cells of the spinal cord—in addition to the muscles and peripheral nerves—in cases of progressive neural muscular atrophy, the bone atrophy mentioned by Dr. Hobhouse was probably analogous to that met with in old cases of anterior poliomyelitis.

Specimen from a Case of Paroxysmal Tachycardia in an Infant, aged 9 weeks.

By NORAH H. SCHUSTER, M.B., and DONALD PATERSON, M.B.

HEART specimen from a female child, aged 8½ weeks, admitted to the Infants' Hospital, Vincent Square, October, 1923. The infant had been slightly cyanosed and dyspnœic since birth. These symptoms had become more severe two days before admission and there had been vomiting the previous day with indications of abdominal pain.

The infant weighed 8¼ lb., having lost ½ lb. since birth, though it took and appeared to digest its food normally until it was 8 weeks old.

When first seen it presented a wasted, feeble, and cyanosed appearance with continuous dyspnœa and a pulse of 200 per minute. The area of cardiac dullness was increased beyond the right border of the sternum and outward on the left side. The liver was enlarged.

During the two days in hospital there were four definite paroxysmal attacks lasting about ten minutes, during which the infant had marked dyspnœa, increased cyanosis of the face and extremities, and was in obvious distress. The pulse on these occasions was always uncountable but immediately afterwards it was noted to be 180 to 200. An hourly chart shows that between the attacks the pulse never fell below 140 and three times it quickened suddenly to 180 or 190. The temperature was irregular and varied between 98·4 and 102° F. Respiration was usually 60 per minute. Digitalis was given but it was discontinued as it seemed to cause vomiting and had no effect on the pulse-rate. Finally there was a severe and prolonged attack ending in death.

At the post-mortem the heart presented the following features now demonstrated: The left ventricle is hypertrophied and dilated, the muscle being in good condition. It communicates with the left auricle by a normal mitral valve, and from it arises the *pulmonary* artery, with its normal orifice and valves and branching to right and left lung in the usual way. There is a widely patent ductus arteriosus of nearly the same calibre as the pulmonary vessel and the aorta.

The right ventricle is of relatively small capacity and the muscle slightly hypertrophied. From it arises the aorta with normal valves and large vessels. It communicates with the left ventricle by a deficiency in the septum, the upper border of which is covered by a cusp of the tricuspid valve. The right auricle is dilated and thin walled and communicates with the left auricle by a partially closed foramen ovale. The left auricle is small and receives the pulmonary veins. Signs of chronic passive congestion are present in the liver and spleen. The lungs are well aërated and not noticeably anæmic or congested.

DISCUSSION.

Dr. G. A. SUTHERLAND said that his interest in this case, unlike that of the exhibitors, was in the clinical happenings during life, and the value of the post-mortem findings consisted in the extent to which they shed light on those happenings. There was, first, that great abnormality—the transposition of the aorta and pulmonary artery; but there seemed to be many foramina left, the foramen ovale, the patent ventricular septum, and the patent ductus arteriosus. Thus the question of the circulation became very involved.

With regard to the condition during life, attacks of paroxysmal tachycardia at that age were very rare, and very little was known as to their origin. The last case of the kind he remembered seeing was that of a child 3 years of age, who was sent to hospital on account of attacks which were diagnosed as *petit mal:* but when the house physician observed them he found they were very similar to the attacks described in this present case. The attacks lasted a few minutes, and the rate would fall from 180 to 60.

Respecting the pathology of the attacks, a little light was thrown upon it when, upon closer examination of the heart, it had been discovered that there was an aneurysm present, the position of which was considered to be on the pulmonary artery, or the ductus arteriosus. After death it had been found that there was an aneurysm of the pulmonary artery. In the light of that event, he (Dr. Sutherland) was inclined to attribute the attacks of paroxysmal tachycardia to the pressure from the aneurysm. It was difficult to explain the origin of the tachycardia in the present case or to base it on the myocardial changes described. All through life cardiac degeneration was very common, yet this type of paroxysmal tachycardia was very rare. Therefore the mere presence of degenerate myocardial changes was not sufficient evidence. Possibly there might have been some pressure in this case which had led to the tachycardia. He did not think that such a case had previously been described before the Society.

Dr. F. PARKES WEBER suggested that this case was of the kind formerly described in France under the name "congenital paroxysmal cyanosis" (Variot, Sébilleau, &c.). Paroxysmal cyanosis had accompanied the paroxysmal tachycardia in the present case, and paroxysmal tachycardia accompanied the cyanosis in "congenital paroxysmal cyanosis." Sometimes a cardiac malformation more or less similar to that in Dr. Schuster's case had been found post mortem in Variot's cases of "congenital paroxysmal cyanosis."[1]

Dr. DONALD PATERSON (in reply) said that these cases, although rare in medical records, nevertheless might not actually be so rare as was supposed. Collapses in children suffering from congenital heart disease were not extremely rare and if the pulse could be taken before and during these attacks as it was in the reported case it might be found that these were really attacks of "tachycardia." Had not the pulse been taken hourly in this case its true nature and the interesting fact that these were really attacks of paroxysmal tachycardia would undoubtedly have been missed.

[1] Cf. the elaborate Paris thesis by J. Sébilleau, "De la cyanose congénitale paroxystique," Paris, 1904, with illustrations.

Section for the Study of Disease in Children.

President—Mr. R. H. Anglin Whitelocke, F.R.C.S.

Case of Chronic Nephritis simulating Diabetes Insipidus in a Child.

By Geoffrey Evans, M.D.

I. C., schoolgirl, aged 13 years, was brought to the out-patient department of St. Bartholomew's Hospital by her mother on account of thirst and polyuria. Shortly afterwards she was admitted to President Ward.

History: Good health until four years ago, when her mother noticed that she was not growing, and the child developed unusual thirst. This thirst came on gradually, and has continued since. At that time she was drinking about eight cups of tea and eight glasses of water in twenty-four hours. Frequency of micturition D. 7-8, N. 3-4. Two years ago the school doctor observed that the child was losing weight; she was taken to a general hospital, and according to the mother the case was diagnosed as diabetes. Since then her health has been good. She is in Standard VI, and joins in games with other girls of her age. Her only symptoms have been occasional morning headaches, in the left frontal area, which pass off during the day, in addition to the polyuria and thirst already referred to. Appetite has been fair, digestion good, except for occasional pain in the umbilical region during the past month; bowels open regularly with saline. There has been no dyspnœa, no puffiness of the face or legs, no visual disturbance.

Past history: None of scarlet fever or sore throat. History of head injury on three separate occasions, not attended with loss of consciousness. Measles and whooping cough in infancy.

Family history: Nothing of importance.

On admission, November 14, 1923: General condition good, skeleton normal, body small for her age, weight 58 lb. No puffiness of the face or limbs; complexion clear, but pale. Teeth, tongue, fauces and neck showed nothing abnormal. Cardio-vascular system: Heart normal except for a soft systolic murmur at the apex of doubtful significance, and heart sounds at both apex and base rather louder than normal. Pulse 100 on admission, became steady about 88, regular; volume and character about normal; perhaps a little hard. Blood-pressure 96-54, and later 115-75 mm. Hg. Radial artery palpable. Retinal vessels normal. No change in the fundus oculi. Kidneys: Not palpable, and no renal tenderness. Urine: Quantity in twenty-four hours varied between 2 and 3 litres. Specific gravity 1003. Trace of albumin. No sugar. Very pale in colour. Deposit from a catheter specimen showed a few red blood cells and white blood cells, no casts and no crystals. In another specimen renal epithelial cells were seen.

The following special examinations were carried out:—

Radiogram of pituitary fossa; report states that pituitary fossa appears normal in outline and size; the sphenoidal and frontal sinuses appear quite undeveloped. Blood examination: Wassermann reaction and sigma negative; urea, 200 mgm. per cent. (Dr. Graham); red blood cells, 3,830,000; hæmoglobin, 44 per cent.; colour index, 0·58; white blood cells, 10,400; differential count: polymorphs, 56 per cent.; lymphocytes, 32 per cent.; large mononuclears 11 per cent.; basophils, 1 per cent.; eosinophils —. MacLean's urea concentration test showed gross impairment of renal function; after 15 grm. urea the percentage of urea was less than 1 per cent., and only reached 1 grm. in the first hour and 0·9 grm. in the second hour.

Diagnosis: Chronic nephritis, probably without arterio-sclerosis; uræmia. Ætiology, ? congenital kidney defect.

The diagnosis of chronic nephritis as distinct from diabetes insipidus was made on the presence of a trace of albumin in the urine and the presence of cells in the urinary deposit, the retention of urea in the blood and the evidence of failure of renal function, as shown also by the urea concentration test. The patient's symptoms, which are those commonly seen in chronic uræmia, supported the diagnosis. The next point of interest is the absence of any indubitable signs of arterio-sclerosis; thus, the retinal vessels are normal, the blood-pressure, condition of the palpable arteries and the size of the heart (as estimated by clinical examination) vary little from the normal. Different types of chronic nephritis in children are not as yet clearly defined, but the present case is in contrast in its cardio-vascular aspect to the group of cases in which there is marked cardiac hypertrophy and persistently raised blood-pressure. Whereas in the present case there is probably little arterio-sclerosis, in the cases with marked cardiac hypertrophy there is diffuse hyperplastic sclerosis.[1] The cause of the disease in the present case is obscure. The clinical history of the case is simply that of the gradual onset of failure of renal function, and there is no history of nephritis or of disease commonly complicated by nephritis. It is suggested that there is a congenital renal defect in this patient, since congenital abnormality of the kidneys is not uncommonly found in cases of chronic nephritis in children.

Some Suggestions concerning the Medical Statistics of Children.

By Bernard Myers, C.M.G., M.D.

An eminent physician lately spoke at a Section of the Royal Society of Medicine and expressed the view that medical statistics were unreliable and of not much importance. It is quite a commonplace saying that anything can be proved by statistics; if such were the case there would be no necessity to waste time in an endeavour to prove their value, but the point of misunderstanding is probably due to a difference in the hypothesis from which we start, the accuracy of our methods and the logic and efficiency of our deductions.

A statistician should be absolutely honest, fair, accurate and painstaking in his work; he should clearly understand the subject under consideration, have a

[1] Geoffrey Evans, *Quart. Journ. Med.*, Oxford, 1922, xvi, p. 33.

discerning mind, an efficient and obvious method, be capable of logical judgment and deduction, and present his facts simply, briefly, lucidly, and, if possible, in tabular form, allowing those interested to see without much effort the lesson taught or the comparison made.

Statistics may be used to test an hypothesis, or to estimate and compare the value of certain facts. Qualitative comparisons, so to speak, can be made and also quantitative comparisons of various physiological and pathological conditions in children. Computations may be made of the comparison of symptoms and also signs in diseases, or again in respect of one special factor only. It is of the greatest importance where we are dealing with the statistics of diseases such as pulmonary tuberculosis, rickets, diabetes, kidney disease, marasmus, &c., that we agree to accept the presence of certain definite symptoms and signs as clear indication of the disease in question in the body, and likewise those denoting its activity and progress.

It would probably give the best results if one individual personally examined all the cases which were to enter into the series of statistics, provided that he proceed upon such sound lines as would be approved by those most competent to judge. In the case, however, of a large number of children it would be impossible for one man to examine them all in the time likely to be at his disposal and, therefore, for the sake of accuracy it is practically essential for those who wish to do similar work to meet, discuss and agree upon the exact details of the method of procedure. Such an assembly, in the case of those interested in the statistics of diseases of children, should prove most helpful. This would apply not only to hospitals and clinics, but also to school medical departments and welfare centres. Certainly under these conditions the scope and value of statistics would be enhanced.

Under the physiological heading would be placed such statistics as are based on carefully ascertaining the height, weight, and various measurements, &c., at different ages in apparently normal individuals or those suffering from only slight deviations from health.

At the Children's Clinic, Marylebone Road, statistics are being prepared on a series of children; some from the physiological and others from the pathological point of view. Each child is measured and weighed when first examined and at definite intervals. The symptoms from which the child is suffering, the history of the present illness, the previous history from birth onwards, including instrumental labour, appearance at birth, diet, infectious diseases, accidents and operations, family ailments, idiosyncrasies, &c., are carefully noted. The course of the disease in the child and the effect of treatment are of special interest. The first statistics embracing a period of three and a half years are now being compiled.

Take such a common complaint in children as worms, and ask a dozen medical men their opinion as to the varieties, the method of introduction into the body, the frequency, age when first noted, the likely duration if treated and untreated, the local and general symptoms, the method of treatment and the presence of similar worms in other members of the family, and I think that we are likely to be a little surprised at the diversity of opinions expressed. Some, for instance, being to the effect that worms are comparatively infrequent while others believe them to occur in the majority of children. As a matter of fact we have found at the Children's Clinic, where an endeavour is made to find out in the case of each child if he is suffering from worms, that in from 20 to 35 per cent. of the children worms can be demonstrated. The question as to which symptoms, if any, are due to worms, such as thread worms, often

becomes a vexed one, but the figures, from observations of large numbers of children, prove that there are in the great majority of cases definite symptoms of a local character, and not infrequently also more general symptoms due to toxic absorption affecting the general health. The opinion is sometimes expressed that thread worms, if left untreated, tend to die out in time; this has certainly not been our experience, and we have known a case recently of a woman of 45 who showed an appendix full of thread worms at an operation, and another woman of over 70 years of age who has suffered from round worms from early childhood until the present time. In each case the person affected had a toxic appearance. Statistics based upon accurate observations on each patient alone can answer most of these questions correctly.

For some time we have endeavoured to make careful notes of the earliest symptoms of tuberculosis, and our statistics suggest that in practically all such cases the first symptom to be complained of was a feeling of listlessness. Fatigue seems to be present earlier or later in the day, according to the severity of the case, while activity remains. Again it is the first symptom to disappear as the patient recovers his health. Before, however, this statement could be finally accepted many more cases would have to be examined and placed among the statistical records.

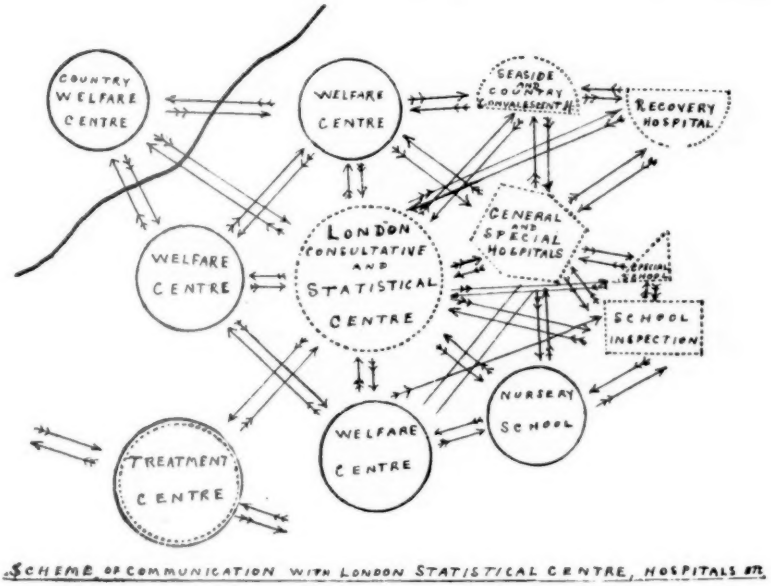
The nervous child is always of interest to the pædiatrician, if sometimes a cause of worry. We see the type very often in hospital and private practice. In a recent series of statistics at the Children's Clinic the nervous child constituted 10 per cent. of the children attending during a period of two years. This figure may be qualified by larger numbers. Statistics help us to elucidate the frequency of the various symptoms, the importance of heredity, the effect of different methods of treatment, and the prognosis. All these points are fairly important if we are to build up an A1 nation.

These examples will be sufficient for our purpose.

Let us leave the subject of statistics in connexion with institutions like hospitals and clinics and see if it would be possible to make systematic use of the enormous mass of material available in large towns—at welfare centres, school clinics, treatment centres and hospitals, by preparing a card for each individual child indicative of all the important ailments from which he might suffer from birth to 14 years of age. If such could be done the information so obtained, rendered into statistics and properly tabulated, would be of great interest and most instructive to medical men, and often of distinct value in the treatment of the individual if he were admitted to hospital either during childhood or afterwards. It would be a medical record, available for the hospital, of important information otherwise probably not obtainable. Not only would normal progress and the effect of disease in the individual be recorded, but as statistics, the arrangement of important facts with regard to age-periods, sex, the cause of particular diseases, treatment, the comparison of similar diseases in different localities under varying home conditions would be available.

May I specially lay stress upon the necessity of particularly watching the early symptoms of disease, the course and the effect of treatment. To do this on a large scale would certainly entail efficient organization and a large amount of work, but still I think we shall agree that it is quite possible of attainment. This can be seen by means of a diagram which explains itself.[1]

[1] *Postscript.*—Dr. Poynton (see remarks in Discussion) thought that a recovery hospital ought to be introduced into the scheme represented in the diagram. This has now been inserted. —B. M.



Scheme of intercommunication with statistical centre.

A 1150

HARRY BROWN,
Son of CHARLES BROWN,
6, Lisson Grove, N.W.

Born 3/3/23 M.

1/1/24. *70.* *Welfare Centre, Salisbury Street, London, N.W.*
B.W. 7 lb. 2 oz. Breast-fed 3 months, then milk-mixture.
Nutrition good. Cut teeth 9 months.

4/5/25. *81.* *St. Mary's Hospital. Bronchitis*—recovery.

6/9/25. *2842.* *N.W. London Fever Hospital. Sc. F.*
Recovery complete; no ill-effects left.

10/1/28. *2060.* School-Inspection at *Holmes Road*, Chalk Farm.
Infantile Paralysis, left foot and leg.

5/2/28. *1101.* *Royal National Orthopædic Hospital*, Great Portland Street, London.
Tendon-Transplantation. Condition greatly improved, and walks well with instrument.

Note.—A 1150 indicates the central registry number while 70, 81, &c., are the child's numbers of the hospitals, &c., sending nominal rolls to the statistical centre. The above information would distinguish Harry Brown from any other child of the same name.

[Other diagrams demonstrating nominal rolls suggested to be sent from welfare centres, school inspections, and treatment centres, to the statistical centre were shown by means of the epidiascope: also the recording of the information so received, on the card-index system, and a method of making the statistical investigation of a disease, illustrated in this instance by thread worms.]

Dr. F. J. POYNTON said he thought the paper omitted a very important point, namely, recovery hospitals. This should have a circle in the diagrammatic scheme.

The time had come when the English people, doctors and all concerned, should see that we had recovery hospitals connected with the great hospitals, not merely convalescent homes. He would like Members of this Section and all interested in children's diseases to keep this requirement in mind, so that such hospitals could be affiliated to the children's hospitals, divided into proper sections.

Case of Juvenile Tabes Dorsalis in a Girl aged 10 Years.

By C. Worster-Drought, M.D.

K. B., girl, aged 10 years. The mother states that the labour was normal, but that the child suffered from a skin eruption during the first year of life. Although the child has always been somewhat irritable and peevish, she kept fairly well until about eighteen months ago when she was knocked down by a bicycle. Following this accident she became more irritable, lost her appetite, slept poorly, was constantly crying, and has also suffered from occasional incontinence of urine.

Physical Examination.—Pupils: Right larger than left; neither pupil reacts to light, but both react to accommodation-convergence. Right internal strabismus. Optic discs and other cranial nerves normal. Knee-jerks and ankle-jerks both absent. Some degree of tendo Achillis and ulnar nerve analgesia. No Rombergism and no incoördination of upper limbs. Heart and lungs normal. The liver is just palpable below the costal margin. Teeth good. Mesial part of frontal bone somewhat prominent and slight thickening of wrist epiphysis.

Blood: Wassermann reaction positive. Cerebro-spinal fluid: Fifty-two lymphocytes per c.mm.; total protein content 0·02 per cent.; Wassermann reaction positive; colloidal gold reaction positive—curve of "luetic" type (·0344000000).

Mentally, the child is quite intelligent, though peevish and somewhat emotional; at school she does very well.

Family History.—The child is the youngest but one of seven children. Shortly after the birth of the third child, the father is known to have contracted syphilis and to have passed the infection on to his wife. Since this time the mother has had four miscarriages, and of the four children born alive, the first died at the age of 10 weeks from (?) meningitis, the second at 18 months from broncho-pneumonia, the third is the patient, and the fourth is apparently quite healthy, her blood yielding a negative Wassermann reaction.

Tabes dorsalis is admittedly a very rare condition in childhood and is even less frequently met with than general paralysis of the insane. The first recorded case appears to have been reported by Henoch in 1875. Remak, in 1885, recorded three cases, but the most important contribution to the subject was made in 1903 by O. Marburg, who reported the cases met with at Fuchs' ophthalmic clinic and cited thirty-four cases from the literature. The difference between juvenile tabes dorsalis and Friedreich's ataxia was not always apparent to the early observers; Hildebrandt first called attention to the error and drew the distinctions between the two conditions.

The age of 10 years is unusually early for the disease to be manifest, the average age incidence of the recorded cases being 15 years. The youngest case recorded is that described by Mingazzini and Bascheri-Salvadori, in which the onset occurred at the age of 3 years. The patient in Henoch's case was aged 6 years, and the late Mr. Sydney Stephenson showed a case,

aged 9 years, before this Section on November 26, 1915.[1] According to Cantonnet's analysis of cases, nearly twice as many females as males are affected, a striking contrast to the condition as seen in later life. Hereditary syphilis is responsible for the majority of cases, although a few are recorded in which the syphilitic infection occurred during infancy.

The pupillary signs are usually the first to appear and optic atrophy with complete blindness may follow. The percentage of cases showing optic atrophy is much higher than in adults. Loss of the deep reflexes occurs early and lancinating pains are complained of in 25 per cent. of cases, as compared with 75 per cent. of adult cases. The bladder is occasionally involved, the first symptom—as in the present case—usually being intermittent incontinence. Ataxia very rarely develops, and neither crises, girdle pains, nor trophic lesions have been reported.

For comparison with this case I am also showing one of early Friedreich's ataxia. It will be seen that the only signs which the two cases have in common are loss of the knee- and ankle-jerks. The case of Friedreich's ataxia shows general muscular incoördination with an ataxic gait and normal pupillary reactions, whilst the case of juvenile tabes exhibits Argyll-Robertson pupils but no ataxia.

Case of Early Friedreich's Ataxia (Shown for Comparison with the Case of Juvenile Tabes Dorsalis).

By C. Worster-Drought, M.D.

S. B., boy, aged 14 years.

Physical signs: Pupils equal and react normally: lateral nystagmus is observed only at times. Knee and ankle-jerks absent. Early pes cavus with extensor plantar reflexes. He walks on a wide base with considerable ataxia of the cerebellar type. Romberg's sign positive. Some incoördination of both arms and legs with dysdiadokokinesia. Occasional nodding tremor of head, especially after exertion.

The Wassermann reaction is negative in both the blood and cerebro-spinal fluid and the cerebro-spinal fluid is normal in all respects.

No history of the disorder in the patient's family can be traced.

Specimen of the Spleen from a Case of Gaucher Splenomegaly.

By O. L. Addison, F.R.C.S., and Harold Pritchard, M.D.

(I) Mr. O. L. Addison.

I think this condition is sufficiently rare to warrant the exhibition of a specimen from it at the Section. It was removed from a female child 1 year 7 months old, under the idea that the case was one of splenic anæmia. The symptoms appear to be characteristic, namely, enlargement of liver and bronchitis; and the sections of the spleen are characteristic too.

Dr. Harold Pritchard, whose patient the child was, will give further details.

[1] *Proceedings*, 1915-16, ix (Sect. Study of Dis. in Child.), p. 13.

(II) Dr. HAROLD PRITCHARD.

The patient first came under my care six months previously and was regarded as a case of splenic anæmia. As the child appeared to be going downhill Mr. Addison was asked to remove the spleen. This he did, the removal being followed by some improvement in the child's health.

Previous to operation the blood picture showed an anæmia of the chlorotic type with a leucopænia and a relative lymphocytosis: after excision of the spleen the blood showed a leucocytosis of 15,000 to 25,000; this is usually the case. The patient is one of three girls, the other two being in good health: the family history is negative. The father gives a positive Wassermann reaction, the mother a negative Wassermann and the three children a negative Wassermann.

The average length of life of patients suffering from Gaucher's disease is nineteen and a half years; they become progressively anæmic and death takes place from intercurrent disease.

Description of the Specimen.—Weight, 17 oz. Dimensions $6\frac{1}{2}$ by 4 by $2\frac{1}{2}$ in. Colour paler than normal. A uniform diffuse enlargement with no capsular thickening. The hilum glands were slightly enlarged.

Note the large masses of cells with clearly defined nuclei which fill the blood sinuses. These cells are endothelial in character and are probably derived from the lining cells of the sinuses. There is no pigment seen in this specimen. Similar masses of cells are described as occurring in the liver and in the bone marrow.

DISCUSSION.

Dr. F. PARKES WEBER said it was clear that Gaucher's disease was a systemic disease, affecting not only the spleen but the whole reticulo-endothelial system of cells, including the Kupffer cells of the liver, endothelial cells in the bone-marrow, &c. The peculiar "large clear cells" or "Gaucher cells" were distended with some substance, apparently a lipoid substance; in fact, in this disease this system of cells seemed to constitute a store-house of lipoid material. Mr. Addison had kindly allowed him to have a piece of the excised spleen in the present case, and by examining microscopical sections of it with crossed Nicol's prisms, Dr. J. W. McNee had proved the absence of doubly refracting lipoid and had shown that the substance infiltrating the cells in question was not a cholesterine lipoid. F. S. Mandlebaum, in America, maintained that the substance was not a cholesterine lipoid, but some combination of protein and lipoid. This disease was, Dr. Weber therefore thought, of the nature of some "inborn error of metabolism," and should be grouped with the diseases which Sir Archibald Garrod had described as due to an inborn error of metabolism, including alkaptonuria, cystinuria, congenital hæmatoporphyrinuria and congenital steatorrhœa. To the same group of diseases should probably be assigned congenital functional abnormalities and deficiencies of organs or tissues, such as of the bone marrow in congenital hæmolytic jaundice and congenital hæmolytic anæmia and the tendency to an aplastic type of anæmia. This view would account for the occasional familial occurrence of Gaucher's disease. Was there a familial history in the present case?

Dr. J. W. MCNEE said the disease was not quite so rare as the exhibitors considered it to be; this was the third example of the condition of which he had seen sections since 1920. Clinically it was most often mistaken for splenic anæmia. The present specimen showed the type of cells better than any specimen he had seen. There was no doubt that in the cells there was some abnormal material, probably of a lipoid nature. He did not think its chemical constitution had yet been discovered, but it was not akin to cholesterin fat. What it resembled most closely, microscopically,

was the material in amyloid degeneration, but obviously it had no relation to that condition.

Dr. KNYVETT GORDON said that Mr. Addison had sent a piece of the spleen to him for examination and it had shown no sign of fat. He (the speaker) had been interested in tracing the origin of the Gaucher cells; he had recently seen several cases of Gaucher's disease in Vienna. If one stained frozen sections, by Giemsa's stain—mounting in glycerine—one could trace the origin of these cells; they could be seen in almost every stage, the primitive blood cell, then cells resembling leucoblasts, then cells as in the sections. And there were many cells of the type which Marschalko described; they could not be seen in hæmatoxylin-stained sections.

Dr. F. J. POYNTON asked whether it was possible to diagnose this disease during life? If so, was it right to remove the spleen? Did such removal benefit the patient?

Dr. CAUTLEY said the points raised by Dr. Poynton were very important. If the view expressed by Dr. Parkes Weber was correct, namely, that the condition represented an error of metabolism, it seemed clear that mere removal of the spleen would be unlikely to cure it, still less would splenectomy have any effect if Gaucher's disease were a general affection of reticular cells throughout the body. What was Mr. Addison's view about the prognosis, and as to how the child was now developing?

Dr. F. PARKES WEBER, in further comment, said he did not think that because a disease was not limited to the spleen, it followed that splenectomy must necessarily be useless. A disease might affect a tissue-system or cell-system of the body, and yet splenectomy might do good. Familial or congenital hæmolytic jaundice likewise affected the reticulo-endothelial system of the body (but in another way) and yet, although the disease affected the liver as well as the spleen, removal of the spleen successfully carried out did good in very many cases.[1]

Mr. ADDISON (in reply) said that in the cases of which he had looked up the records and in which the spleen had been removed that operation had not affected the course of the disease at all; the liver had continued to enlarge. That seemed to be the prevalent idea. Therefore, unless in this case the patient's condition should improve very much and the liver cease to enlarge as a result of the operation, he did not think the right treatment in the future would be removal of the spleen. He believed the disease in question had been diagnosed from puncture of the spleen.

Case of Fragilitas Ossium with Blue Sclerotics.

By B. WHITCHURCH HOWELL, F.R.C.S., and W. L. KINNEAR, M.D.

H. C., GIRL, aged 3½ years. She was sent up for an opinion by Dr. Pratt, and was admitted on October 27, 1923, to the Queen's Hospital for Children, Hackney Road, under the care of Mr. B. Whitchurch Howell.

Family History.—Father alive and well. Mother alive and well. Only child. Mother broke collar-bone, wrist, humerus, radius and ulna. Altogether mother had four fractures of right arm. First fracture at 1-2 years old. Last fracture when 13½ years old. This is the only other member of the family similarly affected. In addition she has blue sclerotics.

Previous Health.—Had diphtheria two years ago. Fractures as follows:—

(1) 1921, January. Fracture of radius and ulna (left), 1/3.
(2) 1921, July. Fracture of radius and ulna (left), 1/3.
(3) 1922, August. Fracture of tibia (right), 1/3.
(4) 1922, November. Fracture of radius and ulna (left), 1/3.
(5) 1923, July. Fracture of radius (right), 3/4.
(6) 1923, August. Fracture of radius and ulna (left), 1/3.

[1] Cf. remarks by F. Parkes Weber, *Proc. Roy. Soc. Med.* 1922-23, xvi (Sect. Med.), p. 77.

She was treated for the fractures with splinting, and union took place in normal time. Alignment was good until last fracture. Internally she was given virol and calcium.

On Examination, on November 5, 1923.—Height, 2 ft. 11 in. Weight, 1 st. 13 lb. 11 oz. *Head:* Prominence of occiput rather more marked than usual. Sutures perfectly closed; no bossing; no tabes. *Eyes:* Sclerotics distinctly blue. Fundi normal. No nystagmus; no strabismus. *Teeth:* Very carious and worn. No pyorrhœa present. *Colour* good. *No anæmia. Chest:* Well developed. No signs of rickets or of fractured ribs. *Heart* and *lungs* normal. *Abdomen:* Liver and spleen normal in size. *Extremities: Right forearm*—fracture healed with good alignment; *left forearm*—marked curvature of both bones at junction of upper and middle thirds; limitation of supination. *Right leg*—fracture healed in good position. *Urine:* Repeated examinations showed no abnormal constituents. *Blood:* Wassermann negative. Blood calcium, 12 mgr. per 100 c.c. of blood.

Operation.—Osteoclasis of both bones of forearm on November 3, 1923. Correction maintained in plaster of Paris. (1) X-ray before correction of malunion showed excessive amount of external callus. (2) X-ray after correction showed good alignment.

Mother's Blood.—Calcium, 20 mgr. in 100 c.c. of blood.

Diet.—Ordinary.

Drugs.—1/200th phosphorus, night and morning.

Opinions are asked as to treatment.

A Case of Anæmia in a Premature Infant treated by Intraperitoneal Injection of Blood.

By Eric Pritchard, M.D.

F. N., aged 8 months on admission. Present age 11 months 1 week. She was brought up to hospital on October 2, 1923; the mother complained that the child was losing weight and was very pale. The child was one of twins, both born at the seventh month of pregnancy. The mother had been quite healthy during pregnancy, and the labour apart from being two months premature was quite normal. The twins were the mother's first children. This child's birth-weight was 2 lb. 10 oz. She weighed on admission 10 lb. 3 oz. Present weight 12 lb. 10 oz. The child was never breast-fed; she was fed first on cow's milk one part, and water two parts—three weeks; then "Cow-and-Gate" full cream. She is now on mixed diet.

She was extremely pale on admission; and the mucous membranes were also pale. Blood count: Red cells, 2,416,000 per c.mm.; white cells, 19,200 per c.mm.; hæmoglobin, 26 per cent.; colour index, 0·5; polymorphonuclears, 22 per cent.; large lymphocytes, 3 per cent.; small lymphocytes, 72 per cent.; transitional cells, 3 per cent. The spleen was palpable, but not very enlarged. The Wassermann reaction was negative.

On October 30, 20 c.c. of the father's blood were injected into the child's peritoneal cavity. And the blood count on November 2, was: Red cells, 3,952,000; white cells, 13,200; hæmoglobin, 40 per cent.; colour index, 0·5; polymorphonuclears, 35 per cent.; large lymphocytes, 6 per cent.; small lymphocytes, 54 per cent.; transitional, 5 per cent.

On January 9, 25 c.c. of blood from one of the nursing staff (the blood being of the same group) was injected into the child's peritoneal cavity, and the blood count on January 11, was: Red cells, 3,440,000; white cells, 10,400; hæmoglobin, 38 per cent.; colour index, 0·5.

DISCUSSION.

Dr. CAUTLEY said that there was no need to discuss the point about anæmia being very apt to occur in prematurely-born infants, particularly twins; but was it necessary to give small quantities of blood intraperitoneally, rather than subcutaneously? What was the blood count before the second intraperitoneal injection was given? On January 11 the red cells were even less than on November 2, after the first injection of blood.

Dr. IZOD BENNETT said that evidence was wanted that blood given intraperitoneally was definitely absorbed into the circulation. This could be determined by a series of experiments on animals of different types. It seemed that intraperitoneal extravasations of blood were not absorbed in the form of living corpuscles. More evidence was needed as to that point before this method was generaliy adopted; not that there was any danger in the process.

Dr. KNYVETT GORDON said that if healthy blood were injected into the peritoneal cavity of an animal, there occurred a large exudate of phagocytic cells, which easily ate up red cells. In this case there was a steady diminution in the number of white cells, and not a great increase in that of the red cells. Also the quantity injected was very small. The blood picture was that of a bacillary infection. There was an absolute and relative lymphocytosis, probably of intestinal origin. It was a question whether it was not a case in which the exudate of the phagocytes simulated that which was produced by an intraperitoneal infection, the blood cells not getting into the general blood stream at all.

Dr. ERIC PRITCHARD (in reply) said that he did not himself believe the blood cells could be absorbed in these circumstances, but the clinical advantage of the method seemed obvious after each injection, whatever the *modus operandi* might be.

A Child in Whom the Sex has not yet been determined.

By W. E. HILLS, M.R.C.S.

THIS infant, aged 16 days (on January 25, 1924) is the first child of healthy young parents. At its birth it was casually dismissed by the nurse as a girl, and it was nct until some days later that its genitalia were noticed to be abnormal. It was otherwise apparently quite healthy and weighed 7½ lb. at birth. When it was a week old I was asked to see it with a view to the expression of an opinion as to its sex.

The following description is based on the assumption that the child is a male, which seems the more probable hypothesis.

The penis is short and adherent by its under surface to the tissues beneath. The prepuce has a hooded appearance which increases the resemblance of the penis to a clitoris. The urethra opens into a depression below the base of the penis. There is no evidence of a vagina. The scrotum is completely divided in the middle line, leaving a deep groove between its two halves, which makes its resemblance to labia majora very striking. There is sometimes a rugose appearance of the skin in this position which suggests the presence of contractile tissue (dartos) beneath the skin. On rectal examination nothing

suggestive of a rudimentary uterus was felt. There is no evidence of either testis in the inguinal canal or scrotum.

Examination of the kidney regions gives no evidence of hypernephroma.

DISCUSSION.

Dr. CAUTLEY said it looked to him like a male infant in whom there was marked hypospadias with undescended testicles.

Dr. F. PARKES WEBER said it would be of interest to see whether any precocious sexual development would take place in this case. In some cases precocious development and a hermaphrodite type of external genital organs had been associated with hypernephroma. The present child, however, seemed so badly nourished that it might not survive.

Dr. F. J. POYNTON said the most remarkable case of the kind he had had was one he exhibited at the last International Congress of Medicine, held in London. It was a question whether it was a boy or a girl. Learned doctors from all parts of the world saw the case, but could not make up their minds on it. The parents had brought the child up as a girl, and Mr. Morgan, with whom the speaker consulted, agreed it was wise to let the child be brought up as the parents thought best. Mr. Morgan himself thought that child was a boy, and that this opinion would be confirmed if a bass voice was developed and other male characters at puberty appeared.

He had also had another case which puzzled him very much. In that case the child was proved to have no uterus, and the diagnosis was that the child was a boy.

Dr. D. PATERSON said that a review of the literature showed that in a number of cases in which there was a dispute as to the sex, the children, on abdominal section, were usually found to be boys.

Postscript.—The child was seen on March 19, 1924. It had wasted considerably during the early weeks of its life owing to inability on the part of the mother to feed it, but it is now taking a food which suits it and has put on weight and now weighs 8 lb. 0½ oz.

The general appearance of the genitalia is now more of the male type. The penis measures 1½ centimetres in length, and the mother states that the infant passes a strong stream of urine straight forwards.

Choreiform Movements Persisting for Four Years.

By T. IZOD BENNETT, M.D.

THIS girl, aged 10 years, is now showing choreiform movements for the fourth year in succession. The patient first came under my observation three years ago, after a colleague had seen her in violent choreiform movement; after the application of cold packs she went on to a state of complete flaccid paralysis. She lay in that condition in hospital for nearly two months, and then gradually recovered voluntary movement. For a week or two afterwards her condition was normal, then the choreiform movements reappeared and they have persisted ever since. The child has never given any indication of having a rheumatic affection. The mother states that three months before my colleague saw the child she was well; that then she had a sudden access of somnolence, lasting not more than twenty-four hours, and at the end of that time the movements began.

When first seen in this violent condition the patient had been given

bromides in such large doses that there was a bromide rash; she had also had various hypnotic drugs. I have myself employed various remedies, not in excessive doses; I have not used trional. Nothing seemed to influence the course of the case. For the last twelve months I have considered it was probably encephalitis. Some Members seem to consider it a case of tic and that suggestive treatment might do good. I should value suggestions as to treatment.

DISCUSSION.

Dr. CAUTLEY said that the case was probably one of encephalitis lethargica, followed by myotonic movements. There might be a neurotic condition or chorea in addition, but there was no chronic jerk of the tongue.

Dr. WALTER CARR said that he did not think this was a case of ordinary protracted chorea. There was no lesion of the heart, and he believed there was no family history of chorea or rheumatism. Chorea was most unlikely to last such a long time with practically no variation. He regarded it as a sequel of encephalitis lethargica, and thought that the prognosis as regards recovery was very unfavourable. He advised that drugs should be stopped and the child placed where she could live an open-air country life.

Case of (?) Sarcoma of Back.

By REGINALD C. JEWESBURY, M.D.

PATIENT, L. R., a female infant, aged 11 months. Both legs have always been limp and have never been moved. A swelling on the left side of the back has been noticed since the middle of December, 1923, and has since increased slightly in size.

The child is unable to sit up. The head was held up at seven months. The first tooth came through at four months. The patient has always been constipated and has had rectal prolapse. Apparently there has always been urinary incontinence.

The birth was normal at full term. Birth weight 9 lb. There are three other children, all healthy.

A swelling is present on the left side of the back, apparently fixed to the lowest ribs and to the spine, from about the level of the eleventh dorsal to the second lumbar vertebræ. The tumour is very hard and rounded, the surface smooth, except for slight ridges. It is dull on percussion, and there is no fluctuation or pulsation. It does not appear to be tender. It is also palpable bimanually, but not movable except with the spine.

The other systems are normal.

There is a slightly raised temperature, otherwise the child seems well.

X-ray Report.—"A dense shadow on the left, not apparently connected with bone. (?) Some erosion of first and second lumbar vertebræ."

Blood Count.—Red cells normal. White cells 15,800, polymorphs 58 per cent.

Electrical Reactions.—Show complete R.D. of all muscles of legs, glutei, probably psoas, and sphincter ani.

The provisional diagnosis is that of periosteal sarcoma; the question of the causation of the paraylsis dating from birth appears to be one for discussion.

Case of Renal Insufficiency in an Infant.

By Eric Pritchard, M.D.

Patient is a female child, aged 8 months, suffering from vomiting and other symptoms, which were thought to point to pyloric stenosis. Mr. Tyrrell Gray and Dr. D. Paterson saw the case in September, 1923, and neither thought it was one of pyloric stenosis. The baby was sent to a home to be kept under observation. There it has been doing badly, its weight remaining stationary. Vomiting came on whenever there was an attempt to increase the amount of food, and especially when the protein was increased. The child's skin has a very unpleasant earthy smell. A sample of urine was found to contain a very low percentage of urea, 0·5 in the first instance, and later it was a little higher. A urea-concentration test showed that the urea in the blood was fairly high, i.e., 56 mg. per c.c. blood, although the urea in the urine was a quarter of the normal. Whether the renal insufficiency is due to cystic kidney, hypoplasia or to interstitial nephritis, I cannot say. There is a trace of albumin in the urine, the specific gravity is very low, i.e. 1006, and there are no casts or renal cells. The weight of the child is now 8 lb. The birth weight was 6 lb. 8 oz.

Case for Diagnosis.

By B. Whitchurch Howell, F.R.C.S.

In this case there is local muscular atrophy or wasting with local atrophy of fat, with, perhaps, some of the subcutaneous and cutaneous tissues as well; and I have brought the patient here in the hope of getting opinions as to the diagnosis and treatment. She was sent to me by two doctors on account of the sudden instability of gait; the doctors say that since they first saw the patient the condition has become more marked. The wasting is chiefly in the gluteal muscles.

She is the second child of first cousins. The other child had nothing abnormal in her appearance except fair hair and albino-like eyes. I will go into the case more fully, and report it to the next meeting of the Section. The Wassermann test has not yet been done.

Dr. F. Parkes Weber said that the case was certainly not one of the recognized "superior type" of lipodystrophia progressiva, such as he had described ("Lipodystrophia Progressiva," London, 1918). He would draw special attention to the minute nodules, probably fibrous, which could be rolled about in the subcutaneous tissue over the tibiæ.[1]

[1] Mr. Howell kindly allowed me to study the patient in detail a few days after the meeting, and I came to the conclusion that the case was one of patchy atrophy of subcutaneous fat, allied to circumscribed sclerodermia (morphœa). One of the atrophic depressed patches was a zona-like band across one side of the thorax. Another atrophic patch formed a groove on the plantar surface of one of the feet and involved the second toe, simulating a congenital defect. In two places at least the child's skin was abnormally "glossy."—F. P. W.

Section for the Study of Disease in Children.

President—Mr. R. H. Anglin Whitelocke, F.R.C.S.

DEMONSTRATION OF CASES.[1]

By H. C. Cameron, M.D.

(1) Chronic Remittent Ophthalmoplegia (Oppenheim) Oculomotor Migraine (Charcot) in an Infant.

Louisa W., aged 2 years 8 months, was admitted to the ward when 8 months old for sudden paralysis of the muscles of the right eye, with ptosis, immobility of the eyeball and of the pupil. Within a few days the paralysis had almost disappeared, leaving behind it only a slight ptosis and slightly deficient pupillary reactions. Lumbar puncture showed nothing abnormal. The paralysis has become complete on four occasions, in each case lasting only a few days. In all other respects the child's health is satisfactory and the



Fig. 1.—Case (1). Chronic remittent ophthalmoplegia (Oppenheim).

paralysis is not accompanied by pain, fever, vomiting, giddiness or other symptoms. There is nothing to suggest the presence of syphilis in the patient or in the family, and no history of migraine in the parents. There is no family history of any similar trouble. The dates of attacks have been as follows: first, November 7, 1922; second, May 22, 1923; third, August, 1923; fourth, December, 1923.

[1] Meeting at Guy's Hospital, February 22, 1924.

(2) Cyclical Vomiting in a Boy in whom the Attacks are heralded by an Increase in a Chronic Remittent Ptosis, shown for the sake of Comparison with Case (1).

WILLIAM C., aged 10 years. Epilepsy in the family but no migraine or history of bilious attacks. No other member of the family affected with ptosis. For the last eighteen months has suffered from recurring attacks of headaches and vomiting at monthly intervals. Each attack is preceded by increasing drooping of the left eyelid, for about twenty-four hours, so that the child's mother is always able to predict its oncoming. The attacks last about a week, during which time the ptosis gradually decreases but never entirely disappears even between attacks. No visual phenomena during the attack, which does not differ from cyclical vomiting in children. In the intervals the boy is in good health, though thin, and mentally very active.

(3) Congenital Nuclear Hypoplasia (Moebius) involving the Facial and Oculomotor Nuclei in Association with other Congenital Abnormalities.

RONALD C., aged 3½ years, with bilateral facial paralysis of congenital origin. In crying and laughing there is no change in the mask-like expression. The forehead, especially, is completely immobile. There is profuse salivation



FIG. 2.—Case (3). Congenital nuclear hypoplasia (Moebius).

from the paresis of the lips. The mother states that in infancy suction was awkward and difficult but that suction was better performed at the breast than with the bottle. The movements of the eyes are very restricted, especially the lateral movements. There is troublesome epiphora, for which in the first year

of life the child was taken to an eye hospital where the lachrymal duct was dilated. The optic discs are normal and vision is good. The labio-glosso-pharyngeal nucleus appears in this case to have escaped. Speech is well advanced and is only defective in that P and B sounds are replaced by T and D. The tongue is protruded well. Intelligence is good. Most of these cases show other congenital defects. Here, the feet show talipes equinovarus and the fingers of the right hand are webbed and deformed.

(4) Congenital Lagophthalmos.

CHARLOTTE H., aged 6 years, born with Cæsarean section. Shortening of the upper eyelid, due apparently to tonic spasm of the levator gives the appearance of exophthalmos. The defect was apparent in early infancy. The

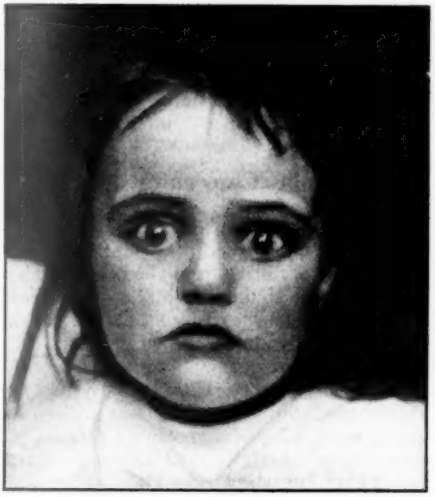


FIG. 3.—Case (4). Congenital lagophthalmos.

eyelids can be approximated with voluntary effort, but during sleep the eyes remain widely open. No other congenital abnormality and no other members of the family similarly affected.

(5) Three Cases of Erb's Palsy occurring in One Family.

THE mother has had seven children; six are now living. One, a twin, died of wasting at four months. The surviving family consists of:—

(1) A girl aged 11 years, normal; (2) a girl, aged 9 years, left arm affected; (3) a boy, aged 8 years, right arm affected; (4) a boy, aged 4 years (the surviving twin), normal; (5) a girl, aged 2½ years, normal; (6) a boy, aged 6 months, right arm affected.

Pelvimetric examination of the mother revealed no abnormality. The girl affected weighed about 10 lb. at birth ; a vertex presentation ; normal labour. The elder boy weighed (?) 13½ lb. at birth ; difficult labour ; considerable force used in extracting the child. The younger boy weighed 12 lb. at birth, and labour was difficult. In all three cases the paralysis was noticed soon after birth. The confinements were conducted by three different midwives.

The family was transferred to the care of Mr. Trethowan who reports, December 6, 1923 : " It is interesting to note that the two boys are examples of Erb's paralysis of the arthritic shoulder type, rather than the paralytic type, while the girl is more purely paralytic and less arthritic. The clinical condition is supported by the radiograms of the shoulder-joint. The marked feature of both boys is the extreme stiffness of the shoulder-joint, wherein there is very little independent movement, what there is being mostly at the scapula. In both these cases the inversion contracture (presumably maintained by the shortness of the subscapularis) is marked, as well as the tendency to the backward subluxation of the head of the humerus. The girl's condition is the least serious of the three. The shoulder-joint is comparatively free, and abduction, combined with external rotation, is only very slightly limited. The girl with the paralytic type has a good radiographic shoulder, but the elder boy of the arthritic type has a rough, flattened, irregular head to the humerus."

In point of intelligence the elder boy is distinctly backward.

(6) Myotonia Congenita (Thomsen).

HILDA S., aged 9 years. Patient's mother had five brothers and two sisters. Of the five brothers, two died in asylums, one at the age of 17 (he had been mentally deficient from birth) and the other at about 30 years of age. On the paternal side there is no significant history of disease.

Patient's mother bas had thirteen children, eleven still living. The following is a list of the children : Female, aged 29 years, normal, five healthy children ; female, aged 27 years, normal, one healthy child ; female, aged 25 years, normal, two healthy children ; male, aged 23 years, normal ; female, aged 21 years, normal ; male, aged 20 years, Thomsen's disease ; female, aged 19 years, normal ; female, aged 17 years, normal ; female, died of meningitis ; female, aged 12 years, normal ; female, aged 9 years, the patient ; female, died soon after birth ; female, aged 6 years, normal.

Weighed 8 lb. at birth ; breast fed ; always had nocturnal enuresis and slowness of movement ; takes a long time to dress, and cannot do drill properly at school. Shows marked enlargement of calf, thigh, arm, forearm and neck muscles. Walks on a wide base and slow in starting, but once started rapidly achieves a normal pace.

The reaction time to visual and auditory stimuli is within normal limits.

Drum tracings, illustrating muscular contractions, are shown.

(7 and 8) Amyotonia Congenita (Oppenheim), Advanced and Incipient.

(*a*) *Advanced.*

ELSIE S., a girl aged 9 years, shown on March 23, 1917 (*Proc. Roy. Soc. Med., Sect. Study Dis. Child.*, 1917, x, p. 95), when aged 2 years. " E. S., aged 2 years. The family history is without bearing on the case. The child was

breast fed until the age of 12 months. The teeth were cut without disturbance at the age of 8 months. There has never been any digestive disturbance, and the child, though small, is fairly well nourished. She has been under my observation for nine months. Lately there has been rapid improvement, and crawling is now accomplished comparatively easily." Shown again on February 28, 1919 (*Proceedings*, 1919, xii, p. 25), when the amyotonia was more clearly ev dent, although the child, then older and in other ways stronger, had learnt to walk. To-day after a further interval of five years, the flaccidity and hypermobility of the joints is very striking. The hypotonicity involves arms, legs and trunk, but not the face. Eighteen months ago for the first time evidence of atrophy of muscle groups began to appear. Until that time the child was not wasted and when at rest, sitting or reclining, appeared in every way normal. There is now atrophy of the trapezius, triceps and other muscles of left arm and shoulder. No pseudo-hypertrophy, mechanical irritability or fibrillar contraction has ever been noted. The tendon reflexes are absent. A radiograph of the bones illustrating absence of rickets but extreme slenderness is shown, taken at the age of 2 years.

(*b*) *Incipient.*

GWEN L., aged 18 months. Plump and well covered, with active movements, not noticeably weak, yet with extreme amyotonia of all the muscles, so that the most fantastic postures can be assumed. An X-ray examination has excluded the the presence of rickets. The general health has been good. The child is just learning to walk. I believe, however, that the course will be similar to that of E. S. In neither case is there a family history of any similar disease.

(9) Lymphangeioma of the Leg.

A BOY, aged 6 years. The swelling was noticed a few days after birth and rapidly increased after the sixth month. At eighteen months when first seen the affected leg measured three times that of the other below the knee. The swelling ended somewhat abruptly at the middle of the thigh. Two attempts had been made to excise the redundant tissue at least in part but no perceptible decrease resulted. An X-ray of the affected leg showed great thickening of the periosteal bone. At eighteen months when he came under my care I considered the question of amputation. The photograph exhibited shows the condition of the tumour at that time. Because after a period of rapid growth it had apparently ceased to extend, I decided against amputation. The boy has grown strong and sturdy. He walked in his third year. Walking had been prevented until then by the sheer size of the tumour. The swelling compared with the size of the boy is now of moderate dimensions although actually its measurements have not changed. It now causes but little inconvenience.

(10) Hypothyroidism of very Slight Degree.

DOROTHY B., aged 18 months. Breast fed until 15 months. Sat up at 5 months. Walked at 15 months. Admitted a fortnight ago with the complaint that she was losing her power of walking. The skin was somewhat dry, the abdomen a little prominent with slight eversion of the umbilicus. The temperature was a little subnormal. The facial expression was a little

dull. The ossification of the skull was defective, the anterior fontanelle widely patent, the posterior fontanelle just palpable. There were no other bony deformities suggesting rickets. The X-ray examination shows no trace of rickets, but the wrist is still cartilaginous and the epiphysis for the os magnum which normally should appear in the first year is still absent. Treatment has not yet been begun.

(11) Spasmophiliac Convulsions during which Hemiplegia occurred.

A. D., MALE, aged 1 year 3 months, a full time normal child, breast fed for nine months. The child had its first fit in October, 1923, when aged 10 months. Similar fits occurred about once a month until February this year, when the fits became much more frequent, recurring two and three times each day. Attacks of breath-holding and laryngismus were apt to occur whenever the child cried. After one such fit on February 3, it was noticed that the right arm was spastic and that the child could no longer walk. On February 13 the child had a succession of fits, about twenty in number; each one commenced with twitching of the right eye-lids, spreading to the whole of the right side of the face, and to the right arm and leg. The right arm was spastic afterwards.

When seen at the out-patients' department the next day the child had no fits, but Chvostek's and Trousseau's signs were positive. The right arm and leg show paralysis of the upper neurone type, with rigidity and increased tendon reflexes.

Since admission into the ward, the child has had many similar fits accompanied by some laryngeal stridor.

The question to be considered is whether the organic lesion was due to a vascular accident caused by the cerebral congestion during the spasmophiliac convulsion, or whether the child suffered from some form of encephalitis which converted the "latent" into "manifest" spasmophilia.

(12) Case for Diagnosis: Arachnodactyly.

[The patient in this case, happening to attend in the Ophthalmological Out-patient Department, was brought by Mr. A. W. Ormond and shown to the Section. It was recognized by Dr. Parkes Weber and others as a case of arachnodactyly. Mr. Ormond has since furnished the following particulars.]

T. K., AGED 12 years, youngest of three children. Two brothers, aged 18 and 16, healthy. No family history of disease of any sort. At birth he was noticed to be thin. When aged 2½ years he was taken to a doctor because of his extreme thinness and was treated for tuberculosis. His vision has always been very defective and has prevented him attending school since the age of 7 years. "Needling" of the eye has been performed four times at the Royal London Ophthalmic Hospital since 1920. Although blind and illiterate he is active mentally and has greater strength and much better health than his appearance would suggest. His height is 6½ in. greater, his weight 18 lb. less, than the average of his age. The head is dolicho-cephalic, the thorax narrow and flat (circumference in nipple line, 22½ to 23½ in.). The limbs, and especially the hands and feet, are disproportionately slender.



FIG. 4.—Case (12). Arachnodactyly.

Other abnormalities are a highly arched palate, an abnormal conformation of the pinna, bilateral iridodonesis, dislocation of lens in left eye (the right has been absorbed as result of operations), absence of subcutaneous fat, acrocyanosis, coarse straight hair, slight bilateral contractions of the biceps brachii,

preventing complete extension of the arm, slight dorsal scoliosis, slight webbing of the fingers, slight upward displacement of the patellæ with thickening of the lower ends of the femora, susceptibility to mydriatics. Radiographic examination of the bones shows some slight rarefaction of the long bones in places, but no gross abnormality. Sella turcica shows no expansion but the dorsum sellæ is rather vertical in position. Left eye extremely myopic—30 diopters. In the right eye the lens has been lost. In the right eye, vitreous opacities obscure the fundus. In the left, the fundus, seen with difficulty, appears normal.

Section for the Study of Disease in Children.

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A Case of Progressive Lipodystrophia in a Boy aged 7½ Years.

By Vincent Coates, M.D.

S. P., MALE, aged 7½ years, first seen by me when just aged 7 years, having been brought by Dr. P. T. Jones, of Coleford, Somerset, with a diagnosis of progressive lipodystrophia. No complaints whatever as to general health.

Past History.—Nephritis two and a half years ago. No previous illnesses.

Family History.—Father and mother healthy. Both in great poverty with many hardships during the first few years of married life. Mother: One stillborn child, two miscarriages, two healthy girls, one healthy boy (patient).

History of Present Illness.—When aged 5 years had nephritis (see above).

On Examination.—General condition, sallow complexioned and cadaverous looking child. Marked loss of subcutaneous tissue of face, upper arms and thorax. Hollow cheeks and depressions in front of eyes. The zygomatic ridges are prominent and there is a very slight tendency for the hair to extend in front of the ears and across the nose. There is a fine downy hair between the shoulders. The silky skin can be lifted quite easily for some distance from the thorax and arms and the examining fingers almost meet owing to the lack of subcutaneous fat. His measurements, the thickness of skin, &c., are as follows: Weight 22·3 kilos., height 120·5 cm. Circumference: Arm 16 cm., forearm 17 cm., thigh 32·5 cm., leg 24 cm. Skin thickness (lifted fold): Cheek 4 mm. +, breast 3 mm., arm 2 mm. +, forearm 3 mm. thigh 12 mm., leg 12 mm. Alimentary system: A few carious teeth. Cardio-vascular system: No abnormality found. Respiratory system: No abnormality found except a slightly lowered vital capacity. Central nervous system: No abnormality found. Urogenital system: Albumin frequently present in urine. Both testes small, but not abnormally so. X-rays show the sella turcica to be normal. The Wassermann reaction is negative. All attempts to get a blood sugar curve have resulted so far in failure owing to the rapid coagulation of the patient's blood. The calcium content of the serum is 14 mgm. per 100 c.c.

Very few cases of this condition appear to have been recorded in boys and the ætiology is obscure. Various hypotheses have been advanced as to its causation. Heredity is stated to be a factor in some instances; in others, fright, acute infections, trophoneuroses, &c. The factor of endocrine disturbance has its adherents, and this suggestion is strengthened by the good

results obtained in two cases by polyglandular therapy recently reported in America. The fact that the calcium content of the serum in this case is rather above the normal 10·5 mgm., and that the two cases above referred to were successfully treated by glandular extracts containing parathyroid, tempts one to consider the desirability of a fresh examination of the serum for calcium both ionic and combined.[1]

DISCUSSION.

Dr. F. PARKES WEBER remarked that the present case was the second of the kind which had been shown before this Section, and it was an excellent example of the disease as met with in the male. The appearance differed from that in most female cases in that the lower extremities were not fat. It corresponded exactly with the first male case shown, here, by himself, on April 27, 1917.[2] That case was originally seen by Dr. E. A. Cockayne, who passed it on to the speaker, as he (Dr. Cockayne) had to be absent from England at the time.

In another case, in a girl, which the speaker had shown with Dr. T. H. Gunewardene, there was grave suppurative ear disease, from the result of which death ultimately occurred, and the post-mortem microscopical examination was made by Dr. H. M. Turnbull, of the London Hospital.[3]

The child now shown by Dr. Coates appeared to be remarkably normal apart from the lipodystrophia, and it was to be hoped that the disease would as in various other cases become stationary. He had no reason for believing that the lipodystrophia shortened life. The patients, unless some other disease was present (such as the ear disease in the above-mentioned case), did not suffer in any way, though they might dislike the appearance of emaciation. A question arose as to whether the bilateral atrophy of fat in the face which had been observed occasionally in young males was a lesser degree of lipodystrophia progressiva. It was, he thought, probable, but he did not think that question could be settled at present. If such bilateral facial cases were admitted, certain cases of hemiatrophy of the face, in which the hemiatrophy was confined to the subcutaneous fat, would have to be included as examples of *unilateral lipodystrophia*.

Dr. F. LANGMEAD said that some years ago he showed before the Section a similar case [4] in a girl, in whom the disease began at about 5 years of age, and she was aged 7 years when he showed her. In that case the legs were not unduly obese. He had seen her again a week ago and he found that her condition had remained stationary, which supplied ground for Dr. Weber's hope. His own patient felt cold weather a good deal, and was very sensitive to weather changes. Except for that, she was active and well, and was bright at school. In her case there was no evidence of muscular weakness.

He did not think these cases could well be regarded as due to polyglandular defect. At the present time it was quite common, in the absence of a reasonable explanation of an obscure or peculiar disease, to attribute the condition to a defect of the ductless glands, as it was well known that serious defects could occur in those glands without any histological evidence being obtained. In these cases why should the wasting stop so abruptly at the pelvis? And on that assumption there was no reason why the legs should escape altogether.

[1] This case will be published in fuller detail and with photographs in the *British Journal of Children's Diseases*.

[2] F. Parkes Weber, *Proc. Roy. Soc. Med.* (Sect. for Study of Dis. in Children), 1917, x, p. 117.

Weber and Gunewardene, *Proc. Roy. Soc. Med.* (Sect. for Study of Dis. in Children), 1919, xii, p. 13, and 1920, xiii, p. 1 (containing Dr. Turnbull's report). In a recent article on the subject Professor L. N. Boston, of Philadelphia (*New York Med. Journ.*, 1923, cxviii, p. 668) has confused his case with Dr. Langmead's case (*Proc. Roy. Soc. Med.* (Sect. for Study of Dis. in Children), 1919, xiii, p. 6).

[4] *Proceedings*, 1919, xiii (Sect. for Study of Dis. in Children), p. 6.

With regard to the parathyroids and the calcium content: before the calcium content could be estimated in his own case, the patient was lost sight of. The result from parathyroid treatment was always difficult to estimate. Recently he saw a chemist who had been working for one of the large firms which did much slaughtering, and he had been shown a small bottle containing 4 oz., which was said to be all the parathyroid which could be secured from 240,000 animals. If that was so, where did all the parathyroid come from?

Dr. COCKAYNE said that shortly before the war a tuberculosis officer sent to him a boy with extreme thinness of the face and upper part of the body. In every other respect he was fit and healthy, and he (the speaker) was required to state whether the boy had tuberculosis. He could not find anything organically wrong, and said so, but did not recognize it was an example of the disease under discussion. Shortly afterwards he thought it must be so, and looked up the notes for the year with the idea of sending for him, but that note was missing, and so the case was never described. A little later he came across the case which he handed to Dr. Parkes Weber as he himself was engaged on war work. He had not heard of the boy since the war, and would be interested to learn something as to his present state.

DISCUSSION ON ENURESIS.

Dr. A. DINGWALL FORDYCE.

THE subject for discussion this afternoon is one which in the initial stage appears to me to demand consideration from a very general point of view.

ἐνουρειν signifies to void urine; this voidance in a baby is uncontrolled: in a healthy elder person it takes place at discretion. The voidance of urine is of course a natural act and any abnormality in its occurrence must be either in its method or in its time. In the case of a normally incontinent baby it takes place at any time and all times, and it occurs without implication of the higher nervous centres. After three years of age or so the higher nervous centres—those of will and of inhibition, take over command of lower reflexes and the normal child passes his urine as willed.

The continuance or recurrence of any baby peculiarity is a definite abnormality and cannot be a physiological reversion. Urinary incontinence after babyhood is a sign of nervous abnormality. In other words, enuresis is a nervous *symptom*—not a disease *sui generis*. Children who are mentally defective and children with certain gross physical lesions are often distinctly incontinent, but I take it that to-day we are not dealing with such. We are considering the symptom of enuresis as it frequently confronts us in children who suffer from no gross mental or physical defect.

The normality of a child of 3 years is a compound of his physical and mental structure: this again is the result of his nature and his nurture. Long before the age of 3 years the influences of heredity and of environment have merged and clashed, and a long step forward has been made in the determination of the attainable degree of bodily welfare and peace of mind. The excretion of urine from the bladder is the result of a co-ordinated neuro-muscular effort controllable by the will. Incontinence demonstrates either neuro-muscular incoördination or faulty power of inhibition. Occurring during sleep, it must signify either excessive urgency of stimulation or torpidity of control.

In cases such as those of which we are speaking in which there is no obvious physical abnormality it is a matter of common knowledge that

ordinary urine examination reveals usually nothing outside the bounds of the normal, and we have no reason for believing that the determining cause of the symptom is excessive urgency of stimulation.

On the other hand, we have very definitely to consider neuro-muscular inco-ordination and interference with the power of control. We have in a word to deal with the nervous system throughout its extent—in its highest function as in its lowest. Is it inevitable in all cases that abnormality of the higher centres should be present?

Undoubtedly in cases of gross lesions or local abnormality the symptom may occur as a direct result of the lesion or abnormality, and there may appear no reason to implicate higher nervous centres. But it is hard, if not altogether impossible, in the case of a young child to be confident in asserting dogmatically that so far and no farther is abnormality of nervous function present.

A local abnormality of function in itself reacts in evil fashion on the general condition, and, further, this local abnormality is sometimes only one sign and end-result of a general morbid condition. Purely urinary symptoms are often nervous in origin and yet the nervous condition is so closely linked with endocrine disturbance that it is very hard definitely to unravel the complicated course of events. For instance, alimentary glycosuria—renal diabetes—is common among sufferers from enuresis; again the connexion between lymphatism—i.e., lymphatic hyperplasia and thyroid enlargement—and enuresis is classical. In both instances the system generally is involved. A neuro-muscular disturbance not caused by a local gross lesion is a condition secondary to general bodily or mental abnormality. We must then in all cases consider very specially the nervous system as a whole.

In the course of normal development children acquire, first, the power of action and, secondly, the power of restraint—of inhibition. In the case of the organic function, urination, the action is automatic from the first. Restraint and inhibition however have to be acquired. The acquisition requires effort: this effort must be called for and called for early, and it must be sustained by the support of a healthy body and a restful mind.

Rational treatment postulates definite diagnosis: this in turn demands thorough examination of the child, his history and his environment. Is the symptom present as part of nervous hyper-excitability—general or local—or is it an indication of abnormality of the will? Accurate diagnosis is often extremely difficult, associated as it is with so many human influences. With care however it ought to be possible to determine whether the child with respect to this symptom is in the former or latter category, and if the latter whether he is anxious or careless, whether he purposely does it to annoy or does it largely without knowing.

Treatment must be based on thorough complete examination, it must deal with the whole child and his daily life, and it must be essentially individualistic. Optimism and patience are great adjuvants in all dealings with children, and never more so than in these cases. But do we always realize all that is implied in the words "patience" and "individual care"? Let me quote to you some words of Miss Macdowall in relation to the care of mentally defective children:—

> It is quite ordinary for a young child to come to school with the habit of emptying his bladder so constantly that he never seems to stop. There is nothing for it but the most dogged perseverance. We attend to such every ten minutes when they are awake. After each meal it is necessary to shorten the time to five minutes. If the

teacher is keen and is determined to win, in about a month the time can be lengthened considerably. The first stage of sleep, say from 7 to 9 p.m., is the most critical time; the child should be attended to at the end of thirty minutes, then thirty minutes again, then sixty minutes. After that, as a rule, he will go dry till 10.30, then until 2 o'clock, and from that until waking, though we have had to attend to him between times.

In Orphan Homes, Sheltering Homes, &c., &c., enuresis is very common and very intractable. But it is just in these institutions that thorough individual, patient care is impossible.

Sure measures of prophylaxis and therapeutics must equally be based on patient care and the absence of fussiness.

The field of pædiatric work is childhood. The pædistrist studies the child in health and in illness. The fact of the existence of *childhood* is his *raison d'être*. He is fortunate in that his speciality is limited to no system or part of the body; is not connected with any special microbe, method of investigation, therapeutic procedure or vocabulary. He has at least a good opportunity of maintaining a balanced mind and of refraining from undue enthusiasm in any particular alley of general medicine. He learns early that patient individual care often works wonders, whether employed with or without a *specific* line of treatment.

At the present day the treatment of enuresis, apart from various generally accepted details, is largely conducted along one or more of three directions. The first is suggestion, the second is medicinal therapy—largely by means of endocrine preparations—and the third is mechanical and surgical treatment. With the third I do not deal, except to say that any effects produced are probably largely psychological. Discussion of these first two aspects—wide enough in themselves—will occupy the remainder of the allotted time.

All natural education is suggestion. The moral, mental and, to a large extent the physical, growth and development of a little child, is healthy or abnormal according to the conscious or subconscious suggestive effect of his human and material environment. Suggestion is vastly important to a child. The difficulty does not lie in the evaluation of suggestion, but in determining, in relation to the individual child, what is the best form of suggestion, how it can best be applied and in what manner it can best reinforce, and be reinforced by, those other methods of dealing with the child—methods which his condition demands and his environment calls for.

The difficulty is insuperable without a sound diagnosis. When we have determined that the individual child suffers from a psychopathy, and that this is of a certain nature, only then are we in a position to employ rationally any treatment along the lines of suggestion. Thus surely founded, suggestion-treatment is one of our most important means of dealing with this symptom.

In the consideration of medicinal treatment, reference should be made to two preliminary points.

(*a*) The greatest tonic—nerve tonic—in childhood is rest, mental and physical rest. An afternoon nap is often worth its weight in gold. Associated with a douche of cold water in the morning and suitably restrained gymnastic exercises, the vaso-muscular system is aided and fortified—a particularly important indication in relation to this symptom.

(*b*) The other point is the extreme importance of the state of the digestive canal. Any treatment, indeed all treatment, must commence by efforts being made to secure a healthy gastro-intestinal condition, and all treatment must *follow* on most rigid regulation of the diet.

Of drugs most in use to-day, probably the commonest are alkalies, thyroid extract, belladonna, and perhaps calcium, camphor and pituitrin. All these have keen advocates; treatment by any one is sometimes successful, sometimes not.

Much time has passed since the suggestion was first made that treatment by small doses of thyroid was a satisfactory cure for adenoids and nocturnal incontinence. We are, however, still largely in the dark as to the signs and symptoms of minor degrees of hypothyroidism, as to the indications with regard to the dosage of thyroid extract, and as to the effect of thyroid therapy in different abnormal physical conditions,

In the administration of belladonna we have a most useful aid to suggestion. The employment of other drugs is called for in proportion as the general condition seems to demand.

As we have met to discuss a subject which has already been discussed for centuries, I refrain at the outset from entering into details of theory, advice and practice. I therefore conclude my remarks by saying that enuresis is a common and troublesome symptom, far more common and more troublesome than it ought to be. But it is not a thing apart, to be dealt with by the devotees of some fashionable cult. It is a symptom of defective health in a child, and its only rational treatment is that of the whole individual child and his environment.

Mr. Ralph Thompson

said that he found it desirable to discuss, first, certain cases of physical defect, though Dr. Fordyce, in his opening remarks, seemed to wish that aspect excluded. He thought enuresis meant something more than Dr. Fordyce said; it would be found to mean a deliberate bed-wetting. The cases he had to show were most incontinent in bed.

With regard to cases of ordinary enuresis as the surgeon saw them, he had had very good success with catheterizing and irrigating the bladder with increasing quantities of fluid, in cases which had not responded to drugs, such as thyroid and belladonna. He had had about ten cases of that nature. He got the patient to suspend the act two or three times during the bladder-emptying process. He did not say that that method should be used in all cases. Even in hospitals and in homes intended for these cases, it was very important to exclude certain definite physical conditions which might be accompanied by somewhat anomalous symptoms. He remembered, for example, a case of tuberculosis of both kidneys, in which the child was passing clear urine. He showed him at a meeting of the Section of Urology of this Society, and was laughed at for showing what was only an example of 10,000 similar cases in London. A month later, that patient died of tuberculous meningitis. After death the bladder was found not to be diseased, but there was tuberculosis in both kidneys. At the homes for enuresis it was very important that the medical officer should be warned against the possibility of spina bifida occulta being present, as the speaker had seen that condition cause enuresis; also vesico-vaginal fistula. He also laid stress on the fact that many of these cases of enuresis had increased knee-jerks and a high blood-pressure; this enabled the medical man to have something more tangible to go upon than the mere enuresis, and the direction of the patient's attention to other conditions might do much to check the habit.

With regard to congenital deformities in the lower urinary tract, he would

give his experiences in seventeen cases of such deformities, apart from hypospadias. He intended to refer to patients with ectopia vesicæ and true epispadias, and in so doing to correct certain prevalent ideas, some of which were perpetuated in text-books. He asked members to look upon these deformities as being clefts of the bladder. There was the complete cleft or ectopia; there was the incomplete cleft below, which was true epispadias; there was the incomplete cleft above, called urachal fistula. The groove on the dorsum of the penis in true epispadias was not the urethra, but was simply a groove between the two corpora cavernosa. The sooner men got rid of the idea that it was an abnormal urethra, the better for their patients.

With regard to urachal fistula, he had seen only one case—years ago—the details of which he did not now remember. He would like to hear of such a case now, as he believed urachal fistula might afford a clue to the cause of the trouble. The first two conditions he mentioned could be coupled together clinically, because in all the cases he had to show the pubes were widely separated from each other, and there was no symphysis pubis. That applied equally to epispadias and ectopia vesicæ. (Slides shown.)

He showed a slide of ectopia vesicæ, and asked Members to compare the relative size of the extroverted bladder in a child aged 2 months, with that of a child, shown in the next slide, aged 4½ years, the infant having the larger bladder.

When should operation be done on these cases of ectopia vesicæ? His reply was, in the first two months of life, for both æsthetic and surgical reasons. There was a larger bladder to deal with than in the older baby, and by having the operation done early there was a good chance that control of urination might be set up. The child shown was operated upon by him at the age of 2 months; he sewed up the bladder, and there was good union. Death took place at 14 months, from broncho-pneumonia, and the mother assured him the child had been holding its water much better since the operation. Three cases of ectopia vesicæ on which he had operated had been successes; he had had one failure in which the child died, and from this he learned much. Put shortly, the lesson from that case was, that having made the suture of bladder and subcutaneous tissues, time should not be spent in making the tissues look neat, as it jeopardized life in these very young patients. The patient aged 4½ years did not get control, and that supported what he said as to the wisdom of early operation. He brought down the rectus from the right side, split it, and brought it round to the orifice of the urethra. It was not a great success; the child was mentally defective, and was not very well looked after. While at home she suffered from incontinence, but when in hospital she held 10, 12, 14, and 25 oz. of urine.

The next illustration was that of a boy, older, with very widely separated pubes, and a wide separation between the legs. He was one of the patients he (the speaker) had shown that day. On account of the great breadth between the thighs he had inguinal hernia.

The next was a drawing of a case of epispadias in a boy aged 15 years; the opening on the dorsum of the penis and incomplete cleft of the bladder could be clearly seen. He was in a miserable condition, and was willing to have anything done for his relief. He (Mr. Thompson) had great objection to transferring ureters to any part of the intestine, or transferring the bladder to any part of the intestine. He was aware that Sir Harold Stiles and Sir Berkeley Moynihan had published most successful cases, but every surgeon could not expect to get such good results. And it must be remembered that

the operation was a final one, and that if it failed death would probably take place. As he had said in an article in the *British Journal of Children's Diseases*, it should only be attempted if other operations had failed in their object. He therefore thought a muscle might be brought down round the opening, so as to get it to act as a sphincter. The rectus seemed to promise well, therefore some dissections were done on the rectus. The vascular and nerve supply seemed to be quite satisfactory. When one divided the rectus at about the level of the navel, the epigastric artery was divided with it; then the rectus was split and turned down, one part being placed on each side of the orifice of the epispadias. The twelfth dorsal nerve was found to pass into the rectus low down. It split into two branches, which passed up to supply two-thirds of the rectus which lay below the navel. Within three weeks of that operation being done, the boy was holding his water quite satisfactorily at night; at the end of five weeks he held it for two hours, at the end of seven weeks for three hours; he then passed it in a good stream before a number of medical observers. The patient eventually died from double renal calculi; his death could scarcely be held to have any relationship to the operation. [Slides showing the stages of the operation were exhibited.]

The next slide exhibited referred to the case of a girl on whom he had operated, splitting the rectus and bringing it round the orifice of the vagina and urethra. In that case he buried the rectus deeply and anchored it to what he considered would be the position of the central point of the perineum. She did remarkably well while in hospital, and continued to progress satisfactorily afterwards. Three months later she was exhibited herself before a Section of the Society, and was quite dry. He thought she would need to be medically trained so as to bring the rectus into play as a sphincter, but she could hold her water one and a half hours even now.

He had had a case of glandular epispadias, and Mr. Joly had also reported one. In this case the opening was on the dorsum of the glans, and there was a normal urethra within about two inches of the end of the glans, then the normal urethra opened on the surface at the dorsum. Probably it was not causally related to incontinence, but was only a coincidence. Washing out the bladder with increasing quantities of fluid soon put a stop to the patient's incontinence. There was a double bend of the urethra, and possibly some obstruction existed, and thus there might have been an anatomical cause for the incontinence.

Dr. James M. Smellie.

After the exclusion of gross disease of the brain and spinal cord, epilepsy, mental deficiency, genito-urinary and other diseases, and local malformations, &c., there still remain a comparatively large number of children in whom enuresis is a most distressing symptom. I limit my remarks to this group.

The normal healthy infant, with careful training, should have some control of micturition before the age of eighteen months, and such control should be complete, even in the badly trained child, by the time the second birthday is reached. There are, however, many children over three years of age the subjects of urinary incontinence. Such incontinence may be nocturnal, diurnal, or both. In many the condition is of occasional occurrence only, and then usually by night; but in some, fortunately by far the smaller proportion, this condition recurs so frequently as to make their lives a misery to themselves and others.

Naturally, it is only these more severe cases that are seen in the out-patient room, and of forty such cases personally observed during the last eighteen months the enuresis was nocturnal only in twelve, nocturnal and diurnal in twenty-eight, and in none did the incontinence occur by day only. Further, in twenty-four of these children the enuresis occurred every night and in sixteen every day.

In some children, the subjects of this complaint, the normal control of micturition has never been obtained, whereas in others, after a varying period of continence, it may be following an acute illness or it may be from no assignable cause, the inability to control micturition has suddenly returned. Fifteen of the above-mentioned patients had been continent for some period. In twenty-two the incontinence had persisted since infancy and in the case of three patients there is no note.

A fact of some interest, possibly of ætiological importance, to which attention has been drawn by many writers, is the increased frequency or precipitancy of micturition; indeed this may be the symptom for which treatment is sought and the incontinence only revealed by questioning. I noted its presence in 60 per cent. (twenty-four) of my cases.

What part, if any, heredity may play, cannot be stated, but it is instructive to find that in nine of these cases there was a history of either one or other parent having had incontinence in childhood, and in four a history of other children in the family having been similarly affected. In twenty-six cases the family history was definitely negative.

Except for the exclusion of definite signs of disease the physical examination of these children does not assist. I have been particularly struck by the good physical and mental development and well nourished appearance of these little patients; it is very exceptional to find their state otherwise. Detailed examination of the nervous system reveals normal reflexes, &c., in the large majority of cases, and it is only in a small percentage that a brisk knee-jerk is found. On the other hand some degree of nervousness, but without any definite physical signs, is stated to be present in nearly half of these children.

I have records of the systolic and diastolic blood-pressure in thirty cases, in all of which the readings are within normal limits, as given by Griffith [1].

For obvious reasons urinary examinations of out-patients are restricted to fresh specimens obtained at the time of examination. I append a table showing results of such examinations in all my cases and contrast them with an equal number of specimens obtained under similar circumstances from children suffering only from other minor ailments.

	Enuretics	Controls
Albumin	0	0
Sugar	0	0
Acidity (as HCl) average	0·116 per cent.	0·116 per cent.
Urea—average	1·6 "	1·65 "
Diastase—average	12·5 units	10·8 units
Microscopical examination ...	*Nil*	*Nil*

Simpson [2] divides his cases into (*a*) those with a "normal" urine, (*b*) those with an extremely acid urine, (*c*) those with an alkaline urine, and (*d*) those with bacilli in the urine, but my figures are at variance with his divisions. In the first place every urine examined was found to have an acid reaction, and in the second place in no case was a hyperacid urine, so frequently assigned as the cause of the incontinence, revealed. It is doubtful if it ever occurs, except perhaps in those children who have been under

treatment and whose consumption of fluids has been reduced. Children with bacilluria should not be included under the diagnosis of "essential" enuresis, and any such that were encountered during this investigation have been discarded from consideration.

The only possible deduction from these figures is that there are no appreciable urinary changes in enuretic children, and that in particular there are no grounds, when treating them, for any restriction of their normal amount of fluid. Such restriction can do no good; it may do harm.

The relationship between sleep and enuresis is always interesting and has been the subject of much controversy. Courtin [3] has recently published some observations on this point. He tested with graded stimuli at fixed times during the night, fifteen children who suffered from enuresis and forty-one with the normal control. The results he obtained from these experiments led him to conclude that abnormally deep sleep has no causal connexion with nocturnal enuresis.

The customary and generally recognized lines of treatment of this troublesome complaint yielded, in my hands, extremely disappointing results. In such children as appeared to have enlarged or septic tonsils or adenoids, carious teeth, phimosis, &c., these conditions were efficiently dealt with by my colleagues at the Children's Hospital, but without any appreciable benefit resulting so far as their incontinence was concerned. Moreover, the well-known "anti-spasmodics," belladonna and hyoscyamus, also thyroid and pituitary (separately and in combination), failed to produce much result within a reasonable time. I hold no brief for such treatment as injections of serum into the perineum, the retro-rectal region, the epidural space or lumbar puncture, and these methods were not attempted.

The attendance of these out-patients is frequently sporadic; this renders it difficult to obtain accurate results, but after the elimination of those who attended for less than a month my results were the following:—

Drug used	Number of cases treated	Average duration of treatment	Number of cases definitely relieved
Tinct. belladonna	6	... 6 weeks	... 1
Tinct. hyoscyamus	7	... 8 ,,	... 2
Thyroid and pituitary (separate and together)	6	... 5 ,,	... 2

To sum up, a total of nineteen patients were treated, the average duration of treatment was just over six weeks, and yet in only five cases (25 per cent.) was any real alleviation obtained.

It was the appreciation of these results which led me to turn to the study of the physiology of normal micturition, in the hope that from this source some less empirical and more logical methods of treatment might be derived.

The Physiology of Normal Micturition.

The bladder receives its nervous supply from the pelvic plexus. This plexus is constituted by the junction of the hypogastric nerves from the inferior mesenteric plexus, and the pelvic nerves from the sacral plexus. The pudic (or pudendal) nerve innervates the urethra but does not send any branches to the bladder.

The pelvic nerves are the motor or parasympathetic nerves, their stimulation producing contraction of the bladder and relaxation of the urethra. The hypogastric nerves, on the other hand, are theoretically the inhibitory

nerves; and in the cat, at any rate, these two nerves are entirely antagonistic to one another, but in man it would appear that the sympathetic hypogastric nerves cannot inhibit the bladder to any marked extent (Elliot) [4].

The normal tonus of the urethra keeps its lumen occluded and it is only open during the passage of urine. In fact, it is this tonic contraction that retains urine in the bladder, the normal act of micturition consisting of a sustained contraction of the bladder with a simultaneous relaxation of the compressor urethræ. To be more explicit, the vesical opening of the bladder is guarded by a sphincter composed of unstriped muscle fibres which are continuous with the middle circular fibres of the bladder, so that, although the compressor urethræ keeps the urethra closed the passage of urine through the internal meatus is actually prevented by the tonic contraction of this internal sphincter. In animals, however, the pelvic nerves of which have been severed, it is the compressor urethræ which prevents the escape of urine (Barrington) [5]. Accordingly the normal tonic contraction of this internal sphincter must be dependent, partly, if not entirely, upon the pelvic nerves.

Barrington [6] has demonstrated the following five reflexes in normal micturition :—

(1) Distension of the bladder leads to its powerful contraction; (2) running urine through the urethra leads to a powerful contraction of the bladder; (3) feeble transitory contractions of the bladder are produced by distension of the posterior urethra; (4) running urine along the urethra leads to its relaxation; (5) relaxation of the urethra is obtained by contraction of the distended bladder.

As he observes, these first two and last two reflexes are of such a nature that when one begins the others are automatically brought into action, so here they need not be considered further. The third, however, although its use does not seem evident, would appear to warrant a closer scrutiny. Both its afferent and efferent fibres are contained in the hypogastric nerves, as division of the spinal cord, the pelvic nerves, or the pudic nerves does not abolish it; but it no longer occurs when the hypogastric nerves have been divided [6].

The Etiology of Enuresis.

It has already been shown [5] that after division of the pelvic nerves urine may pass through the internal meatus into the posterior urethra, and now it can be appreciated that, provided the hypogastric nerves are intact, the resulting distension of the posterior urethra will cause a contraction of the bladder. Therefore, should any condition be present disturbing the function of the pelvic nerves or upsetting the tonic contraction of the internal urethral meatus, the third of the previously mentioned reflexes will then occur. The amount of urine in the bladder will be of minor importance, the reflex being just as likely to occur when the quantity is small as when it is large.

If these points be borne in mind it would appear as a logical sequence that the cause of the so-called " essential " enuresis of children is an atonic condition of the internal urethral sphincter, due either to some hypo-function of the pelvic, that is the motor, nerves to the bladder or to some lack of tone on the part of the unstriped muscle fibres themselves—possibly a combination of these two factors. This theory is in keeping with the well-known clinical fact that vesical distension is by no means a necessary antecedent to the involuntary escape of urine. Moreover, the success in treatment obtained in some cases by elevating the foot of the bed is thus readily capable of explanation. By

this means the urine must then collect in the fundus of the bladder, and there will be no involuntary evacuation until it has reached the level of the urethral orifice. During the daytime the urine may be held by the voluntary contraction of the compressor urethræ, which will account for the diurnal continence possessed by many of these children, and also for their increased frequency of micturition.

It is noteworthy that Bokay [7], professor of pediatrics at the University of Budapest, is stated to consider that incontinence of urine in children manifests itself by atony of the sphincter added to the spasmodic contraction of the muscle of the bladder, but he assumes the spasm to be the cause and not the consequence.

Treatment.

Admitting this ætiology, what is the treatment that should be adopted, and what drugs, if any, are there that are likely to be of benefit?

The atropine group (belladonna, hyoscyamus, &c.) have a depressant action upon the parasympathetic nervous system, and in addition paralyse smooth muscle; it seems probable that the former action will be obtained with a smaller dose than the latter action. If my theory is correct this depressant action upon the parasympathetic might aggravate the condition. The success obtained by the exhibition of atropine, therefore, may be due to its action upon the involuntary muscle, and to achieve this it is necessary to administer the very large, and it may be harmful, doses that have been advocated. This would not appear to be rational therapy.

Two other alkaloids, well known and widely used for their actions upon the involuntary nervous system, and which incidentally have been claimed to be of some success in the treatment of the condition under discussion, are adrenalin and pituitrin. Cushny [8] states that adrenalin stimulates the myoneural junctions of both the motor and inhibitory nerves indiscriminately, while Dale [9] has shown that pituitrin directly stimulates the unstriped muscle irrespective of its nervous supply. But what is here desired is some drug which will affect the motor myoneural functions with, in addition if possible, the unstriped muscle direct, but leave unaffected the inhibitory functions. Ergot would appear to satisfy these demands, as its action is mainly due to the two alkaloids, ergotoxine and ergamine. Ergotoxine, in small doses, stimulates the motor myoneural functions, but has no effect upon the inhibitory functions, while ergamine acts directly upon the unstriped muscle cells [8]. Ergot thus seems peculiarly suitable for the treatment of this malady, and by its administration I have obtained results considerably in advance of those I have obtained by other drugs. To a child of 4 or 5 years I give 5 minims of the liquid extract of ergot, with $2\frac{1}{2}$ minims of extract of liquorice (which successfully conceals the unpleasant taste) in a drachm of peppermint water. This is given three times a day for a week, and if no improvement has been effected the dose may be gradually increased. For older children a larger dose may be prescribed, but I have not found it necessary to exceed half a drachm a day. The children take this medicine readily, and no unpleasant symptoms have been observed. I do not claim that treatment by ergot is new, as both Ruhräh [10] in 1912 and Simpson [2] in 1913 mention this drug, but neither quote any precise reasons, details or results, although Simpson goes so far as to state that he has rarely seen any good results follow after belladonna has been used unsuccessfully.

Electrical treatment by means of a weak faradic current, with one

electrode on the spine and the other on the perineum or pubis, was tried in a few cases. There are many reasons to be urged against the application of such treatment to such regions of the body, especially in older children, and the results obtained after a few weeks of this treatment did not appear to warrant its continuation.

However fascinating this medicinal point of view may be it is necessary to remember that treatment must be directed to overcome the lack of nervous co-ordination. Drugs can only assist the patient to obtain the normal control, and therefore some form of re-education should also be adopted in every case. The method I have found most satisfactory is that recommended by Herrman [11], namely: First, the child must urinate at regular stated intervals. I have found it best to begin with two-hourly intervals and gradually to increase these as the continence improves. Secondly, the whole contents of the bladder must not be evacuated at one time, but the patients should be taught to pass a few drachms, then pause for about five seconds, then another few drachms, and so on until the bladder is emptied. A practical difficulty that arises in connexion with treatment on these lines, and one which tends to nullify accurate observation of results, is the indeterminate degree of co-operation on the part of the parents, and for this reason I have not included those patients to whom this method has been recommended.

Thirteen cases have now been treated by ergot for an average duration of six weeks, with the following results:—

(1) Discharged completely cured, five cases. (2) Alleviation so marked that they ceased to attend, four cases.

Conclusion.

In conclusion I have endeavoured to show that the contraction of the bladder in the "essential" or "idiopathic" enuresis of children is a reflex contraction, due to a hypotonic condition of the unstriped muscle fibre forming the internal urethral sphincter. The exhibition of ergot would appear to be the best method of treatment, but the small number of cases so far treated does not permit of any decisive results being stated.

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Dr. Cameron

said that in his view it would be better if it were frankly recognized that the common variety of enuresis was hysterical in origin. In general, one had to deal with a highly suggestible and sensitive child, whose failing weighed on its mind and was a real source of misery. The mother might be in doubt as to the child's real attitude to the matter. An obstinate carelessness was sometimes assumed, but that was of the nature of a compensation process, just as the childless woman might proclaim her dislike of children. But of the same child it was usually agreed that he was unduly sensitive to criticism, and easily cast down by blame. Upon such a nature the suggestions of incontinence derived from the mother acted strongly. Hysterical manifestations in child-

hood were determined for the most part by maternal anxieties. Mothers did not commonly fear for their children that they would become paralysed, or blind, or mute, and these manifestations were uncommon because all such suggestions were lacking. The hysterical symptoms of childhood centred around the sensitive gastro-intestinal tract and the urinary apparatus; pica, bulimia, polydipsia, anorexia nervosa, pollakiuria, enuresis, were of everyday occurrence. With a sensitive child, if too much was made of an occasional accident when the bed was wet, the fear of recurrence was easily implanted. Often all the stress was laid upon the failure, none upon the growing control. From early infancy the child grew accustomed to the vexed and offended face of his mother or nurse. "Again!" was the constant cry. He thought enuresis could be cured by anyone who could oust from the child's mind the miserable feeling of impotence and the constant dread of the accident. Self-confidence must be restored and this could only be done by someone confident of his own powers and capable of communicating a feeling of confidence to the child. The devices which he used to assist the child to regain the confidence which had been lost were dependent on the age. Up to the age of 4 or 5 years a brightly coloured rug might be used and if it was well stage-managed might be instantly successful. In older children other devices had to take the place of such simple suggestions. The difficulty in general was to control the atmosphere in the home. In schools and still more in reformatories and training ships the cure was difficult for a different reason. Although functional nervous disturbances were less common among the poor than the well-to-do, enuresis was an exception. The burden of washing thrown upon the mother forced her to concentrate on the child's failing and the mother's pessimistic attitude had its usual ill-effect. In schools, if punishment were meted out or other boys became critical, the disorder could become epidemic. Sometimes the hysterical suggestion extended to the bowel and incontinence of fæces was added. In such cases there was anæsthesia of the rectum.

Mr. R. H. Anglin Whitelocke (President)

thanked those who had contributed to the discussion, from which, he said, he had himself learned more about the subject than he ever knew before. He was especially struck by Dr. Cameron's clear exposition of the psychology of the condition.

Section for the Study of Disease in Children.

President—Mr. R. H. Anglin Whitelocke, F.R.C.S.

Herpes Recurrens.

By J. D. Rolleston, M.D.

First, I would like to say a word upon the nomenclature. By herpes recurrens we mean every form of herpes which is not herpes zoster. The French are better off than we are in this respect, because when they say "herpès" they always mean catarrhal herpes, febrile herpes, herpes facialis, or herpes labialis, as distinct from herpes zoster, which they call "zona."

The patient is a boy, aged 13 years, who, according to the history given by the mother, first developed a severe attack of herpes on both cheeks after an attack of pneumonia at the age of 5 months. Since then he has always been delicate, and has been liable to attacks of bronchial asthma, but he remained free from further attacks of herpes until about two years ago, and since then he has had about half a dozen attacks. The eruption usually appeared on one or both cheeks about the third day of what seemed to be a feverish cold. The eruption did not leave any scars. He had measles, chicken-pox and whooping-cough at the age of 2 years but none of these diseases were complicated by herpes.

I first saw him on February 16, 1924, when he was admitted to the Grove Hospital with a mild attack of faucial diphtheria, on the second day of disease, when he presented clusters of herpetic vesicles on both cheeks. During the next three days vesicles continued to appear on the forehead, cheeks and lobe of the left ear (*see* fig. p. 50). The patient was given 9,000 units of antitoxin on admission and the throat became clear in a few days. Apart from a trace of albumin in the urine on the day of admission no complications referable to diphtheria ensued, nor did any serum reaction develop. The temperature was 99·8° F. on the day of admission and became normal the next day. By March 6 the herpes had mostly disappeared, but on March 16 a fresh crop developed on the upper and lower lip and angle of the left nostril, and during the next two days similar lesions appeared on the cheeks and in the right supra-orbital region. The temperature did not rise above 99·2° F. On April 12 a small patch of herpes appeared on the right cheek, but the eruption on this occasion was confined to this region.

There is no doubt that this case is one of herpes recurrens, as distinct from herpes zoster. An interesting point about the boy's history is, that though he had had three other infectious diseases besides diphtheria, viz., measles, chicken-pox and whooping-cough, he never had herpes with them; it seems that there are certain infectious diseases which are prone to produce herpes. Those most frequently associated with herpes are pneumonia, malaria, and cerebro-spinal fever, namely, about 40 per cent.; then, after a long interval, come scarlet fever, with 6·5 per cent. [1], and diphtheria with 4 per cent. [2].

A few years ago a French text-book [3] dealt with the question of herpes and scarlet fever, and remarked that my estimate of 6·5 per cent. seemed to be an exaggerated figure. About a year later, a very extensive epidemic of scarlet fever occurred in Geneva, which was reported by Saloz and Schiff [4], in a paper read before the Academy of Medicine, Paris. They quoted this remark about my figure of 6·5 per cent. being exaggerated, and said 11 per cent. of their cases of scarlet fever had herpes.

Why some diseases are associated with herpes, and others not, I do not know, and I shall be glad to hear some statement throwing light on that point. About sixty years ago Hebra [5] mentioned the fact, and said the profession were in the dark as to why intermittent fevers and certain other diseases should be associated with herpes.



Herpes recurrens has been very much "in the air" lately in connexion with lethargic encephalitis: but I shall not now enter into that question, as it would occupy most of the time of the meeting. Those interested in the subject will find a good résumé in the Ministry of Health's Report by Dr. Allan Parsons [6] and in the more recent Belgian Report [7], on the work done by French observers, especially Levaditi, Harvier and Nicolau. Still more recently a paper was written by Bastai and Busacca [8], of Turin, who injected the cerebro-spinal fluid and blood of herpetic patients into rabbits and reproduced herpetic lesions in the cornea and the characteristic encephalitis, not only during the time the patient had herpes, but also in the intervals; thus the patients seemed to be in a state of latent infection.

Also, Nicolau and Banciu [9], of Bucharest, carried out a series of inocula-

tions of herpes, and managed to produce relapsing herpes with the vesicular fluid obtained from a case that had recurrent herpes, indicating that there are two kinds of catarrhal herpes, one which is accidental, the other which is more persistent and more worthy of the name recurrent herpes.

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Dr. F. PARKES WEBER said that many cases of recurrent herpes of the so-called febrile or catarrhal type (that is to say, not herpes zoster) occurred with no very obvious infection, or with only a very slight infection, shown in children and young persons by a little "indigestion," slight coryza, &c. Such attacks were often "pre-menstrual" in women. Instead of the lips and nose, the cheeks and various other parts might be affected. Women sometimes had the eruption on the buttocks or thighs, especially at or just before the menstrual period; occasionally only one buttock was affected. The herpetic eruption might be arranged in a line, on one buttock or one thigh, and in that way it might simulate herpes zoster. Recurrent attacks of penile herpes of the catarrhal type were well known in men, and men and boys, like females, might also suffer from recurrent herpetic attacks on the perineum, buttocks, or thighs.

Case of Sclerema.

Shown by W. G. WYLLIE, M.D., and G. A. HARRISON, M.B. (for HUGH THURSFIELD, M.D.).

GIRL, aged 5 months. On the fifth day after birth, the mother first noticed that the child's arms and legs were stiff. Gradually the stiffness of the limbs became less, but hard lumps were noticed in the skin. These lumps are now present in the skin of the back and of all four extremities, hands and feet excluded. On the outer aspect of both upper arms are larger swellings which are soft and semi-diffluent; that upon the left arm was aspirated and found to contain a white pultaceous material. Microscopically this substance showed masses of crystals, most of which were doubly refractive. The child was born at full term, of a normal labour, and weighed 8½ lb. She has always been difficult to feed and the stools are usually constipated. Both parents are healthy. There are nine other children, one of which suffered from marasmus in infancy. Another child died of marasmus.

REMARKS ON THE CASE.

I. Dr. W. G. WYLLIE.

In regard to the diagnosis, there are three conditions bearing considerable resemblance to one another, which must be considered. These are: sclerema, pseudo-sclerema, and sclerodermia. In sclerema, the age of onset is from birth to 6 months. In pseudo-sclerema the onset is usually in the first week of life.

Both conditions appear to be connected with an abnormal state of the fat in the deep layers of the skin and in the subcutaneous tissues, and sometimes there is present a slight overgrowth of the connective tissues in these areas. The points of difference between the two diseases, according to Professor Langmead, are, that in sclerema the children affected are usually undersized, weakly, and often premature, while in pseudo-sclerema the affected infant is very often neither undersized nor weakly. In sclerema the changes usually commence in the skin of the thighs, and spread upwards over the buttocks, trunk, and on to the neck, and there are no nodular formations in the skin, while pseudo-sclerema is characterized by firm, nodular circumscribed patches in the subcutaneous tissues. In neither condition does the skin pit on pressure. Constitutional disturbances are nearly always present in sclerema; the temperature is usually subnormal, and the pulse and respirations are slowed, while in pseudo-sclerema the temperature is usually normal, and the pulse and respiratory rates are unaffected. Nearly all cases of pseudo-sclerema recover, the subcutaneous nodules disappearing gradually, some of them softening before their final disappearance. But in nearly all cases of sclerema the patients die in a short time. Considering its chronicity and the presence of firm nodules under the skin, I think it probable that this present case is one of pseudo-sclerema.

Dr. John Thomson considers that pseudo-sclerema may represent an infantile form of the diffuse symmetrical sclerodermia which is sometimes seen in older children.

II. Dr. G. A. Harrison.

I cannot yet give much information on the chemical pathology of this case, as the investigation has not proceeded far.

Various hypotheses have been advanced to account for the chemical findings: (1) One suggestion is, that owing to the temperature being subnormal in a proportion of cases, the fat may solidify out. That seems unlikely, because until the point of death there is only a drop of 3 or 4 degrees. (2) Another suggestion is that there is a diminution in the oleic acid and a corresponding increase in stearic and palmitic acid fat. Dr. McNee showed a case before the Section of Dermatology a few years ago, and the analysis of it bore out this contention, but nothing has yet been advanced either to support or to refute it. Dr. McNee has prepared the sections in the present case, and members can see that there is a large increase in doubly-refracting acicular crystals, which are probably cholesterol esters. The raised melting-point may be explained by the presence of the cholesterol esters, rather than in an abnormal increase in the palmitic and stearic acids. Thus (3), one may imagine there is a general disturbance of fat metabolism, and in such case one would expect to find a high blood cholesterol. The blood cholesterol in this case, however, was found to be 59 mg. per 100 c.c., a definitely low figure. Or (4) there may be a local deposit of cholesterol ester owing to local changes. Lastly (5), it is not impossible there may be an excess of cholesterol in these tissues owing to abnormal feeding. This child was fed on No. 1 Virol on the first day of life, and until its admission to hospital it was fed on a mixture of Virol and cow's milk in the daytime, and cow's milk and whisky at night.

I have wondered whether there may be an associated disturbance of carbohydrate metabolism, but the blood-sugar is subnormal, namely, 0·067 per cent. half an hour after a feed. Another observer has described X-ray appearances

similar to those demonstrated in the present case, and has attributed them to a deposition of lime-salts in the subcutaneous tissues.

DISCUSSION.

Dr. J. W. McNEE said that the present case was the third he had seen. The first was in University College Hospital, and for seeing the second he was indebted to Dr. Donald Paterson. In the present case Dr. Thursfield had kindly allowed him to cut sections. Histologically the condition was interesting because of the cellular reaction which occurred. In the first case, already shown to the Society in another Section by Dr. A. M. H. Gray and himself, there were great numbers of acicular crystals mixed up with the fat and also lying separately in sheaves. They were doubly refracting, and evidently contained cholesterol esters. On analysis he was surprised to find only 0·4 per cent. of the mass was cholesterol ester, the great bulk of material being a combination with fatty acids, a combination which was always doubly refracting to light. Obviously, these crystals acted as an irritant, and had caused considerable cellular reaction in the subcutaneous tissues, with the formation of giant cells.

He had been much interested in the chemistry of the first two cases, and had been waiting for a third case so that there might be a chance of confirming the findings in the others. In his first and severest case, the melting point of the separated fat was 50° C. Dr. Paterson's case was not so severe, and the melting point was 38·5° C. The fat would thus be solid at the body temperature of these infants. He did two normal controls in infants, and the melting points of the fat in them were 32° C. and 34° C., the fat thus being fluid at body temperature. He thought the pathology of the condition could be explained on the view that there was an alteration in the fats, there being an excess of high-melting-point fat over the normal. He would be interested to know what the examination of the present case from that standpoint might reveal.

Dr. F. PARKES WEBER thought this discovery by Dr. J. W. McNee, and others, of the presence of doubly-refracting cholesterin esters in three cases, constituted one of the greatest discoveries in regard to the pathology of sclerema neonatorum since the eighteenth century, when the disease was first recognized. Still, the pathology was not yet quite clear.

Why did not adults present this condition? In 1890, W. P. Northrup, in the *Archives of Pediatrics*, referred to the fact that the fat of adults differed from that of new-born infants; the latter contained more palmitic and stearic acids than the former, and solidified at a much higher temperature, as had been pointed out by Ludwig Langer in 1881.[1]

The result of warming up the affected infants had been very beneficial; it suggested that the special tendency to solidification of infantile subcutaneous fat was intimately connected with this rare infantile disease. The excess of fatty acids in infantile fat might be a factor favouring the separation of free (palmitic and stearic) fatty acids in the exposed and colder parts (subcutaneous tissue) of the body. These free fatty acids would then act as a local irritant, setting up inflammation in their neighbourhood.

S. S. Chalatow[2] made the important suggestion that when irritating fatty acids were in the tissues, there was a tendency for cholesterin to be taken up from the blood and to become combined with the fatty acids so as to form non-irritating amorphous cholesterin esters. The formation of these cholesterin esters, which were at first amorphous, isotropic and non-irritant, might be regarded as the result of a conservative vital reaction on the part of the body. But, according to Chalatow's view, there followed a kind of *circulus vitiosus* action, manifested by the transformation of the non-irritating amorphous isotropic cholesterin esters into irritating anisotropic (doubly refracting) cholesterin esters, which were even more irritating than the fatty acids.[3] Thus a foreign-body

[1] W. P. Northrup, "Sclerema Neonatorum," *Archives of Pediatrics*, Philadelphia, 1890, vii, pp. 1-8.

[2] S. S. Chalatow, "Die anisotrope Verfettung im Lichte der Pathologie des Stoffwechsels," Jena, 1922, p. 174.

[3] Chalatow (*loc. cit.*) is at that part of his book especially discussing what he calls "myelinosis," that is to say, an anisotropic fatty infiltration of the main (chief-functional) cells of organs of epithelial origin, such as the liver and the kidney. He is not (at that part of his work) referring to what he calls "xanthomatosis," that is to say, an anisotropic fatty infiltration of cells of mesenchymatous origin in various organs or parts of the body, such as occurs in cutaneous xanthoma and aortic atheroma.

giant-cell reaction was set up in the affected tissues—a phagocytic reaction similar to that which might be called forth experimentally or accidentally by the introduction of a little silica into the subcutaneous tissue. The cells concerned in such a giant-cell reaction probably belonged to the class of mesenchymatous "wandering cells" ("ruhende Wanderzellen," the "macrophages" of Metchnikoff, the "clasmatocytes" of Ranvier, the "histiocytes" of Aschoff, the "polyblasts" of Maximow and Chalatow, &c.), and the giant-cell reaction was probably of the same nature as the well-known Langhans tuberculous giant-cell reaction; in one case the phagocytic cells were called forth by the presence of tubercle bacilli and in the other case by the presence of irritating and foreign bodies.[1]

Case of Death from Uræmia due to Interstitial Nephritis (? of Congenital Origin); Renal Dwarfism.

Shown by W. G. Wyllie, M.D., for Hugh Thursfield, M.D.

Patient, a girl, aged 12 years and 5 months, always a delicate child with a history of frequent headaches, thirst and weakness of bladder control. She was 7½ in. below normal stature for her age, and 36 lb. below normal weight. Previous illnesses: Measles and whooping-cough three years ago. In the last three months there was progressive weakness of the legs. On April 8 she was unable to stand and had an epileptiform seizure after vomiting. In the next five days she had ten fits in all and had only transient periods of consciousness during that time. On admission to hospital, April 14, consciousness was absent and frequent twitchings of the limbs were observed. A purpuric rash was present upon the limbs and trunk, the skin otherwise being of a pale and muddy colour. The ocular fundi were normal; the abdominal reflexes, knee and ankle-jerks were absent, the plantars were extensor. The heart was considerably enlarged, and loud presystolic and systolic mitral murmurs were present. No urine was passed. Death occurred within twelve hours after admission. Skiagrams of the knee-, ankle- and wrist-joints showed broadening of the epiphyseal lines and irregular calcification.

Post-mortem Findings: The left kidney was small with a granular surface and an adherent capsule: the right kidney consisted of a mere shred of tissue. The bladder was large with hypertrophy of the walls. The heart was enlarged, both ventricles being hypertrophied, especially the left. Early degenerative changes were present in the intima of the ascending portion of the aorta. The brain was congested; the other organs did not show any special changes.

Special attention has been drawn in this country to the skeletal deformities, and the condition is termed renal dwarfism. Several such cases have been shown before this Section; three in 1920 by Dr. Paterson, who showed a further case in 1921. In the *American Journal of Children's Diseases*, Green reported a case, and gave a summary of twenty-seven cases collected from the literature.

Case of Transplantation of the Rectus for Incontinence due to Epispadias.

By Ralph Thompson, M.Ch.

I show this patient to-day. It was the case about which I spoke at the last meeting, in the Discussion on Enuresis. Before the operation the

[1] Cf. Evans, Bowman and Winternitz, "Experimental Study of the Histogenesis of the Miliary Tubercle in Vitally Stained Rabbits," *Journ. Exper. Med.*, New York, 1914, xix, p. 383.

child could only hold about 1 to 1½ oz. of urine; now, two weeks after the operation, she can hold 4 to 5 oz. She still wets her clothing, but she is very much better than formerly. Very careful subsequent medical training is necessary, so as to ensure the best possible result.

This is the fourth operation I have done for the condition. One of the patients died nine months after the operation, from a different condition. The other three are still alive, and hold their water much better than they did before the operation of transplantation of the rectus to form a sphincter vesicæ.

Case of Myelogenous Leukæmia associated with Priapism.

By PEARSE WILLIAMS, M.D., M.R.C.P.

PATIENT, a boy aged 11 years. Painful priapism was the first symptom noted. He has an enlarged spleen the tip of which reached to the level of the iliac crests. A blood count gave 643,000 leucocytes per cubic millimetre, of which 18 per cent. were myelocytes. The priapism in this case is persistent, and some œdema of the scrotum has developed. Advice is asked as to treatment of this particular condition.

DISCUSSION.

Dr. EDMUND CAUTLEY said he had not previously seen a case of priapism in a child with leukæmia.

Dr. F. PARKES WEBER said that *persistent* priapism was recognized as an occasional early symptom of leukæmia, though he had never heard of its occurrence in so youthful a leukæmic patient as the present one. He did not think that with any such severe disease as leukæmia an incision should be made in the penis, though it was justifiable in some cases. He suggested a trial of X-rays, as by removing the leukæmic infiltration one might get rid of the priapism, which was due to secondary leukæmic blood-clot.

In a man (H. B.), aged 33, with myeloid leukæmia, to whose case he had referred in the discussion on a paper by Dr. Gordon Ward, on "Nodular Leukæmia," at the Medical Section of the Royal Society of Medicine, persistent priapism had occurred during the year preceding the one in which he (Dr. Weber) saw him. It lasted eighteen days, until a surgeon incised the penis and let out much leukæmic blood-clot.[1]

Mr. RALPH THOMPSON suggested that the priapism was due to increased abdominal pressure, compressing the veins around the base of the bladder. This child's abdomen seemed to be very much distended.

The Use of X-rays in the Diagnosis of Congenital Pyloric Stenosis.

By GEOFFREY FILDES, M.B. (Radiologist to St. Thomas's Hospital), and J. FOREST-SMITH (John and Temple Research Fellow, St. Thomas's Hospital).

THE diagnosis of congenital pyloric stenosis in infancy depends largely on the finding of a palpable, pyloric tumour on physical examination in a case in which projectile vomiting and visible peristalsis of the stomach are present.

[1] See remarks by F. Parkes Weber, *Proc. Roy. Soc. Med.* (Medical Section), 1912, v, p. 105. Regarding various causes of long-continued priapism, see F. Parkes Weber, *Edin. Med. Journ.*, new series, 1898, iv, pp. 267-271.

Opinions differ as to how often such a tumour is palpable in cases which may later be proved to be stenosis. R. Hutchison states that "the pylorus may be so tucked under the liver that no tumour can be felt." Holt says that "the existence of a tumour is often a matter of uncertainty." In our own series of six cases of proved stenosis, the pylorus was palpable in four and indefinitely palpable in one. In the other, even after the diagnosis had been made by X-ray examination, the thickened pylorus could not be felt, and this occasioned no surprise when at the subsequent operation it was found to lie high under cover of the liver. Whatever may have been the necessity for an exact diagnosis some years ago when the treatment of the condition was almost entirely medical, now, when a majority of practitioners accept modern surgical treatment as offering the best chance of recovery, it is essential that the diagnosis should be confirmed at the earliest opportunity. The use of the bismuth meal provides a means of early diagnosis, and with care does not appear to add any marked risk to the patient.

The material used in this paper consists of six cases of pyloric stenosis: two cases of pyloric spasm in infants, and two cases of duodenal obstruction, all admitted under the care of Dr. Jewesbury at St. Thomas's Hospital in 1923. We wish to thank Dr. Jewesbury for allowing us to publish these results. For the purpose of comparison, we have included one healthy infant of seven weeks, who was given a bismuth meal in order that we could establish the normal.

Technique.

The stomach is washed out two hours before the bismuth meal is to be given, and no food is allowed in the interval. Most of the cases admitted with vomiting have already had this stomach wash as a routine measure, and it is well to arrange the meal at such a time as not to cause this procedure to be repeated unnecessarily. Sodium bicarbonate solution has been used in the preliminary stomach lavage, and a small quantity of the solution has been left in the stomach. It may be possible that the presence of this alkali has to some extent relieved the spasm in the two cases of pyloric spasm in this series. We are awaiting an opportunity to test the radiological findings in a case of pyloric spasm without this preliminary use of alkali. The infant is clothed in a woolly combination suit with gloves and socks in order to prevent heat loss, wrapped in a blanket and taken to the X-ray room. A stomach tube is passed, and $1\frac{1}{2}$ to 2 oz. of a warmed, not too thick, bismuth emulsion is poured into the stomach by means of a funnel. We have never found any difficulty in giving this amount. In those cases in which it is known that there is a tendency to reject a feed immediately after it has been given we have found it advisable not to remove the tube until the infant has been screened. In other cases, where vomiting is delayed for some ten minutes or so after a feed, the tube is withdrawn at once, the blanket discarded, and the infant held in the radiologist's gloved hands behind a vertical screen. Sufficient room is left behind the screen to allow the child to be turned laterally to the left in order to show more clearly in the atrium of the stomach. A low milliamperage with suitable penetration is used. On screening, the points noted are the size of the stomach, the existence of marked peristalsis and especially whether any of the bismuth is passing the pylorus. A film is substituted for the screen, and an exposure of less than a tenth of a second is used. The stomach tube, if not already removed, is now withdrawn, and the patient wrapped in the blanket and taken back to the ward. The giving of the meal, the screening, and the taking of the first film should not occupy more than five minutes. We have not found any

tendency for the infant to vomit the bismuth after the first few minutes. It is again taken to the X-ray room, one and a half to two hours later, and the second film obtained. When this has been done the bismuth is washed out of the stomach and an ordinary feed is substituted.

Possible Disadvantages.

Two objections to the use of this technique are at once suggested. The infant, already exhausted from constant vomiting, may suffer from the shock of handling and exposure entailed. By using the one-piece woolly suit and by using a standard technique the amount of handling and possible heat loss is cut down to a minimum. The second possible objection may result from the use of bismuth. We have made it a practice always to remove any bismuth that may be left in the stomach at the end of two hours, but we have never seen any ill effects arise which could possibly be attributed to its use either in this series of cases, or in others which we have examined. The best argument in favour of the small risk which is added to the chance of recovery of these patients by such an examination is provided by the end results of our cases. The six patients with pyloric stenosis, and the two with duodenal obstruction were all operated on by Mr. C. Max Page within a few days of the X-ray diagnosis—indeed, in one case, within two hours. The immediate mortality was *nil*, and all the patients were discharged from hospital cured, with the exception of one infant who succumbed, in the fourth week after operation, from an infection then prevalent in the ward.

Results.

On screening a normal infant after giving a bismuth meal it is seen that the stomach is high in the anatomical position. Peristalsis is not marked and bismuth is beginning to leak through the pylorus. In no case in infancy have we seen any true duodenal cap. It would be of interest to know at what age the stomach begins to show the adult radiological appearance. A second film, taken two hours later, shows only a trace of bismuth in the stomach; most has passed the duodenum and is seen in the small gut (fig. 1). Fig. 2 shows the appearance in a case of pyloric stenosis which, clinically, was easy to diagnose. Vomiting was projectile, peristalsis was seen, and a tumour could be felt. On screening, there was complete stenosis; the stomach was large, peristaltic waves were marked, and no bismuth was passing the pylorus. At the end of two hours the whole of the meal was still in the stomach (fig. 3). The next case is of interest owing to the fact that the vomit was distinctly bile-stained; this has been known to occur in cases of pyloric stenosis, but is suggestive that the obstruction may be below the second part of the duodenum. It is obvious that the stenosis is at the pylorus and not in the duodenum. Fig. 4 is also typical of pyloric stenosis. The infant in this case was admitted, aged 6 weeks, with a history that vomiting had begun a week previously. In hospital the vomiting was noted as regurgitant rather than projectile. The stools are small and dark green in colour; collapse was marked; no gastric peristalsis was seen and no pyloric tumour could be felt. The patient was treated by stomach washes and was given small feeds of "Ideal" milk and brandy. At the end of five days, though the general condition had improved, the vomit began to show blood, and later became frankly coffee-ground in character. The radiological diagnosis was confirmed the same day by Mr. Page, and the patient was discharged fourteen days after operation.



FIG. 1.—Appearance two hours after meal in normal infant. Bismuth in small intestine.



FIG. 2.—Pyloric stenosis.

The typical radiological findings in a case of pyloric stenosis consist in the failure of any but the smallest amount of bismuth to pass the pylorus at the end of two hours. The opposite is true in the case of pyloric spasm, although the diagnosis may be difficult clinically. Excessive peristalsis may be, and usually is seen, on screening, but almost at once bismuth appears in the duodenum and within two hours the ileum contains most of the meal.



FIG. 3.—Pyloric stenosis, two hours later than in fig. 2.



FIG. 4.—Pyloric stenosis.

It may be stated—although we do not suggest it as a method of treatment—that bismuth in these cases appears to do good. Vomiting and peristalsis became less marked after the meal in both in our series, and in the last few weeks we have had the opportunity of seeing a third in which similar radiological findings were followed by marked improvement in the symptoms.

DUODENAL OBSTRUCTION.

Although there may still be some controversy over the respective merits of medical and surgical treatment in congenital pyloric stenosis, everyone agrees that surgical measures alone are able to benefit the cases of duodenal obstruction. Reference has already been made to bile-stained vomiting occurring in a case of pyloric stenosis. The presence or absence of such bile has usually been held to differentiate the two types of obstruction clinically. Bismuth meals, in which the technique we have already described was followed, were given in two cases of duodenal obstruction already reported to this

Section by Dr. Jewesbury and Mr. Page.[1] It will be seen in these cases that bismuth immediately passes the pylorus and fills the first part of the duodenum, giving an appearance never obtained in the pyloric cases. Two hours later, the condition is not materially altered (fig. 5).



FIG. 5.—Duodenal obstruction of second part, showing some bismuth in first part.

CONCLUSION.

We would suggest that the bismuth meal is unnecessary in cases of pyloric stenosis where the condition can be recognized clinically, but is of the greatest value in differentiating pyloric spasm from pyloric stenosis: that in the presence of bile in the vomit it is essential for deciding the site of obstruction. Further, with care, the advantage of an early accurate diagnosis outweighs the possible disadvantage of the examination to the patient.

DISCUSSION.

Dr. BERTRAM SHIRES said that in opening the discussion on the excellent paper they had just heard he must mention that he had been associated with Dr. Fildes and had been present when most of the X-ray examinations were made. He felt, however, that he must take that opportunity of giving his experiences in the examination of infants by the opaque meal. During the past three years he had examined about a hundred cases by this method. The technique he adopted was the following: the opaque meal was substituted for a normal meal, that was, it was given after fasting for two or three hours.

[1] *Proceedings*, 1922-23, xvi (Sect. Study Dis. Child.), p. 50.

The meal consisted of 1 to 2 fluid ounces of a bismuth mixture according to the age of the child.

The mixture was made up as followed:

Bism. carb.	℥i
Syrup.	℥i
Pulv. trag.	q.s.
Aq. ad	℥i

or—

Bism. carb.	℥v } Sterilize, if to be kept
Pulv. trag.	grs. c }
Elixir sacchari.	min. xx
Water	℥xx

The fluid, which was quite thin in consistency, was given either by spoon or bottle. He had never failed to give the requisite amount by one of these methods.

The radioscopic and radiographic examination could be carried out without the infant's warm clothing being removed. The screen examination was made with the child in either the erect or prone positions but not in the supine, for in this position the bismuth shadow was seen in two distinct loculi—one the cardiac end, the other the pyloric.

He would here diverge for a moment, to mention that a physician in the provinces had recently written to him with regard to a case of gastrospasm, or hour-glass stomach, in a baby, which he had discovered on opaque meal examination. He (Dr. Shires) suggested that the examination had been made in the supine position and advised re-examination in the prone position. This the physician did and reported that the bilocularity was no longer evident but that he still found on gastric lavage that he was unable to empty the stomach except by withdrawing the catheter a little—which he considered was evidence that the hour-glass contraction still prevailed. Dr. Shires suggested the prone position for the lavage which met with a perfectly satisfactory result. It was evident that the catheter emptied the pyloric end but not the cardiac end until withdrawing a little.

To return to discussion of the screen examination: in the normal stomach immediately after ingestion of the meal, some of it was to be seen in the jejunum. True waves of peristalsis were not visible, but rather a general contraction towards the pylorus with small quantities of food trickling through the duodenum. In the first half hour quite a considerable quantity had passed into the ileum. The stomach emptied in 1½ to 2 hours.

This would be demonstrated later.

In cases of stenosis or spasm varying degrees of delay in the passage of food from the stomach were met with. In every case in which there was a palpable tumour there was very definite delay in the early stages and a large residue in two hours. In a number of cases where there was no palpable tumour but in which there was a large gastric residue in two hours—operation or post mortem proved them to be true cases of stenosis. In six cases which were operated upon in which there was slight delay in the early stages but no large residue in two hours, no undue hypertrophy of the pylorus was found. This type of case, of which he had seen many, must be considered as due to spasm, for in the majority of instances they had been relieved by medical treatment. Hyperperistalsis was never visible as in an adult stomach, in that no incisura were demonstrated on screen or film—but small peristaltic ripples were more evident in cases of spasm and stenosis than in the normal. He had not observed any deformity of the pyloric antrum in any of these cases, in fact he (Dr. Shires) thought it would be perceived from the radiograms he was about to show that there was no antrum formation such as was seen in the adult.

Apart from the cases reported that afternoon his (Dr. Shires') cases included two of post-pyloric obstruction, in both of which delay in the gastric evacuation and a persistent retention in the duodenum had been shown; one of these had already

been brought to the notice of this Section by Mr. Higgins, and the other was a case of atresia of the third part of duodenum discovered post mortem.

Vomiting during this examination was quite a rare occurrence. A number of the true pylorics had brought up mouthfuls, but in no case had the entire meal been vomited. Knowing this, one of his colleagues had in many cases prescribed an opaque meal for persistent vomiting, perhaps he would tell them with what result? He (Dr. Shires) understood that such result had been successful. In conclusion, Dr. Shires said that, in his opinion, true cases of pyloric stenosis exhibited such definite symptoms that an X-ray examination was rarely necessary. In the doubtful cases he thought it was of considerable value in helping the clinician to determine the best method of treatment, and by repeated examinations after lavage and regulated diet the clinician was in a position to decide whether surgical measures were necessary, always provided that the child's resistance was not lowered so as to prohibit operative procedure.

Mr. MAX PAGE said he thought the results obtained by radiological examination of these stomachs were very valuable to the clinician. From the surgical point of view, it was very important to know where the obstruction was, so that the operation could be carried out promptly. He asked whether Dr. Shires, in his large series of cases, had experienced any mishaps in giving bismuth meals. In the small series which Dr. Fildes quoted he knew there had been no trouble, but it was possible some might occur with a larger number.

Dr. CHODAK GREGORY quoted a case of her own in which the issue had been rather disappointing. An infant was admitted with symptoms of pyloric stenosis, but there was no visible peristalsis, and no tumour was palpable. A skiagram showed that as long as four hours after the meal scarcely any of the food had left the stomach. Operation was not done, as the child was not in a fit condition to undergo it, and death occurred three days later. No stenosis was found. She did not know how much of the obstruction was due to spasm, and how much to the atonic condition of the muscle. Had the child been in a better condition, it would probably have been operated upon, on the evidence of the X-ray findings.

Dr. C. G. TEALL (Birmingham) said that when, at Birmingham, a commencement was made in the examination of children by means of a bismuth meal for this condition, he could find very little information and he had started in the dark. The cases he examined came, almost entirely, from Dr. Parsons. One of his first cases had shown absolute stasis two hours after a meal was given and pyloric stenosis had been his diagnosis. No operation was performed, but the child died. The pyloric canal was found to be rather larger than usual. Since then he had realized that spasm of the pylorus did occur apart from stenosis. Possibly it was an exaggeration to say that if no emptying of the stomach was seen in the first hour the presence of spasm rather than that of stenosis should be suspected, but he thought that in hypertrophic stenosis the stomach tended to empty itself to a small extent during the whole period of the examination. He had examined children forty-eight hours after the meal and found barium in the stomach even in a case in which the stomach commenced to become evacuated in twenty minutes, though the emptying process was very slow. There was a general tendency to accept the statement that normally the stomach became emptied in two hours, and to allow that supposition to govern the conclusion as to whether stasis were present or not. Examination should be made three hours after the meal and even next morning.

With regard to the radiological diagnosis, he had hoped Dr. Parsons would have been able to attend, as that subject had been dealt with in his Goulstonian Lectures. The view held at the Birmingham Children's Hospital was that, except in rare cases, the X-rays were not essential in the diagnosis of this condition or in that of pylorospasm. Dr. Parsons had told him (the speaker) that usually he could distinguish between the two conditions clinically, but that while this was so the X-ray examination was of considerable help in the diagnosis of pylorospasm. The X-rays should be regarded as a valuable auxiliary, as these children were very ill and opera-

tion had to be decided upon quickly. Spoon-feeding, they considered, caused less trouble and disturbance than any other method, and in no case had they had any untoward results after the X-ray examination. In one case X-ray examination had not been carried out before operation, and that case turned out not to be one of stenosis. If the X-ray examination had been done first the necessity for operation would probably have been obviated.

Dr. C. G. WHORLOW said he did not think there was much doubt that X-ray examination was of the utmost value in certain of these cases. But one thing needed to be borne in mind with reference to the point raised as to untoward results. He did not think there was any risk when the examination was carried out by a competent radiologist, but it sometimes happened that cases were taken to unqualified radiographers. In his opinion an infant's skin was probably more radio-sensitive than that of an adult, and he knew of a case in point where a child was taken to a radiographer for the investigation of constipation by means of a barium meal examination, and a very severe burn had resulted. These examinations were apt to become prolonged, whereas it was of the utmost importance that they should be carried out quickly, especially as serial radiograms were usually necessary.

Dr. EDMUND CAUTLEY remarked that this question was one of considerable importance. These examinations were easily carried out in hospital practice, but were not so available in private. He thought there was much more risk of error if the diagnosis were based on the X-ray examination than if it were based on a palpable pylorus. When the condition was definite, he thought the pylorus could be felt in almost every instance, though not necessarily at the first attempt. Even if it were situated higher up than usual in a baby, if the patient were quiet and the muscles relaxed, one could get the fingers under the liver, and if there were sufficient hypertrophy to warrant operation, it could be felt.

He thought there was considerable risk in what had already been referred to, namely, mistaking a delay in the passage of a bismuth meal in the stomach due to the stomach being atonic and dilated, with or without pyloric spasm, for organic stenosis. If diagnosis were based on X-rays alone, cases in which there was no hypertrophy would be operated upon. If a bismuth meal were followed through, in a case of pyloric hypertrophy, some of the food could be seen to be passing through, whereas in spasm there might be complete obstruction. In hypertrophy the vomiting might be so acute that some blood-stained fluid would be brought up, due to swelling of the mucous membrane from the spasm, grafted on to the hypertrophy, or it might be due to secondary gastric catarrh. What had been of most value in these X-ray examinations seemed to be the distinguishing of duodenal stenosis from pyloric stenosis. Fortunately the former was very much the rarer, but in a well-marked case of it he did not doubt there would be a most definite X-ray picture; one would see the hypertrophied stomach and the first part of the duodenum bulging and containing much of the bismuth meal. The papers which had been read were of extreme interest and of great value, but he thought it would be found that clinicians would more readily depend upon the symptoms of a case, its history, and the presence of a dilated stomach, visible peristalsis, and a palpable pylorus; he himself would be loth to allow a case to be operated upon unless he could feel the pylorus.

Dr. FILDES (in reply) said he regarded the radiologist's function in life as that of trying to assist the clinician, not dictating to him. A certain number of clinicians seemed to expect from the radiologist a hard-and-fast diagnosis, but that was not possible, because one was dealing with shadows, and had to read into the skiagram, as nearly as possible, the true condition. The picture was supplied as an adjunct to the means employed in whatever other department to which the child was sent, the object being to do everything possible for these children before any drastic measures were taken. If a bismuth meal could be given without any danger to the child, it seemed to be a good thing to be carried out, on account of the information it afforded.

With regard to feeding, it was the experience of himself and of his colleague that it was easier and more rapid to use a tube passed straight into the stomach; it was the

least worry to the child, and the time occupied was minimal. The amount of irradiation such a child received was so small that it was negligible; screening occupied five or six seconds, and the exposure for the skiagram was about one-tenth of a second. And as the patient was so small, a low milliamperage sufficed.

He wished it to be understood that most of the work of the paper had been done by Dr. Forest-Smith.

Dr. BERTRAM SHIRES (in reply) said he knew of no untoward incident having occurred to any of the children he had examined. He had had to deal with very sick children, and he had been told by the house physician that the child was worse afterwards, but nothing more. He arranged that if there was any residue of barium in the stomach, it should be washed out immediately, so that the child could be put on to its ordinary feeds again. If the barium residue were left in the stomach and the child was then fed normally, there might be a residue forty-eight hours, even seventy-two hours, afterwards, because it was kept sedimented in the stomach, and it did not pass out. That applied to the adult also. Great care was taken over the preparation of the bismuth; it was sterilized, and all the bismuth was passed out of the child in twenty-four hours, therefore he did not think much harm could happen in that way.

He agreed with Dr. Fildes that the time-interval necessary for exposure of the child to the rays must be quite negligible; the time occupied was not more than that required to examine a child's chest under the screen.

Sections of Study of Disease in Children, Neurology, Obstetrics and Gynæcology, and Orthopædics.

Dr. CUTHBERT LOCKYER (President of the Section of Obstetrics) in the Chair.

DISCUSSION ON "BIRTH INJURIES, WITH SPECIAL REFERENCE TO INTRACRANIAL INJURIES WITH HÆMORRHAGE, AND TO NERVE INJURIES."

Dr. CUTHBERT LOCKYER (Chairman)

said this was the second joint discussion in which the Section of Obstetrics and Gynæcology had been invited to participate, the first occasion having been one on which the Section were the guests of the Psychiatry Section. As hosts to-night, they extended a cordial welcome to the members of other Sections who had been kind enough to come, and probably join in the debate.

The meeting would hear from Mr. Eardley Holland a lucid account of the pathology of intracranial trauma, hæmorrhage, &c., and to others those present would look for the later results in those patients who had survived.

He thought it was fitting that on the present occasion one should recall two interesting historical facts. The first was that in 1861 a very valuable paper was read before the old Obstetrical Society by Dr. W. J. Little,[1] who was senior physician to London Hospital, and founded the Royal Orthopædic Hospital. The paper was "On the Influence of Abnormal Parturition, Difficult Labours, Premature Birth, Asphyxia Neonatorum, on the Mental and Physical Condition of the Child, especially in relation to Deformities." Jumping thirty years, there was another interesting communication to record, on "Visceral Hæmorrhages in Stillborn Children, an analysis of 130 Autopsies; being a Contribution to the Study of the Causation of Stillbirths." That was by Dr. Herbert Spencer, a past-president of this Section. Dr. Spencer had written expressing his deep regret that he was unable to be present, owing to duties in connexion with another Society, of which he was president, the Medical Society of London. The following extract from Dr. Spencer's work on the subject[2] showed an intelligent anticipation of the work more recently done by Mr. Eardley Holland:

> "The author gives a detailed account of a consecutive series of 130 autopsies on fresh, mostly stillborn, fœtuses, in so far as congestion of and hæmorrhage into, the viscera are concerned. Appended are tables of the more important organs affected.

[1] *Trans. Obst. Soc. Lond.* (1861), 1862, iii, pp. 293-344.
[2] *Trans. Obst. Soc. Lond.* (1891), 1892, xxxiii, p. 203.

The main part of the paper consists of a description of the naked-eye and microscopic appearances of the various viscera as regards congestion and hæmorrhage.

The causation of the hæmorrhage is discussed, and the following practical conclusions are drawn:—

(1) In stillborn children, or those dying shortly after birth, congestion or œdema and hæmorrhages are usually found in various important viscera.

(2) These hæmorrhages occur in cases delivered naturally or by version or by forceps, through normal and abnormal pelves; in primiparæ and multiparæ; with large and small children; in "easy" and difficult, rapid and prolonged labours.

(3) The hæmorrhages are, however, most frequent and most severe in children subjected to much pressure by the parturient canal or instrument, or the hand of the attendant, especially when delivered by the lower extremity.

(4) Cerebral hæmorrhage is more frequently found in stillborn children delivered by the forceps than in those born by the breech, and in these latter more frequently than in those born naturally by the head.

(5) Hæmorrhage into most of the other viscera is more frequently met with in pelvic than in cephalic presentations.

(6) These hæmorrhages and the accompanying injuries are in many cases the cause of the stillbirth, and, when not immediately fatal, may be followed by the gravest consequences.

(7) They are most likely to be avoided by preventing premature rupture of the membranes, by artificial dilatation of the parturient canal (when necessary), by restricting the employment of version and other artificial manipulations to urgent cases, and by preferring cephalic to podalic version in cases suitable for the former.

(8) The use of the forceps should be absolutely limited to cases in which there exists some pressing danger to mother or child, and it should never be employed merely to shorten the time of labour.

(9) In breech presentations, examination of the genital organs of the child should be carefully avoided during delivery. As soon as the child's limbs are born they should be wrapped in a thick layer of antiseptic wool (which keeps the child warm, and prevents the hand from slipping, and protects the limb from pressure). If traction be necessary, it should be made over wool wrapped around the child's limbs or pelvis; it should never be made by the hand around the child's waist.

(10) In delivering the after-coming head, care should be taken that the sterno-mastoid muscles are not unduly stretched or pressed upon. When the after-coming head is in the pelvis and there is even slight difficulty, resort should be had to the forceps to deliver."

Mr. Eardley Holland.

The Etiology and Morbid Anatomy of Intracranial Birth-injury and Hæmorrhage.

During the last few years a remarkable interest has been taken in fœtal and neonatal pathology and an immense amount of material has been investigated under grants from the Ministry of Health and the Medical Research Council. The most impressive fact that has emerged is the truly astonishing frequency of intracranial injury and hæmorrhage as a cause of stillbirth and of death of the infant during the early days of life. Here is a disturbing state of affairs for obstetricians, for these injuries and hæmorrhages are mechanical in origin and are therefore largely preventable; and furthermore, the obstetricians feel a responsibility towards their pædiatric and neurological brethren, because they know that these conditions are not always immediately or even remotely fatal, and they suspect that those children who survive may bear the stigma of birth trauma in the form of various nervous and physical maladies.

The ætiology and morbid anatomy of these injuries are now well enough known, and as I propose to deal only with this aspect of the subject, the scope of my remarks lies entirely in the realm of fact; but when their remote effects are considered we shall feel ourselves on more uncertain and debatable ground, and it is essentially this aspect of the subject that asks for discussion.

The Frequency of Cerebral Hæmorrhage.

The following are some figures relating to the frequency with which cerebral hæmorrhage has been found in stillbirths (macerated fœtuses excluded) and neonatal deaths: Herbert Spencer (1891), 130 cases of stillbirth and neonatal death, 41 per cent.; Holland (1919), 167 stillbirths, 55 per cent.; Capon (1920), 52 stillbirths, 46 per cent., 28 neonatal deaths, 21 per cent.; Browne (1921), 92 stillbirths, 24 per cent., 80 neonatal deaths, 44 per cent.; Cruickshank (1922), 200 premature stillbirths and neonatal deaths, 28 per cent.; Kennedy (1922), 200 mature stillbirths and neonatal deaths, 32 per cent. Recent American and continental workers have published much the same results.

The Sites and Extent of Cerebral Hæmorrhage.

Hæmorrhages may be (1) subdural, (2) ventricular, or (3) in the cerebral substance, and may be classed as severe, moderate, or slight. They often occupy more than one site, and are more often diffuse than localized. A large subdural hæmorrhage spreads into all the cranial fossæ as well as over the surface of the brain. Hæmorrhage from the vein of Galen or its branches drains downwards around the cerebellum, pons or medulla; hæmorrhage when beneath the tentorium cerebelli is likely to have more serious effects than when above it. Subdural hæmorrhage is commoner than ventricular. Hæmorrhage into the cerebral substance has not yet been properly studied. An interesting case of inspiration apnœa in an infant, in which minute hæmorrhages had occurred in the pons, was recently reported by Myers and Kirkwood; the brain stem of this infant was examined by T. Lumsden, and the pathological appearances were similar to the results he obtained in experiments on the cat in connexion with the respiratory centres. In ninety-one cases of cerebral hæmorrhage, F. J. Browne records the following sites:—

Into lateral ventricles	37
Into fourth ventricle	4
Into third ventricle	1
Upper surface of both cerebral hemispheres	4
" " right cerebral hemisphere	5
" " left cerebral hemisphere	6
" " right occipital lobe	1
Base of brain—diffuse	9
Under one or both temporo-sphenoidal lobes	8
Under one or both lobes of the cerebellum	7
Under left frontal lobe	1
Mesial aspect of brain	2
Overlying corpus callosum	5
In relation to cerebral peduncles, pons and medulla	8
Left Sylvian fissure	1
Middle fossa of skull	7
Posterior fossa of skull	7
Over one or both sides of tentorium cerebelli	14
Beneath tentorium cerebelli	4
Into tentorium cerebelli	12
Into falx cerebri	2

The Ætiology of Cerebral Hæmorrhage.

All hæmorrhages are not due to birth-injury. Ætiologically they fall into two groups: (1) Traumatic, or mechanical, and (2) asphyxial, of which I believe the former is much the more important. Hæmorrhage of traumatic origin is usually found accompanied by tearing of the tentorium cerebelli or falx cerebri; whereas, hæmorrhage of asphyxial origin is found in association with the other signs of fœtal asphyxia. This distinction does not always hold good—for many cases with tentorial tears are found accompanied by well-marked signs of fœtal asphyxia (i.e., visceral hæmorrhages). In such cases it is probable that an initial traumatic hæmorrhage causes the circulatory disturbances that lead to severe asphyxia. I think it is also likely that asphyxia is an important contributory cause of traumatic hæmorrhage, because blood-vessels, the walls of which are in a state of high tension from engorgement, are more likely to be ruptured by a superadded mechanical stress than vessels whose walls are in a state of normal tension.

Traumatic Hæmorrhage.

To appreciate the mode of origin of this variety of hæmorrhage it is necessary to know how the fœtal head behaves when acted upon by the forces of labour. The fœtal head is a fragile object the structure of which displays a remarkable combination of qualities which enable it to resist injuries with defects which dispose it to suffer them. To anyone who begins to study the fœtal head from the obstetrical point of view, the wonder must surely be, not at the relatively small number of fœtuses that suffer cranial birth-injuries, but at the large number that reach the world alive and uninjured. During its passage through the birth canal the head is subjected to the action of compressive forces, and is therefore in a state of stress. Cranial stress may be regarded as consisting of two elements, a general compression of the whole head, and a simple longitudinal compression by opposite forces acting at the ends of the long diameter of engagement of the head in the pelvis. The outstanding feature of the fœtal head is its ability to undergo changes in shape, or moulding, when subjected to stress. Alteration in shape is brought about partly by a displacement of the plastic bony vault as a whole and partly by bending of the individual bones which compose it. The displacement of the vault as a whole takes place chiefly at the junction of the plate with the base of the occipital bone, a hinge joint which allows a considerable range of movement forwards and a smaller range backwards. The comparatively rigid plate of the occipital bone moves forwards or backwards carrying with it the rest of the vault, whilst the greatest bending of individual bones occurs in the parietal and frontal bones. Moulding of the head is a typical example of a slide or shearing strain, the head altering in shape but not in size (or only to an insignificant extent). By far the commonest alteration in shape is elongation of the head in a vertical direction with corresponding shortening in the antero-posterior direction, as occurs in vertex and breech presentations (which comprise about 99 per cent. of all presentations) and is the result of pressure applied at the opposite ends of the diameters lying between the occiput and the forehead. A certain amount of cranial stress occurs, of course, in every labour, and in every labour there is a certain amount of head-moulding, but where there is excessive cranial stress a dangerous degree of alteration in shape occurs. For moulding of the head is but the outward, visible sign of something inward and more deeply significant, and the intracranial events which accom-

pany head-moulding are of the utmost importance. The dura mater septa are attached to the cranial bones and form a closed system of stays or ligaments; when the head alters in shape, alterations in the tension of the septa are inevitable, owing to the nature of their attachments to the bones. Excursion and bending of the bones are resisted by the tension of the septa, just as the swaying of a mast is resisted by the tension of its stays. If alteration in the shape of the head becomes too great, as under the influence of excessive cranial stress, the septa tear; and tearing of the septa permits still more extreme alteration in the shape of the head, and dangerous disturbances of the intracranial contents. According to this conception the septa exert a protective function and form a defensive system against excessive alterations in the shape of the fœtal head. Support to the theory that the septa are designed to take stress is found in the fact that they contain special strengthening bands and fibres, arranged on admirable mechanical principles, along the lines where stress is likely to fall during the moulding of the head. The chief system of strengthening fibres in the falx cerebri and tentorium cerebelli consists of two opposing sets of convergent fibres which meet in their points of convergence at the anterior part of the junction of the falx cerebri and the tentorium cerebelli. This area of the septal system is subjected to strain when the head is elongated vertically, as in vertex and breech presentations. Tearing of the tentorium and falx is clear evidence of excessive cranial stress, and forms the best and simplest means of estimating, post mortem, the degree of stress to which the head has been subjected during labour. The tears lie in the area of greatest stress and usually occupy the free border of the tentorium near its junction with the falx; all degrees of tear may be found, from a small unilateral rupture of the superficial layer to extensive bilateral tears through its whole thickness. Very commonly a hæmatoma is found in that region of the tentorium which usually tears; this, I believe, signifies stretching just short of the tearing-point. Tearing of the falx cerebri is less common and is usually found in conjunction with tentorial tears. Tearing of the septa removes the chief restraining influence and the head is then free to undergo still more extreme alteration in shape.

The frequency with which tears of the dura mater septa are found in cases of stillbirth and neonatal death, and especially of course in cases of cerebral hæmorrhage, is very great. I found tears in 48 per cent. of stillbirths; Capon in 60 per cent. of stillbirths and in 25 per cent. of neonatal births; Browne in 37 per cent. of stillbirths, and Cruickshank in 24 per cent. of stillbirths and neonatal deaths; Schaefer found tears in 91 per cent. of stillbirths after difficult forceps delivery.

The mode of origin and source of cerebral hæmorrhage in cases of excessive cranial stress must now be considered. The actual tear is not in itself a mortal or even a dangerous injury; the tentorium is not a vital structure and being a comparatively non-vascular membrane is not itself a source of hæmorrhage, except in those unusual cases in which the lateral or straight sinus is torn through. Cerebral hæmorrhage is not always present in cases of torn tentorium cerebelli, though nearly always; in my own cases 92 per cent. were associated with cerebral hæmorrhage of greater or less degree; Capon found it in 80 per cent. of his cases, but Browne records hæmorrhage in only 40 per cent. My own view is that the hæmorrhage in most cases comes from the vein of Galen or its tributaries (fine veins from the cerebellum, mid-brain and pons). It has already been pointed out that in the common form of cranial stress the apex of the tentorium is drawn upwards, and this upward displacement is very con-

siderable in places of excessive head-moulding, especially after the tentorium has ruptured. Any movement of the apex of the tentorium is necessarily transmitted to the vein of Galen, the fixed point of which is at its entrance to the straight sinus. The vein of Galen follows this movement with two results: its direction is altered so that it is kinked at its entrance into the sinus, and it becomes stretched. The obstruction at the kink results in engorgement of the vein and its tributaries which, under such circumstances, are easily ruptured by being stretched between their fixed points. In only two cases have I found the vein of Galen itself ruptured. I have been helped to this conclusion by studying a number of fœtal heads, which I soaked in formalin before dissecting. The vein of Galen and its tributaries are not the only sources of traumatic hæmorrhage. The cerebral veins may be torn near their terminations in the superior longitudinal sinus, and Cushing has pointed out that these veins are inadequately supported as they traverse the subdural space and become distorted during over-riding of the parietal bones. Kinking of the vein of Galen with engorgement of its tributaries probably plays its part in aiding rupture of the delicate veins of the choroid plexus, thus explaining hæmorrhage into the lateral ventricle.

Circumstances which Determine Excessive Cranial Stress.

The circumstances under which excessive stress and extreme alteration in the shape of the head may occur are fairly obvious. The most important one is disproportion between the head and the maternal pelvis, as in pelvic contraction and excessive size of the fœtus; into much the same category come cases of deficient flexion of the head and great rigidity of the maternal pelvic floor. The forceps is likely to cause these injuries when it is applied too early, when the force used is too great, when the fœtus is delivered too rapidly, when it is misapplied to the antero-posterior diameter of the head, and when it is used to deliver a premature fœtus. When properly used it is doubtful whether the forceps is dangerous in this respect. A very important point is whether the stress reaches its maximum suddenly or gradually; the septa are much better able to resist a slow than a too-rapid head-moulding. In a breech-labour the after-coming head is rapidly, though comparatively momentarily, compressed in a series of diameters lying between the forehead and the occiput. That is why tearing of the septa is so liable to occur as the result of breech-labour. I found tearing of the tentorium in 75 per cent. of dead fœtuses delivered by the breech, and Browne states that it is sixteen times more likely to occur in breech than in vertex presentations. For the same reason, very rapid birth of the fœtus, either spontaneous (precipitate labour) or after the use of pituitary extract, is likely to produce too rapid moulding and intracranial injury. The soft and plastic head of a premature fœtus is much more liable to injury than the firm head of a mature fœtus. It is a noteworthy fact that tearing of the tentorium and cerebral hæmorrhage may occur after an apparently normal labour; such cases, in the absence of prematurity, are hard to explain. I believe that some cases are caused by too zealous guarding of the perineum, especially by strong forward pressure against the perineum, forcing the occiput against the symphysis.

Fœtal Asphyxia and Intracranial Hæmorrhage of Asphyxial Origin.

When the circulation in the umbilical cord is obstructed, or the placenta becomes widely separated or extensively degenerated, the fœtus passes into a state of asphyxia. Stimulation of the respiratory centre by the accumulation

of carbon dioxide in the blood leads to respiratory movements and the resultant alteration in the intrathoracic pressure causes venous obstruction and a great rise of venous pressure throughout the body. If this state lasts long enough the fœtus dies. The post-mortem signs of asphyxia are very common in still-born fœtuses and are found in 30 to 40 per cent. of fœtuses who have died during labour. Multiple petechial hæmorrhages are found on the surface of certain organs, especially the heart, lungs, liver and thymus; subcapsular hæmorrhages of considerable size are often found on the surface of the liver. On section the organs appear congested and sometimes there are interstitial hæmorrhages, as in the suprarenal capsule. Very often in association with these visceral hæmorrhages are found various forms of intracranial hæmorrhage, and the cerebral veins are always remarkably engorged. Usually the hæmorrhage takes the form of a fine film of subdural hæmorrhage over the surface of the cerebral hemispheres; sometimes there is a small amount of hæmorrhage at the base of the brain. An extremely common form is between the layers of the falx cerebri, where a hæmatoma is formed. Still another form consists of small multiple hæmatomata in the pia-arachnoid tissue over the surface of the brain. Occasionally there is a considerable subdural hæmorrhage spread diffusely in all the cranial fossæ and reaching up over the surface of the cerebral hemispheres.

Observers differ about the relative frequency of traumatic and asphyxial hæmorrhage. In my own ninety-two cases of cerebral hæmorrhage I placed seventy-five in the traumatic and seventeen in the asphyxial class; Capon found that hæmorrhage without tearing of the septa was rare, but Browne states that only half of his cases of cerebral hæmorrhage were accompanied by tentorial tears. The post-mortem differentiation between traumatic and asphyxial hæmorrhage is not always possible, and in many cases both factors have co-existed. An asphyxiated fœtus may subsequently suffer from excessive cranial stress, as in a case of rapid delivery by forceps or version for prolapsed cord, and it has already been noted that an initial traumatic cerebral hæmorrhage may cause the respiratory and circulatory disturbances that lead to severe asphyxia.

Dr. H. C. Cameron.

I propose to discuss the question from the clinical standpoint under three headings:—

(1) The symptomatology of subdural hæmatoma in the newly born.

(2) The survival of certain infants who have suffered in this way and the long latent period which ensues before the evidence of damage to the cortex is appreciable.

(3) The ultimate effect upon the physique and intelligence of the child.

(1) Not all the infants have been born after a difficult labour or after a precipitate labour. In a considerable number the symptoms have supervened upon a labour in no way abnormal. In almost all the bulging fontanelle has given evidence of increased intracranial tension. Examination of the cerebro-spinal fluid in a high proportion of cases has shown it to be under high tension and blood-stained. If the examination is made after some interval the blood may be altered, as shown by crenation of the corpuscles or the brown staining of methæmoglobin. The ophthalmoscope in almost half the cases is said to show punctate hæmorrhages, more rarely œdema of the disc. An increased rigidity and spasticity of limbs, often with opisthotonos and trismus, is common, and there are usually either generalized convulsions or localized twitchings,

especially of the face and eye muscles. Light percussion upon the sternum, for example, will often produce a characteristic jerking together of all four limbs. Somnolence or actual coma may be a striking symptom, or there may be persistent crying and restlessness with all the appearances of pain. The pulse is often slowed. Some interference with the respiratory rhythm is the rule. There may be asphyxia, pallida or livida, at the moment of birth and even after respiration is established it may remain shallow, halting, slow or rapid, and apt to cease altogether. The suction reflex is often absent. There may be striking asymmetry of the cranium as a result of extensive tentorial tear. I show a cast of the skull of an infant with this asymmetry; the child suffers from diplegia. The labour was asphyxial.

(2) It is clear that the life of infants who show such symptoms in severe degree is very precarious. I have in my own small experience seen eight cases in which I thought that a subdural hæmatoma was present, and all were fatal. That in cases of less severity the condition is compatible with life seems to me certain, though I am aware that the view that all die has been put forward. So long as our powers of diagnosis only enable us to recognize the severest grade, so long the doubt must exist. The routine use of lumbar puncture and of the ophthalmoscope in the newborn and a careful clinical examination will probably bring to light many less severe cases.

If recovery takes place, a further difficulty is presented by the long latent period which then ensues. The cortex of the normal infant only gradually awakens to activity. Only those parts of the brain which are active from the first are medullated, as for instance the nervous paths for respiration, suction, cardiac action, crying, &c. The pyramidal tracts only gradually become medullated during the first year, and delay in the transition from the spastic uncontrolled movements characteristic of the newborn to that degree of voluntary control of lips, tongue, hands and feet which the normal infant achieves may not be obvious to the parents until the failure to walk or grasp at a year or more proclaims it.

(3) The infant whose cortex has been damaged lags far behind the normal in development upon the sensori-motor side. The spastic limbs can neither be moved at will, nor can they sense and feel objects. Since a great part of infantile education proceeds along sensori-motor paths, the initial delay in development is always in these cases much greater than the ultimate loss. Very many learn very quickly by eye and by ear and from the first the mother may be conscious of the retentive memory. When at length the stiff limbs pass in some degree under control in later childhood improvement may be rapid. In general, the children have the power of concentration and persistence and achieve remarkable precision considering the incoördination in some few much practised movements. The general character may be on a high plane. They may be affectionate and truthful, as they may possess considerable powers of imagination and artistic sense. Children of this type are not likely to be inmates of homes for mentally defective children. Few of my own patients have consulted a neurologist; nearly all have been treated orthopædically. It is by the name of the founder of the Orthopædic Hospital that we know them. In their homes there is often no doubt about their cleverness, though they may be shy and awkward and self-conscious with strangers. Their childhood is so highly abnormal that they are passionate and ill controlled or nervous and unstable. Little comments more than once on the strange resemblance in character and temperament that exists between many of these individuals. In all of these ways they form a group apart, which, at least in its most characteristic representatives, seems to me clearly separated from cases of spastic

idiocy due to a developmental deficiency of cortical neurones. That Little was right in contending that in this group the cause was to be found in cortical hæmorrhage at birth, I believe strongly, although this condition may not be the only or invariable result of such an accident.

Dr. James Collier.

I would like first to make a comment on the very interesting words which we have heard from Mr. Eardley Holland and Dr. Cameron. The sites of meningeal hæmorrhage which were listed on this table make one wonder in how many of those positions one would expect hæmorrhage to produce symptoms. That is one of the most important things which Mr. Eardley Holland could possibly have brought before this meeting, for I can conceive of recovery, in almost every situation, from that hæmorrhage unless it were in the substance of the brain itself. Dr. Cameron remarks that he does not think all these cases are fatal, he thinks many of them recover. Of that, I am perfectly sure. Mr. Holland refers to the veins on the vertex of the brain and the effect of their injury in the thrombosis. Much too little has been said about thrombosis in connexion with birth injuries, and there are some cases in which, from injury to the superior longitudinal sinus and adjoining brain, bilateral softening of the brain occurs. But I would not have you think that that is the common cause for the diplegic spastic states which are found in infants.

With regard to rupture of the vein of Galen, I have no clinical accompaniment or end-result of such hæmorrhage.

Concerning hæmorrhages occurring at birth, Mr. Eardley Holland says there is much that is unexplained; surely there is.

When Dr. Cameron expects evidence of increased intracranial pressure from the ophthalmoscope in the little child, I would remind him that in the soft expanding skull I do not think he will find any exhibitions of intracranial pressure, so far as the fundus is concerned; I do not think I have ever seen optic neuritis until the head is closed.

When Dr. Cameron is describing his most interesting clinical details of diplegia, as resulting from birth injury, he is treading on rather thin ice.

From the nature of my calling as a neurologist my knowledge of the highly interesting and very difficult subject which we are discussing, is that of the end-results, or rather of what have been held to be the end-results, of injuries to the nervous system occurring during the process of birth. I have practically no experience of cases soon after the injury is deemed to have occurred either clinically or pathologically, and I am here to be instructed upon these matters. But for the past twenty-five years I have delved freely into the very large literature of the subject and have several times indulged in lengthy publications, the last of which was my address as President of the Section of Neurology in October of last year and which is appearing in the coming number of *Brain*. There perhaps has never been a more controversial subject in the history of medicine than the pathogeny of infantile spastic states and the explanation of their well-known connexion with difficult birth. This controversy began with Cruveilhier, in the early days of the nineteenth century, when he made the following observation: "If one practises autopsy upon those of the newly-born who arrive into the world in a state of asphyxia, one will find meningeal hæmorrhage in one-third of all such cases and this lesion is without doubt the cause of the asphyxia. It is the traumatism to the brain during delivery which provokes the hæmorrhage." He does not pause to offer any apology

for the other two-thirds of the cases which though born asphyxiated, yet showed no meningeal hæmorrhage. Meningeal hæmorrhage became a common experience in the newly-born after difficult labour, after instrumental delivery, with premature birth, with precipitate birth, with twins, and in syphilitic infants, and it was attributed to injury—to Kundrat's overlapping of the parietal bones and tearing of the superior longitudinal sinus, or if not to direct injury at least to the sudden decompression of the head when delivered, as Marfan put it, when he wanted an explanation for its occurrence in the precipitate birth of a premature child. But Demenil also found copious meningeal hæmorrhage in a child which he himself delivered with the utmost care by Cæsarean section when neither injury nor asphyxia could have played any part. Weyhe's one thousand consecutive autopsies on children dead before the fourth month, from whatever cause, proved the presence of meningeal hæmorrhage in 12 per cent. of all the cases and similar results were obtained by Mraçek in another large series of cases, Meningeal hæmorrhage, then, does really seem to occur not infrequently in the newly-born. The question is—does it always cause symptoms? And, if so, what are those symptoms? There was a time not so many years ago when all infantile spastic states dating from birth, both hemiplegic and diplegic, were attributed by high authorities to meningeal hæmorrhage, but since the post-mortem findings obstinately refused to correspond to this hard-dying hypothesis, and persisted in showing developmental arrests dating long before birth or deep gross lesions in the brain, or no gross lesion at all, and since no one was ever able to explain how meningeal hæmorrhage could cause lobar atrophic sclerosis of the brain, or why the trouble affected the cerebral hemispheres only and in a systemic fashion, and never the base of the brain, where every one would expect clot to accumulate and give trouble, the origin of infantile spastic states in meningeal hæmorrhage is discredited beyond resuscitation. In later life the symptoms of meningeal hæmorrhage are well known. When the effusion is small there may be no symptoms beyond slight headache. When the effusion is larger the symptoms are those of acute meningitis with head-retraction, stiffness of neck, vomiting and convulsion, and the diagnosis is made by lumbar puncture. If the hæmorrhage is arrested, so far as my experience goes, the patient recovers perfectly and has no lasting symptoms. If it is not arrested the patient dies of intracranial supertension. In the newly-born I feel sure that meningeal hæmorrhage could be directly diagnosed, when of sufficient magnitude, by the occurrence of symptoms like those of meningitis, such as head retraction and tonic spasm, and it could immediately be proved by lumbar puncture which is such an easy proceeding in the little child and can be performed with a large hypodermic needle. I incline strongly to the belief that severe convulsions occurring shortly after birth have their cause in meningeal hæmorrhage and I think that in these cases and in those showing symptoms like meningitis, lumbar puncture is likely to be a means of relieving the symptoms and even of curing the patient. An examination of the cerebro-spinal fluid in a series of newly-born children will be our only means of settling the incidence of meningeal hæmorrhage and such an investigation is badly needed.

It must not be forgotten that the great Little who contributed so much to our knowledge upon this subject held that meningeal hæmorrhage and all the spastic states which go under the name of "Little's Disease" were the result of asphyxia neonatorum which produced tiny hæmorrhages throughout the nervous system and that these led ultimately to sclerosis and

atrophy. Even in cases in which there was no asphyxia to be seen clinically Little argued for asphyxia almost as stoutly as Dr. McNutt and Sir William Gowers did for meningeal hæmorrhage as a cause for infantile spastic states. I must confess myself to a leaning towards Little's view in that I think that some few infantile spastic states may really result from asphyxia neonatorum; for it seems to me that the grey matter of the brain is less tolerant of asphyxia than is any other part of the nervous system, and I have seen two cases of carbon monoxide poisoning in which, though the patients recovered vegetatively, yet no function of the higher brain ever returned.

There is, however, no getting away from the fact, which every investigator has corroborated, that 60 per cent. of all the cases of infantile spastic states dating from birth have a history of some abnormality in the process of birth, either prematurity, precipitate birth, dystocia, secondary uterine inertia, instrumental birth or asphyxia neonatorum; and in denying birth injuries as causal factors in these conditions it is essential to put forward some sort of explanation as to why abnormalities of birth and infantile spastic paralysis are so commonly associated. Considering how frequently the birth abnormalities occur in those cases in which the brain has been damaged quite early in fœtal life, it is now generally accepted that for these cases at least and probably for all the cases, both the cerebral defect and the abnormal birth are expressions of a deeply lying pathological influence which has dominated the development of the offspring and the personality of the mother and which has disturbed those mysterious relations between mother and child which determine a speedy delivery at term.

Laceration of the brain, with its immediate associate of hæmorrhage and its remote result of scarring and cystation, undoubtedly occurs as a result of difficult labour and it accounts for some of the cases of infantile hemiplegia which really date from birth. But I do not think that the incidence of this cause is frequent in cases of hemiplegia from birth, for many of these are proved to be of prenatal date and may be the result of fœtal encephalitis and fœtal vascular lesions, while others occur within a few days of birth and may be due to rupture of vessels during convulsion, to encephalitis or even to hæmorrhage into a bruised region of the brain injured during birth. Cases of paralysis from injury at birth seem to be very rare clinically, and specimens of the brain from such conditions seem to be still rarer. Practically all that I have come across have exhibited suggested or proved lesions of the cerebral hemispheres. For this reason I may legitimately suggest that most cases of severe injury to the brain at birth do not survive or we should see more of the end-results, and further I suggest that only those survive in which the lesion is confined to the cerebral hemispheres, for I have never seen evidence of injury to brain stem or cerebellum as an end-result.

From the marvellous powers of compensation for local injuries which is known to exist in the brain of a little child one would expect small local lesions of the cerebral cortex to produce little in the way of external evidence at the moment, and still less or nothing at all eventually. I should expect cerebral hæmorrhage, occurring from tissue disruption during the process of birth, to agree in its manner with the cerebral hæmorrhage which occurs in adult life. It should have the habit of an avalanche and, commencing in a small way, the larger it becomes the more tissue it tears up and therefore the more it bleeds; and like the avalanche it should have no particular tendency to come to a stop, but should soon rupture either into the ventricle or upon the surface; and for this reason it should be recognizable, as are

almost all adult brain hæmorrhages, by lumbar puncture within very few hours or even minutes after its occurrence. During its development it should cause urgent symptoms of intracranial pressure elevation, though these may be mitigated by the expansile nature of the infant's head, and it should in nearly every case prove fatal within a few days.

Here I may be allowed to refer to a very notorious case of cerebral hæmorrhage published by Dr. Sarah McNutt in America, in 1882; it enthroned meningeal hæmorrhage for many years as the cause for infantile spastic states dating from birth. It is most accurately recorded and beautifully illustrated, as a case of meningeal hæmorrhage occurring from injury during the process of birth. But it was not a case of meningeal hæmorrhage at all. It was a large cerebral hæmorrhage which commenced deeply and tore up everything in one hemisphere from the basal ganglia to the cortex and even then remained subpial. There was no injury during birth, for Dr. McNutt herself delivered the child and described the birth as having been particularly easy. This hæmorrhage did not occur at birth, but when the child was twelve days old, and in sequence to very severe convulsions, and its advent was marked by the rapid appearance of hemiplegia.

If an infant with cerebral hæmorrhage from injury at birth survives, what will be the pathological end-picture? Surely the only possible result would be some form of cystation or porencephaly. I have seen several cases of porencephaly in which hæmorrhage might have been the original lesion, and some of these I have unexpectedly found in patients who showed neither mental nor physical infirmity, in other words the porencephaly was not associated with symptoms. Porencephaly is more often, I submit, due to embolism or to thrombosis than to hæmorrhage.

I should have expected thrombosis to occur sometimes when the brain is bruised during birth and when the sinuses are injured by undue pressure, and I should be glad if anyone here will enlighten me upon this point. Turning to injuries of the cranial nerves the only one which I have seen is the usual peripheral injury of the facial nerve. These have always recovered in my experience, with the exception of one case in which the physical signs suggested that the temporal bone had been crushed. One has to bear in mind that there is such a condition as congenital absence of a cranial nerve, and I think that on more than one occasion such a case has been recorded as an irrecoverable case of injury to the facial nerve at birth. Only a few days ago I showed before the Clinical Meeting of the Section of Neurology a man with almost total bilateral facial palsy dating from birth. He had been told by an elder sister that both his jaws had been broken at birth and that the condition of the face had resulted therefrom, but there was no sign whatever of injury to the mandibles, and the Section determined that the case was one of developmental absence of both facial nuclei.

I feel sure that the cases of paralysis of the third cranial nerve, sometimes bilateral, and the cases of ptosis which have been recorded as obstetrical paralysis, are in reality developmental defects also, for they seem never to have admitted of any improvement.

I have twice met with fracture dislocation of the spine from traction upon the body with the head after-coming, and in both cases there was a total paraplegia. The level of the lesion was the seventh cervical in both cases. In one of them the spinal cord was completely torn across, and the portion below the lesion was completely necrotic throughout and reduced to what looked like a little strip of wet wash-leather. There was no sign of hæmato-

rachis in either case, but life had continued in both cases to beyond the twelfth week.

I have seen several cases of the Erb-Duchenne type of paralysis from damage to the fifth and sixth cervical roots, and one bilateral case, and have no doubt that these result from undue separation of head and shoulders from any cause whatsoever, as in the adult. I have also come across one case of Klumpke paralysis (eighth cervical and first dorsal roots) from traction upon a prolapsed arm. Most of these cases have not improved, for the reason I think that irrecoverable end-results of these injuries are more likely to come my way than are the recent and recoverable cases. I have never seen any nerve injury in the lower extremity from the process of birth. But though it is quite out of place in this discussion, may I be allowed just to mention the nerve trunk paralyses of the lower limbs which may occur in the mother during labour? Of these I have seen quite a number. First the obturator paralysis which I have several times seen occurring bilaterally, and in which the mechanism of the injury seems obvious, and, secondly, the external sciatic paralysis which seems to me to be due to the more exposed position of the junction of the lumbo-sacral cord and first sacral root, and its longer course against the bone and in the narrower section of the pelvis. But I am unable to explain why the incidence of the damage should be upon the external sciatic fibres rather than upon the rest of the root. Nor can I say why the right side should be affected and never the left side. All the cases of this nature that I have seen have made a speedy and complete recovery.

Mr. H. A. T. Fairbank, D.S.O., M.S.

Birth Palsy.

I have been asked to confine my remarks to injury of the peripheral nerves during birth, i.e., to birth palsy. A few tables made up from my own series of cases will serve to indicate some of the known facts bearing on the incidence of this affection, which is probably not as rare as it might be. It is usually unilateral. In my series of ninety-eight cases, ninety-three were unilateral and only five bilateral, making a total of 103 arms paralysed. It is rather more common in males than in females, the figures being males fifty-six and females forty-one. The evidence seems to point strongly to the traction theory as being the correct explanation of the paralysis. By traction should be understood any position which involves deviation of the head away from one shoulder, which necessarily puts strain on the upper cords of the brachial plexus, as well as direct traction on the arm which is only exceptionally the cause. The theory that the plexus is damaged secondarily, and that the primary lesion is in the shoulder-joint, has little to support it.

A few years ago I ventured to summarize the arguments in favour of the traction theory as follows :—[1]

" Although the occasional presence of definite adhesions in the joint in young infants suffering from birth palsy strongly suggests damage to the joint in these particular cases, I believe the traction theory best explains the condition present in the vast majority of the cases, and that the subluxation of the shoulder-joint develops gra dually and as a direct result of the paralysis. I hold this view for the following reasons :—

[1] *Journ. Orth. Surg.*, ii, No. 5, May, 1920.

(1) Tenderness and thickening over the plexus in the neck have been noted soon after birth in some cases.

(2) The almost constant finding of definite injury, sometimes amounting to complete rupture, of one or more nerve bundles of the plexus during operation. Those most likely to be damaged by separation of head and shoulder, viz., fifth and sixth cervical, are the ones most often found to be paralysed.

(3) Experimental evidence that traction can produce tearing of these nerves, and that damage to the shoulder-joint is extremely difficult to produce, fracture of the humerus or separation of its upper epiphysis occurring first. (Clarke, Taylor and Prout, Thomas and Sever, T. T. Thomas and Stone.)

(4) Taylor actually felt the plexus tear during delivery of a child, and the lesion was proved post mortem."

Direct pressure of forceps or a finger of the accoucheur, though possible, is, I think, the cause of the damage only in very exceptional cases.

It is about four times as common in head presentations as in breech cases, the figures in my series being seventy-one vertex, nineteen breech cases, and two in which turning and forceps were employed, while in six no details of the birth were obtainable. As a rule, but not invariably, labour has been difficult. Instruments were used in half the cases, and in a quarter of the rest labour is said to have been difficult, while in only seven was labour definitely easy. In six plexuses explored in five patients I found: in one, complete severing of the conjoint fifth and sixth cervical nerves, in one the whole plexus was damaged and embedded in a mass of scar tissue, in a third the fifth cervical showed evidence of damage, while in two out of the other three, though the plexus showed no obvious lesion, the fifth and sixth nerves on stimulation gave no response except in the extensors of the wrist in one case. In these last the lesion was probably intra-neural, as in a case reported by Boyd to this Society.[1]

The paralysis is usually of the upper-arm type, affecting the muscles supplied by the fifth and sixth cervical nerves. In the present series ninety-seven showed the upper-arm type of paralysis, six arms showed damage to the whole plexus more or less, while in no case was the lower-arm type (Klumpke) seen. It is generally agreed that the great majority recover more or less completely, certainly to an extent which contra-indicates operation on the plexus.

The typical palsy of the deltoid, spinati, subscapularis, flexors of the elbow and supinators of the forearm needs no detailed description. The serratus magnus is, I think, affected more than is generally appreciated. Obvious signs of recovery are usually present, in my experience, three months after birth if the case is treated properly. Some muscles apparently recover earlier and more completely than others, e.g., the subscapularis, but the biceps is usually the first to show obvious signs of activity. Wrist-drop, though not by any means always present, may persist. Only a very few show marked retardation of growth of the whole arm.

Treatment consists in retaining the arm in the position which relaxes the paralysed muscles, and daily massage and passive movements of all the joints to prevent contractures. The most important is the use of a splint such as the one shown, which holds the arm abducted at the shoulder, and the forearm vertical and supinated. These cases show a marked tendency to contractures, in the first instance of those muscles which have never been paralysed and, later, even in their opponents which have by that time recovered. Want of

[1] *Proceedings*, 1914, vii (Sect. Study Dis. Child.), p. 111.

voluntary activity in spite of recovery of the paralysis to a large extent, is probably the chief cause of this tendency. If there is no sign of recovery in three to six months, confirmed by electrical stimulation under an anæsthetic, the plexus should be explored. As already stated, if properly treated, obvious signs of recovery are usually present in three months, though I admit that some surgeons consider a longer delay advisable before operation is resorted to. Nothing but secondary suture, if the nerves are found torn, or excision of the scarred portions followed by suture, is of any use. Nerve grafting was tried in two cases without real benefit.

Posterior subluxation of the shoulder-joint, which is, I think, by no means rare in children, is secondary to the paralysis, and its development is inevitable if the case is untreated. In all cases seen only after subluxation had developed, a history of birth palsy is obtained. The subluxation does not develop unless a considerable degree of recovery has taken place. The deformity consists in slight flexion and abduction of the humerus with marked internal rotation. External rotation is impossible, and this prevents use being made of the power of supination, in fact the hand is hardly used at all. Abduction at the shoulder is usually very imperfect. The cases in which the displacement is reducible are treated by daily manipulations and the use of the same splint as is employed in the paralytic stage. Thirteen cases were treated in this way. Of the irreducible cases, seven were treated by forcible attempts at reduction under an anæsthetic, but this proved unsatisfactory on account of the difficulty in obtaining complete reduction, and because of the tendency to relapse. The method of choice is by open operation as devised and published by myself some years ago. The operation consists in dividing the tendon of the subscapularis muscle and the contracted anterior part of the joint capsule. Sever modified this operation and recommended division of the pectoral and the subscapularis without, he said, opening the joint. I have not found it necessary to divide the pectoral, and have not found the limited division of the subscapularis sufficient to give complete reduction, except in the case of one patient aged 16 months. My efforts are now concentrated on obtaining free external rotation and avoiding stiffness in the shoulder-joint which is very prone to occur, the small amount of fixed abduction being disregarded. The arm is now fixed in abduction (instead of adduction as in early cases) and in external rotation, and the plaster cast is removed after three or four weeks and replaced by the metal abduction splint, which allows of massage and active and passive movements. Although the other movements of the shoulder may show little, if any, improvement, particularly when the after-treatment is inefficient, outward rotation is always possible after operation and this allows the hand and forearm to be used in a way that was previously impossible. The tendency to adhesions and contractures of all joints, and not only of the shoulder, still persists in these children and has to be kept constantly in mind.

Dr. Thomas Lumsden.

On the Effects of Hæmorrhages into the Brain Stem.

The following remarks refer chiefly to the effects of hæmorrhage into the brain stem experimentally produced in mammals; but I hope they are not out of place in this discussion, since we may safely assume that hæmorrhage into the infantile brain stem must have similar results.

This assumption is indeed no longer a matter of conjecture, for during the

past year I have examined in serial section two human brain stems into which hæmorrhages had occurred at or just after birth. The results observed corresponded accurately with the experimental findings. Before death these infants showed the type of breathing presently to be described as apneustic.

(A section of one of these human brain stems was shown.)

During an investigation into the central nervous mechanism of respiration reported elsewhere,[1] it was desired to sever intracranially the fifth, eighth and tenth nerves. This is a delicate and difficult procedure, and until the necessary skill was acquired certain unexpected results were observed. These effects proved to be due to hæmorrhage into the brain stem from raising up the brain and dragging on the nerves during attempts to cut them. In this way the effects of serious hæmorrhages at the levels of origin of these nerves were observed, and they were found to correspond exactly with the results of section of the brain stem just below the level into which the hæmorrhages took place. The alteration in the position of the cephalic contents sometimes occurring during birth, which Mr. Eardley Holland has described, must I think involve a considerable alteration in the position of the brain stem. Now, since the cranial nerves and blood-vessels are fixed where they traverse the bones, any shifting of the pons and medulla must drag on these nerves and blood-vessels quite as much as happened in my experiments. We have here, then, a possible explanation of the occurrence of deep hæmorrhages so often seen and the cause of which is uncertain. The results to be detailed were so frequently and fully confirmed by dividing the brain stem at different levels in some 300 animals—cats, dogs, monkeys and rabbits—that they may, I think, be regarded as constant and reliable.

It is necessary to appreciate some recent work on the respiratory centres if we are to locate hæmorrhages into the brain stem, for it is only by noting certain alterations in the character of the breathing that it is possible to predict ante mortem the exact level of the damaged area and to have some grounds upon which to base a prognosis. I will therefore deal very briefly with certain respiratory changes which result from injury to the brain stem.

A hæmorrhage into the brain stem, even if it put out of action the whole of the central system in front of the pons, leaves unconscious respiration normal (as does section anywhere above level 1). We may, therefore, conclude that all the respiratory centres lie below this level, i.e., in the pons and medulla.

A serious hæmorrhage into the upper half of the pons, such as is produced by even gently dragging on the fifth nerve roots, or section at the mid-pons, destroys the highest or pneumotaxic respiratory centre and a profound alteration in the breathing invariably and immediately occurs. The breathing becomes of the type which I have named "apneustic."

An apneusis is a prolonged inspiratory tonus or holding of the breath; a gasp is a sudden involuntary incoördinated inspiration.

Apneustic Respiration.

A steady, deep inspiration is taken and retained, the chest maintaining the inspiratory position for two to three minutes until, indeed, asphyxia supervenes; the chest then sinks slowly and the blood-pressure falls hurriedly, a few gasps are now taken which re-oxygenate the blood, the blood-pressure rises rapidly, a fresh inspiratory tonus follows, and this cycle repeats itself for two or three hours

[1] *Journ. of Physiol.*, 1923, lvii, pp. 153 and 353; lviii, pp. 81 and 111.

with great regularity. The inspiratory tonuses (or apneuses) shorten and finally, for an hour or so before death, only expiratory spasms and gasps occur.

In passing, I may just mention that if during apneustic respiration asphyxia is prevented by continuous ventilation of the lungs, the inspiratory tonus does not end after two or three minutes, but is maintained indefinitely. I have seen it last for over half an hour, only then ending as the result of experimental interference. If, during such a prolonged apneusis, we stimulate the vagus periodically, it is possible so to inhibit the inspiratory tonus as to produce a very fair copy of respiration of normal type. It would appear, therefore, that normal breathing results from periodic inhibition of the inspiratory tonus produced by the apneustic centre through the activity of certain cells and synapses in the upper half of the pons, to which the name "pneumotaxic centre" has been given.

The apneustic type of respiration (which had long ago been noticed by Rosenthal, Marckwald and others) is, we find, normal in aquatic mammals, whales, seals, sea-lions, &c., and in reptiles such as the crocodile, tortoise and turtle. It is a half-way stage between the gasping of a fish out of water and the highly developed respiration of terrestrial mammalia.

Passing now to a lower area: hæmorrhage into the level of the striæ acousticæ (i.e., about the middle of the medulla) destroys the apneustic centre and with it the power to affect inspiratory tonus or retain the breath when taken. The animal is now able to perform only two respiratory acts, namely, expiration and gasping.

The expiration is tetanic or spastic in type, and may be so intense and active as to cause, by what Sherrington has called "irradiation," sickness or even convulsion.

If the hæmorrhage or section is just below the striæ, the animal is no longer able to perform active expiration of any sort, for the expiratory centre situated at this level is destroyed.

Life will now be preserved for a short time by gasping alone, and indeed this is the only act of which the so-called respiratory centre at the "nœud vital" is capable. This centre has, therefore, been renamed the "gasping centre." Whenever pure, regular gasping occurs, we may conclude that the whole central nervous system above the apex of the calamus scriptorius is temporarily or permanently out of action—a state of matters which almost invariably precedes death: hence the familiar expression, "at the last gasp."

The existence of the four respiratory centres referred to above thus enables us to locate hæmorrhages into the brain stem. Time prevents my mentioning some intermediate types of breathing, such as periodic respiration and periodic gasping, which indicate minor degrees of damage to the various respiratory centres, but if anyone is interested in the matter I shall be very pleased to show him further tracings.

Just a word as to treatment. This must be immediate if it is to be of any use. In animals artificial respiration is most effectively achieved by pumping or blowing into the lungs oxygen to which 5 per cent. of CO_2 has been added, the latter in order to stimulate the respiratory centres since O_2 alone has no such stimulant effect. This percentage of CO_2 is quite harmless; I have even found that cats and rabbits can breathe 30 per cent. of CO_2 for two hours with impunity. If the heart has stopped cardiac massage must be instantly performed.

As to prognosis, the lower down the brain stem the more serious are the effects of hæmorrhage, but recovery is possible whatever the level affected

if the hæmorrhages are small and not very numerous. I have seen many such recoveries in animals, the existence of hæmorrhages being in all cases confirmed post mortem. If in the cat the heart has stopped so that the cerebral circulation ceases, unless it can be restarted within five minutes, the cerebral hemispheres will not revive. If the circulation has ceased for fifteen minutes the whole of the brain stem is irrevocably dead. So that while in the cat the heart may be restarted by massage and injections of O_2 and adrenalin, even after it has ceased beating for an hour or more (sixty-seven minutes in one experiment of mine), yet it is quite useless, for the central nervous system has been long dead and can never resume its activity, though the heart may continue to beat for several hours.

It seems probable that in man these viability periods (five to fifteen minutes) would be yet shorter than in the cat, so that most of the cases of attempted revival reported in the press are of necessity quite hopeless and unavailing.

In conclusion may I just recapitulate: (1) If after an intracranial hæmorrhage the normal type of breathing continues we may conclude that there is no serious injury to the brain stem; (2) if the respiration is apneustic in type the hæmorrhage is in the pons; (3) if only expiratory spasms and gasping occur the damage is at the level of the striæ, and (4) if gasping alone is seen then the dominant hæmorrhage is just above the "nœud vital," near the apex of the calamus scriptorius.

Dr. Gilbert Strachan.

In 100 cases of still-birth examined, dural laceration was found in twenty-three, and in fifty cases of neonatal death a similar lesion was present in twelve. The total was thus thirty-five out of 150 cases of stillbirth or neonatal death, approximately 23 per cent. In one stillborn case the seat of the laceration was along the left coronal suture; it extended to the superior longitudinal sinus from which profuse bleeding had occurred, and was caused, no doubt, by extreme overlapping of the adjacent cranial bones. In the other thirty-four cases the laceration was in the tentorium and was accompanied by hæmorrhage of varying degree in twenty-five cases.

The origin of this hæmorrhage and the mechanism of the tentorial laceration have been discussed and investigated by Eardley Holland; in his view the usual source of bleeding is from the distended tributaries of the vein of Galen. In a number of the present series the bleeding did not appear to be sufficiently extensive for such an origin, being represented by only a little clot. In these cases it is supposed that the source of hæmorrhage was either the veins connecting the tentorium with the under surface of the occipital lobes or the vessels between the two layers of the tentorium. Intracerebral hæmorrhage was rare and it was found that tentorial laceration was by far the commonest cause of intracranial hæmorrhage.

Of these thirty-five cases it was found that fifteen or 33 per cent. were delivered by the breech; when it is remembered that three is the usual percentage of breech deliveries, we conclude that, so far as our figures show, the liability to tentorial laceration is increased eleven times in breech presentation. But of these fifteen cases only nine or 25 per cent. were primarily breech presentation, the other six having been turned for various obstetrical reasons. If we take this lower figure we find that tentorial laceration is eight times more likely to occur in breech than in head-first cases. The importance of this conclusion is obvious and points to the necessity for prophylactic external

version in all breech cases, especially in primigravidæ. Of the thirty-five cases, two were postmature, twenty-four were at term and nine were premature, but only slightly so. Only two were vertex cases born without assistance and in normal labour.

In neonatal cases the duration of life varied from one hour to four days, and the symptoms preceding death also varied. In some cases the infant presented no abnormality at birth, and death in these cases usually occurred suddenly; in other cases the child was blue at birth and never acquired a good colour, while respiration was irregular. Generalized twitchings and convulsions were seen in a number of cases, while in others inability to suck, accompanied by drowsiness deepening to coma, was present. In two cases the child became progressively weaker and "faded away."

In the majority of cases under discussion obstetrical difficulty in varying degree was present. While in some cases easy extraction by the forceps resulted in the birth of a stillborn fœtus, in others the child was born alive after difficult forceps extraction. Thus it would appear likely that there is a wide factor, idiopathic to a particular case, determining the likelihood or not of such a lesion occurring in a degree sufficiently marked to cause death.

Finally, from the large proportion of breech deliveries and cases of difficult labour in which tentorial laceration was found, it is reasonable to assume that in a certain number of cases tentorial laceration (probably of slight extent) may be produced and yet the child be born alive and continue to live. In such cases it is probable that coincident hæmorrhage had not occurred.

Dr. Bernard Myers

stated that the case of hæmorrhage into the pneumotaxic centre referred to had been published by Dr. Kirkwood and himself in the *Lancet* of July 14, 1923.

Briefly, he was called to see the infant, a male child, three days old. The mother was a primapara, aged 25. The birth had been accomplished without instruments, &c. The infant was two weeks premature but was a fine baby. After birth some cyanosis was noted for an hour or so, but no respiratory difficulty occurred. Meconium and urine were duly passed. Upon examination the right arm and leg were found to be slightly rigid. Shortly afterwards a few twitchings were noted in the right hand and later in the left hand also. Quite suddenly the infant took a little deeper inspiration than usual and from normal respiration went into a state of inspiratory apnœa. Cyanosis appeared, which gradually became more marked. After about twenty seconds there were two or three short irregular respirations when normal breathing became re-established and the cyanosis passed off. In about twenty minutes a similar attack was witnessed; this lasted forty seconds. Several further attacks took place during this (the third) day, and finally there were convulsions, but the two occurred independently of each other. At 8 p.m. the same day the child gave one deep inspiratory gasp and died.

Nothing abnormal had been noted in the heart or lungs, nor was there any sign of injury, hæmatoma, meningocele, increased intra-cranial pressure, purpura, &c. A provisional diagnosis of affection of the respiratory centre, probably due to hæmorrhage, was made, with possibly similar lesions on the cerebral cortex. Permission having been obtained a necropsy was carried out, and as Dr. Thomas Lumsden's recent observations on the respiratory centres in the cat, in the *Journal of Physiology*, appeared to throw light on the case he was asked to report on the necropsy, which he kindly did.

There were a number of subpial punctiform hæmorrhages both on the con-

vexity of the brain and the walls of the lateral ventricles. Microscopically a very few minute capillary ecchymoses were seen in the upper part of the pons and all the capillaries were much dilated, but except in the area just above the striæ acousticæ serious extravasation had not taken place. Into this region—just above the level of the olivary bodies and constituting the lower 2 mm. of the pons—five or six small hæmorrhages had occurred. The largest (on the right side) measured 2 by 1·5 by 1·25 mm. and a similar one was found in a nearly symmetrical situation on the other side. Applying Lumsden's work on the cat to these necropsy results these hæmorrhages would account for the inspiratory apnœa (apneustic type of respiration).

There were likely to be many similar cases reported.

Mr. D. McCrae Aitken,

speaking as a representative of the Section of Orthopædics, said he hoped that amidst all this gloom of post-mortems this discussion would point to some little light in regard to these children in the future. So far, the only bright and cheerful speaker who had been heard was Dr. Cameron, if one excepted the one hopeful passage of Mr. Eardley Holland's speech, in which Mr. Holland pointed out that the irregularities of the cranium were due to rupture of a ligament. He (Mr. Aitken) foresaw the day when the general practitioner and the general surgeon would be educated in other branches of orthopædics, and then these irregularities of the cranium would be corrected. The neurologists had been of very little assistance to him in the practical treatment of spastic paralyses of all varieties. In the hospitals for the crippled there were cases showing distinct varieties of spastic paralysis arising from cerebral disturbances. In those cases one could at once separate off the paraplegia pure and simple. There were few cases of paraplegia without definite impairment of intelligence. He agreed with Dr. Cameron that the sooner one got these children to use their motor system, so as to get into touch with their surroundings, the more rapidly would results from the re-education occur.

The first and most striking case of this type he had seen was a very gross one. It came under his notice when he was seeing out-patients with Sir Robert Jones in Liverpool. The child was aged 5 years; it exhibited facial grimaces, some irregular athetoid movements of the hands, and a very pronounced spasm in the lower limbs. He regarded the child as an idiot. But Sir Robert spoke to the child, and after a little time noticed that the child was watching him, and therefore took the child in hand, to see what he could do with it. In those days section of nerves was not done. Sir Robert Jones still believed in putting large masses of hypertonic muscles out of action by direct section, or by overcoming them by splintage. In that case, within forty-eight hours the night cries had ceased, and in six weeks that child, which had been merely making noises barely understood by the mother, was able to speak short sentences, showing that it had been receptive during the previous periods, and only an inability to use the muscles of the tongue prevented it getting into proper touch with its surroundings.

There was another child, a girl, whom he had seen at Queen Square Hospital, with Dr. Grainger Stewart. She had violent spasms of the head to the left, turned the eyes to the right, there was contracture of elbows and wrists, contractures of the adductors and flexors of the knees; this condition obviously indicated some degenerative or under-developed condition of the nervous system. That child had a very devoted father; all the other children had been either stillborn or had died immediately after birth. Every evening the father had read industriously to the child, and the child sat looking at the book. The mother could not interpret everything the child tried to say. But at 11 years old that child was reading an ordinary book, though how she could keep her eyes steadily on the book he (Mr. Aitken) did not know. In addition, she had voluntary control of the left foot, and of that only, and with that foot she taught herself to write. He had the child

admitted into hospital, and tried to correct some of her grosser deformities; the spasm was attacked by persistent splinting. The spasm of the right arm had now been got rid of, and she was being taught to write. Then he believed she could be taught to walk, because she had some notion of control of her lower limbs.

Dr. Collier had made a statement which he (Mr. Aitken) could not accept, namely, that the final appeal must be to neurological physicians. Cases could be carried a stage further than a mere diagnosis of the portion of the brain in which the damage occurred; there were possibilities of restoring function by orthopædic means. He would enter a plea that the neurologists, more particularly, should send the cases early to the orthopædic surgeon, so as to enable him to undo deformities and prevent deformities occurring. Many cases, though seemingly hopeless, presented great possibilities as a result of careful re-education, though the prognosis was still far from clear.

Mr. Bright Banister

said he trusted that the gloom mentioned by the last speaker had not also enveloped the obstetricians, who, he was sure, had listened to the discussion with the greatest pleasure. From his own point of view, the discussion had been nothing but hopeful. He was relieved to hear that these cases of intra-cranial hæmorrhage were not necessarily doomed to absolute unfitness in after-life.

From the obstetricians' point of view, he thought the subject fell under three heads: (1) the causation of these intracranial hæmorrhages; (2) the fatalities attending them; (3) the possibility of preventing such happenings.

In regard to causation, he expressed his full agreement with the remarks of Mr. Eardley Holland in regard to the treatment of breech deliveries, in that one ought not to imagine that from the time the umbilicus was born, one had five minutes only in which to deliver a live baby. He (Mr. Banister) had been the central actor in a play of that sort, when fourteen and a half minutes elapsed between the delivery of the umbilicus and the final delivery of the baby; and great was his astonishment at finding that the baby was breathing naturally after about thirty seconds, and that it continued to do well. It could not be too much stressed that the idea of hurry in breech deliveries was apt to be overdone, and that the more they were left alone, and the more interference was withheld until some indication was afforded, the better were the results.

With regard to the causation of birth palsies, he (Mr. Banister) thought there was one factor in the traction theory which he had not noticed mentioned in the discussion, namely, the traction which was exerted on the head after it was born, so as to try to deliver the anterior shoulder. He had often seen the head drawn back towards the mother's anus, in a manner calculated to disturb, if not indeed destroy, the continuity of the nerve roots of the brachial plexus.

As to the prevention of these various accidents, he thought the obstetrician had again to continue on the somewhat irksome path of trying to impress upon general practitioners the importance of avoiding difficulties, rather than to wait to overcome them when they had occurred. It was one more illustration of the truth that, in many cases, a difficulty foreseen was a difficulty obviated.

Mr. Eardley Holland (in reply)

said it had become clear that the aspect of the subject which chiefly merited discussion was the remoter effects of these injuries; this had become evident in the remarks of Dr. Hector Cameron and Dr. James Collier, and it was easy to see that there was considerable difference of opinion between those two gentlemen, who represented different schools of thought.

A great deal could be done towards settling the matter if finer investigations were made of brains of fœtuses and newly-born children. Obstetricians and obstetric pathologists had been ploughing a very lonely furrow in the last few years; they had done a great deal of coarse pathological investigation into these cases, but they knew little about the finer points of cerebral pathology. Neurologists themselves, so far, had shown very little interest in the work, and their collaboration would be most welcome. But there was one aspect of these cerebral hæmorrhages which had scarcely been mentioned, namely, the hæmorrhages into the actual substance of the cerebrum, especially the finer hæmorrhages.

One of the most important points which had been brought before the meeting was Dr. Lumsden's account of his examination of the brain stem of the infant under Dr. Myers' care. If more fœtuses' brains were hardened and then examined in respect of their finer pathology, much fresh knowledge would come to light.

Dr. Cameron (in reply)

said he had been immensely interested in the useful discussion which had taken place, and particularly in the physiological work on the respiratory centre which had been put before the meeting.

It must be recognized that the contention was not that birth injury was the cause of all or most cases of spastic cerebral palsy but that it did account for a comparatively small proportion of the total. It was, he thought, just those cases in which damage seemed to be confined to the sensori-motor side that were most clearly due to intracranial injury at birth.

A fortnight ago he had shown, before the Section for the Study of Disease in Children, three cases of Erb's paralysis in one family of six.[1] He had been wondering whether a similar experience was common. The mother was not found to have any pelvic abnormality, the labours were normal, but two of the children were large.

Dr. James Collier (in reply to Mr. Aitken)

said that no one had written more about the excellent effect of continued hard work in the treatment of infantile spastic states, nor was anyone present who had seen more excellent results in the apparently hopeless cases, than he himself had.

With regard to this difficult pathological question, inasmuch as in a case of infantile spastic paralysis presumably dating from birth one found an irregular lesion such as could possibly have resulted from a bruise, thrombosis or hæmorrhage, he would accept that as a birth injury. But when one came across a brain with no differentiation between grey and white matter, which surely must have stopped in its development at about the third month, and when one saw every degree of systemic neuronic atrophy, with no sign of any tissue absorption and injury, and more especially when one saw such cases—he had seen six consecutively—in which there was no gross lesion at all, the matter was different. Professor Dana once said to him, "I had three cases of diplegia and found no gross lesion." He replied to the Professor that surely he did not expect to find one: and to this Professor Dana retorted that he expected to get evidence of meningeal hæmorrhage. It was in face of those pathological facts that he (Dr. Collier) argued that most of the infantile spastic states were prenatal in origin: and until rows of specimens with another sort of pathology could be put up against the rows already shown in which there was a neuronic lesion only, he thought it would be allowed that the soundest argument was that many of the spastic states which appeared to date from birth were really installed in fœtal life some time before birth.

[1] *Proceedings*, 1924, xvii (Sec. Study Dis. Child.), p. 29.

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Clinical Section.

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A Case of Thoracic Dermoid.

By F. J. Hathaway, M.D.

E. H., male, aged 21, was admitted into King Edward VII Hospital, Windsor, on August 21, 1920, with the history that about a year previously he had been operated upon by Mr. Sampson Handley, at the Middlesex Hospital, for a central abscess of the chest, cause unknown.

On his admission to King Edward VII Hospital there were on the left side of his chest two sinuses, entering deeply into the thorax, discharging clear serous fluid with a little pus. These sinuses were on the front of the left chest, external to the heart, and over the fifth and sixth ribs.

Operation.—Under a general anæsthetic I excised portions of the fifth, sixth and seventh ribs and traced the course of these sinuses into the chest. They contained polypi and arose from the lung itself. I excised a large mass of fibroid tissue from the lung as large as a fœtal head, leaving a cavity of this size, bounded on the inner side by the pericardium, above by lung, below by diaphragm, and on the outer and posterior sides by ribs. This tissue, of which I show slides, was tough and fibrous.

I filled the cavity left with my thymol paraffin wax and as at the end of three months it had not healed I excised some more of this fibrous tissue and transplanted skin flaps into the cavity. He now has a skin-lined cavity about the size of a tangerine orange, with his heart and pericardium internally, lung above and diaphragm below and chest wall behind and to the outer side. He is perfectly well and able to work on his father's farm. He has no shortness of breath.

The pathological report states: "This mass consists of several large polypi and cysts of varying size, all apparently forming part of the same tumour. Microscopically the most prominent polypus shows only the structure of old granulation tissue—in some places adenomatous, in others fibrous—while the deeper parts of the mass, and the tissue in which the cysts are embedded, is made up of a substratum of similar growth, infiltrated by a cystic papillary adenoma resembling one of the tumours that occur in the female breast."

Pigmentation of Skin.

By Douglas Firth, M.D.

F. S., male, aged 60, occupation, seaman and general labourer.

History of present illness: The discoloration of the skin was first noticed six years ago, and has gradually and continuously increased in intensity. Recently there has been a little dyspnœa on exertion and cough at times.

Five years ago the patient attended the Hull Infirmary on account of this pigmentation, and three injections were made into his arm.

Past history: Has had gonorrhœa and syphilis, but has suffered from no other illness that he can remember, and has never undergone any prolonged course of treatment. No history of industrial poisoning or tropical diseases. Bowels regular. Habits are not unduly intemperate.

Family history: No abnormalities elicited.

On examination: Diffuse pigmentation of the skin is present, most intense on the face and hands, less marked on the trunk and arms, and least on the lower limbs. The mucous membranes are pigmented. The colour on the face and hands is leaden or very dark slate, but in the less pigmented parts there is a suggestion of dirty lilac. The tongue is fissured with a leucoplakic patch.

Apart from slight emphysema no abnormality of the respiratory, cardio-vascular, nervous or alimentary systems is detected, and the liver and spleen are not palpable. Urine, acid; no albumin nor sugar. No excess of pigment or of abnormal pigments. Eyes: Retinæ normal, discs clear. No sign of any pigmentation. Nails somewhat curved. Blood-pressure, 150.

Blood examination: Wassermann, positive; red blood cells, 5,572,000; hæmoglobin, 98 per cent.; colour index, 0·88; leucocytes, 14,700, differential count: polymorphonuclears, 60·4 per cent.; eosinophils, 0·4 per cent.; lymphocytes, 37·2 per cent.; mast cells, 2 per cent. Spectroscopic examination of blood shows normal hæmoglobin bands. Fragility of red cells. Trace of hæmolysis in 0·48 per cent. saline (normal 0·42 per cent.). Blood diastase, 8 units (normal, 3-10).

Dr. Whitfield kindly saw the patient for me, and said the appearance was similar to that of the only case of argyria he had seen. He also examined the sections of a piece of skin removed, and he could not detect any excess of pigment or other abnormality.

Loewi's test: Installation of a drop of 1 in 1,000 adrenalin hydrochloride causes dilatation of the pupil.

I am indebted to Dr. G. A. Harrison for the time and care he has spent in working out the bio-chemical reactions of this case.

Postscript: October 25, 1923.—Further examination of sections of skin shows undoubted argyria.

Familial Facial Telangiectases.

By C. F. T. East, M.B.

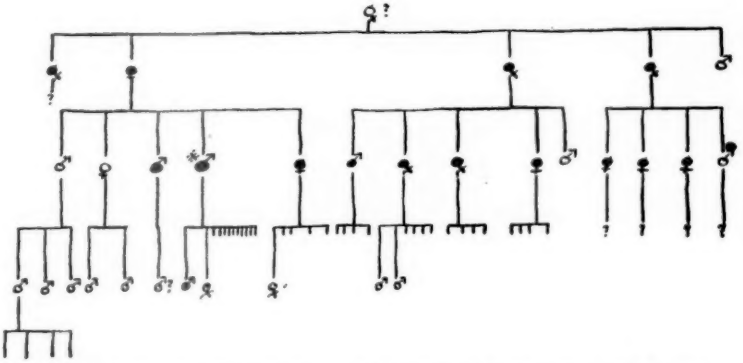
W. P., MALE, aged 48, was admitted with chronic bronchitis and emphysema with myocardial failure.

Scattered over the malar region on each side and down the cheeks are multiple telangiectases. There are also a few over the eyes, and just inside the nose on the septum and on the end of the nose. The mouth is free from them. These appeared about the age of 23. He has had attacks of epistaxis very frequently.

There is a marked familial incidence. His mother and two aunts had them, also two of his sisters. They are appearing on his eldest son, who is now 23. This is about the age at which they have appeared in all cases occurring in his family, all the affected members of which tend to suffer from epistaxis.

As far as can be traced at the present, there are about fifteen members of his family with multiple telangiectases of this nature, the distribution always being on the face.

As already stated, the age at which the telangiectases appear is about 23, and 25 is the latest age at which they make their appearance; the individuals who are clear at the latter age escape altogether.



FAMILIAL FACIAL TELANGIECTASES. CHART SHOWING FAMILY TREE OF PATIENT.

Where an individual is affected the circle of the sex sign is filled in black. Where no information is available the sign ? is used. Where the individuals are under the age at which the lesions usually appear the line of descent is left short. The patient is marked *.

Dr. F. PARKES WEBER remarked that he had described a case of familial telangiectatic epistaxis in the *Lancet*, 1907, ii, p. 160. He had endeavoured at the same time to refer to all the cases previously published, but an interesting paper by R. H. Kennen, published in the *Medical Press* for 1902 (new series, vol. lxxiii, p. 458), had escaped his attention and apparently that of subsequent writers on the subject. "Hereditary Hæmaturia," as described by Attlee, Aitken and L. Guthrie, was perhaps due to an analogous telangiectatic or hæmangiomatous condition in the pelvis (calices) of one or both kidneys. Similarly, "Hereditary Hæmoptysis," as described by E. Libman and R. Ottenberg, was perhaps due to a telangiectatic condition in a portion of the respiratory passages. Dr. Weber thought it was most important to remember that all such familial syndromes might likewise occur, in an apparently non-familial manner, in isolated individuals. Thus, some isolated cases of unilateral recurrent or long-continued hæmaturia had been proved (by examination of the excised kidney) to have a telangiectatic or hæmangiomatous origin.

Case of Tumour of Face.

By C. A. JOLL, F.R.C.S.

PATIENT, a male, aged 53, has had a swelling of the right side of his face since birth. It is a large, soft lobulated growth of a dusky red colour and extends over his chin, right side of face, right ear and on to the right side of his neck.

Until eighteen months ago he was employed as a pitch carter. Two years ago a wart appeared on the tumour ; it has grown slowly since and three other smaller warts have appeared—the largest is about the size of an orange pip. None were ulcerated, nor was there any surrounding induration, nor were neighbouring glands enlarged.

The warts have been destroyed by application of the blunt diathermy terminal. An area of about one square inch of the tumour was needled by diathermy to see if any improvement could be effected. There was no hæmorrhage.

I should be glad to receive suggestion as to treatment.

Case of (?) Syphilis of Lung.

By L. S. T. Burrell, M.D.

Patient, a male, aged 43. History : Onset with hæmoptysis in January, 1922. Several attacks of slight hæmoptysis since. Cough and sputum developed but tubercle bacilli were never found.

Seen in March, 1923. Dullness between the vertebral borders of scapulæ and infiltration of right apex. Fingers clubbed. X-ray showed heavy hilum shadows and opacity at right apex. Wassermann reaction positive. Vital capacity 2,600.

Antisyphilitic treatment was given and he quickly improved and was able to return to light work in July. The vital capacity improved and increased to 3,000. The absence of tubercle bacilli and quick response to treatment suggest the diagnosis.

Case of Pharyngeal Diverticulum.

By T. P. Dunhill, C.M.G., M.D.

Patient, a male, aged 42, tailor by occupation. Duration of disease : Three years. Admitted June 23, 1923, under Sir Percival Horton-Smith Hartley, into St. Bartholomew's Hospital. History: Complains of difficulty in swallowing, and vomiting. First suffered from dysphagia in 1920. Vomits liquids immediately. Some food passes down, but some is invariably vomited after a variable time. There is a choking sensation before he vomits. The vomit consists of clean food and mucus. Gurgling sound on swallowing. States he has not lost weight (although he has gained 14 lb. in eight weeks after its removal). The swelling appears in the neck on patient swallowing food. Operation: June 29, 1923, by Mr. Dunhill, under local anæsthesia, pouch freed and delivered. Operation: August 9, 1923, under general anæsthesia, pouch excised and opening into pharynx closed. The pouch is exceptionally large, and the wound has healed—after removal of the pouch—completely and at once, without sinus formation.

Clinical Section.

President—Mr. G. E. Gask, C.M.G., D.S.O., F.R.C.S.

Case of Endothelioma of Upper Lip.

By Geoffrey Keynes, F.R.C.S.

Patient, a female, A. H., aged 60.

History: In 1911, patient had a hard solid lobulated growth in the upper lip, right side, which was removed at St. Bartholomew's Hospital.

In 1918, a recurrence was first noticed. A tumour resembling the first one was removed in 1919, at St. Bartholomew's, through an incision in the mucous membrane, but it recurred very soon.

She now (1923) has a hard non-infiltrating tumour in the same place as before. Also a hard fixed submaxillary gland on the same side. The case is exhibited on account (1) of the length of the history; (2) of the question of treatment.

A section from the first recurrence is exhibited.

Sections of Skin from Case of Pigmentation (Argyria).

By G. A. Harrison, M.B.

(Shown at Meeting on October, 12, 1923.[1])

The microscopical sections show fine particles of silver, deposited in the basement membrane of the sweat glands. One section has been treated with potassium ferricyanide and sodium thiosulphate to bleach out the silver.

Examination of the paraffin block containing the piece of skin shows that the silver deposit is visible macroscopically. The sections undoubtedly prove the case to be one of argyria.

Dr. Firth said that on receiving this definite evidence of the nature of the case from Dr. Harrison, he had again communicated with the patient and had elicited the fact that a daily application of silver nitrate had been made to the tongue, for the treatment of a leucoplakic patch, for some sixteen years past. The evidence was therefore complete.

1 See *Proceedings*, 1923-24, xvii (Clin. Sect.), p. 1.

A Case of Deformity of the Chest.

By Douglas Firth, M.D.

E. S., female, aged 13, shows marked deformity of the chest. The deformity was first noticed when the child was 2 years old. She was a bottle-fed baby, who "never did well," and was nursed and carried about a great deal by her mother, who at that time was engaged in the care of a sick relation.

On examination, in addition to the deformity, there is seen to be fibrosis of the left lung, and some bronchitis in the right lung. The breathing is almost entirely diaphragmatic. The head is rickety, and there is early clubbing of the fingers.

I suggest that, contrary to the usual sequence, the deformity is the cause of the fibroid lung, that the predisposing cause of the deformity is rickets, and that the actual cause is the carrying of the child in the mother's arms, for the right hand of the child fits into the hollow on the right side of the chest, and the carrying arm (of the mother) into the spinal curve.

Dr. Myers said that the majority of these cases of extreme deformity were due to osteogenesis imperfecta, but there had been no fractures or other evidence of this in the present case, in which he thought rickets was the cause.

Bony Ankylosis of the Lower Jaw. Arthroplasty with Full Restoration of Active Movement after Eleven Years.

By E. G. Slesinger, M.S.

Patient, a male, aged 17.

History.—When 6 years old he had scarlet fever, and following that an abscess formed in the region of the right temporo-maxillary joint. This was drained and ankylosis of the jaw followed. Since then he has been unable to move his lower jaw. He has been in many hospitals, where wedging was tried without success.

When first examined the lower jaw was seen to be underhung, underdeveloped, and immobile. The molars, however, were in apposition. When he tried to open the jaw, it was thought that a slight gliding took place in the left joint.

Operation.—October 26, 1923. A T-shaped incision was used, and the posterior part of the zygoma was exposed and removed. The parotid was partly retracted and partly divided, and the masseter was pushed downwards. The temporal muscle was completely detached, and the base of the condyle was cut through with a chisel. The bone sprang apart and the jaw was wedged open. A flap of temporal muscle and fascia was fixed between the fragments and the incision was closed. The operation was hurried, owing to great difficulty in breathing.

Result.—Within four days patient could open and close the jaw, and could chew toast and biscuit for the first time for eleven years. He has a partial facial paralysis affecting the forehead, side of the nose and upper lip, which is, however, disappearing. His gape is normal, and he can bite and chew painlessly and well. The speech is still rather thick, and is to be re-educated. Some septic teeth, found when the mouth was opened, have been removed. The apposition of the molars is perfectly good, but the front part of the lower jaw is of course underhung.

Postscript.—January 22. The facial paralysis has completely cleared up, except as regards the frontalis. The speech is normal and all movements of the jaw are full and free.

A Case of Lymphogranulomatosis Maligna (Hodgkin's Disease) with Recurrent Purpura and Hæmorrhagic Symptoms—also Remarks on Lymphogranulomatosis Maligna.

By F. PARKES WEBER, M.D.

THE following case of lymphogranulomatosis maligna (Hodgkin's disease) derives its chief interest from the unusual association of the disease with recurrent purpura and hæmorrhagic symptoms. In its later stage it simulated a case of chronic septic pyrexia, with splenomegaly and purpura, and the presence of a systolic mitral murmur made it difficult to exclude chronic malignant endocarditis (endocarditis maligna lenta).

The patient, Mrs. L. S., aged 47, was admitted to the German Hospital on September 22, 1923, with irregular fever, moderate splenomegaly and the remnant of cutaneous purpura. According to the history obtained some lymphatic glands had been excised on the left side of her neck at the Royal Northern Hospital in September, 1920, after they had been obviously enlarged for five months. Since then she had had amenorrhœa. Before the operation some bad teeth had been extracted. About this time, but apparently not in connexion with the extraction of the teeth, she had had a little bleeding from the mouth. The cutaneous purpura had first been noticed in July, 1921, and about that time there had been blood in her urine. Enlargement of lymphatic glands in the left axilla had first been noticed about September, 1922. For six and a half weeks from May 30, 1923, she had been an in-patient at the Royal Northern Hospital, and Dr. W. M. Dickson, resident medical officer, kindly informed me that the diagnosis had been purpura. When in that hospital there were petechiæ over the chest, and there was some blood in the stools. Obstinate constipation. Enlarged glands in left axilla. *Heart:* faint systolic murmur at the mitral area. *Spleen*, not enlarged. Much pyrexia, with remissions. No tubercle bacilli found in the sputum. A blood-count gave: Hæmoglobin, 80 per cent.; erythrocytes, 5,000,000 to the c.mm. of blood; white cells, 4,400; colour index, 0·8. The lymphatic glands excised from the neck in 1920 had suggested lymphoma or simple inflammatory enlargement; the history had pointed to the latter. Here I would remark that a "biopsy" microscopical examination of an excised gland in early cases of Hodgkin's disease does not always reveal the changes characteristic of the fully developed disease.

As already mentioned, patient was admitted to the German Hospital on September 22, 1923, with fever, moderate splenomegaly and remains of a purpuric eruption. She had a packet of enlarged lymphatic glands in the left axilla, said to have been noticed first about a year previously. None of the other superficial lymphatic glands were enlarged, but there was a scar on the left side of the neck from excision of glands in 1920. She was not emaciated. There were no signs of pulmonary tuberculosis. The Pirquet's cuti-reaction for tuberculosis was weakly positive, as it is in most adults. The Wassermann reaction for syphilis was completely negative. Some of the teeth were loose, with suppuration around them. The heart was not decidedly enlarged; occasionally a slight mitral systolic murmur was present, but otherwise there was no sign suggesting any valvular disease. Brachial systolic

blood-pressure: 100 mm. Hg. Urine: free from albumin and sugar. Nothing abnormal by ophthalmoscopic examination. Blood-count (by Dr. G. Welsh, September 24, 1923): Hæmoglobin, 60 per cent.; erythrocytes, 4,480,000 per c.mm. of blood: white cells, 2,800 (leucopenia). The differential count of white cells gave: Polymorphonuclear neutrophils, 65 per cent.; lymphocytes, 30 per cent.; transitionals and large mononuclears, 3 per cent.; no eosinophils; mast-cells, 2 per cent.

In the hospital the pyrexia varied a good deal from time to time, and the patient was obviously losing ground. Removal of some of the loose teeth, and a trial of "electrargol" treatment did no good. Towards the end the pyrexia was higher and the patient was drowsy or semi-comatose. During the last three days an extensive patch of ecchymosis appeared in the skin of the left front of the abdomen. She died on October 13. Blood-cultures had been attempted on October 9, a few days before death, but yielded only staphylococci and diphtheroid bacilli, probably due to skin-contamination. Dr. O. Heath reported that portions of the citrated blood, diluted in broth, were incubated aërobically and anaërobically. "After three days' incubation, growth was discovered in two bottles out of six, but this consisted only of staphylococci and diphtheroid bacilli. Streptococci were not discovered."

Necropsy and Microscopical Examination.

The spleen, about four times the normal size, was of friable consistence and red colour, containing minute whitish granule-like patches scattered throughout its substance. The liver was enlarged and soft, suggesting "cloudy swelling." The gall-bladder contained gall-stones, but was not obviously inflamed. The kidneys were rather large and their macroscopic appearance suggested "cloudy swelling." The heart was not hypertrophied and was free from endocarditis and valvular disease. The lungs showed hypostatic congestion. Besides an ecchymotic area of the skin over the left front of the abdomen there was also an ecchymotic condition of the mucous membrane of the uterus and part of the large intestine. The ovaries appeared normal. No evidence of tuberculosis was found anywhere. The packet of enlarged lymphatic glands in the left axilla consisted of about eight glands of various sizes up to the size of a pigeon's egg. They were of firm consistence and of white or reddish-white colour, showing macroscopically a few small hæmorrhages and one or two minute necrotic patches in their substance. The majority of the deep lymphatic glands—mediastinal, mesenteric, retroperitoneal—were swollen and contained reddish patches (as if from hæmorrhages) or were altogether reddish (like hæmolymph glands).

Microscopical sections were prepared from a piece of the spleen and from one of the enlarged axillary lymphatic glands. In both these the most important feature was the presence of multiple areas of necrosis. In the spleen some of these necroses were large enough to be visible to the naked eye, and they were doubtless the cause of the granule-like patches noted by macroscopic examination of the spleen at the post-mortem examination. Endothelioid cells and a few scattered multinuclear cells (giant-cells) of the characteristic kind met with in lymphogranulomatosis maligna (Hodgkin's disease) were also present. There were no giant-cell systems or giant-cells of the Langhans or tuberculosis type. Dr. H. M. Turnbull, of the London Hospital, kindly examined the sections and told me that he was convinced that the case was one of Hodgkin's disease—of which, during acute stages, such necrotic changes constitute a characteristic feature.

REMARKS ON LYMPHOGRANULOMATOSIS MALIGNA.

An unusual feature of this case was the occurrence at various periods of hæmorrhages in the skin and mucous membranes, and hæmaturia. O. Naegeli, for instance, in the fourth edition of his "Blutkrankheiten und Blutdiagnostik" (Berlin, 1923, p. 450), under "negative diagnostic signs" of lymphogranulomatosis maligna, emphasizes the usual absence of hæmorrhagic signs or complications.

In acute cases and acute stages of lymphogranulomatosis maligna (Hodgkin's disease) I believe that multiple microscopic or macroscopic necroses are probably always present in the affected lymphatic glands and viscera. In the present case, although the necrosed areas were mostly not obvious to rough naked-eye examination, the total proportion of necrosed tissue must have been very considerable. The pyrexia in such cases, whether of the Pel-Ebstein[1] (recurrent) type or of another (regular or irregular) type, may be due to absorption of necrosed tissue or to there being, as I have suggested, a condition of acute dissemination in the body of the unknown causative agent of the disease, analogous to the various kinds of "bacillæmia," &c. Many years ago, in a paper on "Acute Cases of Hodgkin's Disease," I wrote:[2]—

"The acute dissemination of the disease throughout the body should, I think, be regarded as a 'septicæmia of Hodgkin's disease,' using the word septicæmia in the broadest sense of the term, as it is used in 'gonorrhœal septicæmia,' 'pneumococcal septicæmia,' 'influenzal septicæmia,' and as it might justly be used in referenee to . . . miliary tuberculosis. According to this view the lymphadenomatous nodules in the viscera are, of course, not metastatic growths in the ordinary sense of the term (that is to say, in the sense in which secondary carcinomatous nodules are metastatic), but are due to metastasis or generalization of the exciting cause of the disease; that is to say, of the hypothetical microbe."

In spite of the work of E. Fraenkel and H. Much in Germany, and of C. H. Bunting and J. L. Yates in America, and of others, the microbic agent of lymphogranulomatosis maligna seems not as yet to have been discovered, and is certainly not any variety of the tubercle bacillus. That patients with Hodgkin's disease not very rarely likewise have glandular or pulmonary tuberculosis, and sometimes die with acute miliary disseminated tuberculosis, is universally admitted, but the frequency and the age-incidence of tuberculosis are probably sufficient to account for the association of the two diseases whenever it occurs. Latent tuberculosis of lymphatic glands is undoubtedly very common, and it is possible that Hodgkin's disease may sometimes arouse quiescent and latent glandular tuberculosis to fresh activity. The frequency of latent glandular tuberculosis may likewise account for the occasional positive results of guinea-pig inoculation from supposed pure cases of Hodgkin's disease.

I regard lymphogranulomatosis maligna (Hodgkin's disease) as an infection, of unknown causation, of the lymphatic endothelial or endothelial-reticular tissue, commencing most frequently in the cervical or axiliary glands. But as the disease progresses and the infective agent (whatever it may be) becomes more and more disseminated over the body, the whole lymphatic glandular system may become involved. In fact, since lymphatic radicles with their endothelial elements are present in almost all parts of the body, it is not

[1] F. Parkes Weber, "The Pel-Ebstein Recurrent Pyrexial Type of Hodgkin's Disease (Lymphogranulomatosis Maligna)," *Practitioner*, London, 1917, xcix, pp. 62—71.

[2] F. Parkes Weber, *St. Bartholomew's Hospital Reports*, London (for year 1907), xliii, p. 82.

surprising that typical lymphogranulomatous growth may occasionally be found in organs or parts of the body which are supposed normally to be destitute of lymphatic glandular (lymphadenoid) tissue. In advanced cases lymphogranulomatous nodules in the liver are quite common, and in rare cases the vertebral periosteum may be affected, and lymphogranulomatous growth may take place in the loose fatty and connective tissue in the vertebral canal outside the dura mater.[1]

It seems that in typical fairly chronic and advanced cases of lymphogranulomatosis maligna there are always two processes in progress: (*a*) an acute necrotic process in the affected parts; and (*b*) a reactive process of fibrosis. Two analogous processes are, of course, generally at work in cases of chronic tuberculosis and tertiary syphilitic gummatous disease, notably in tuberculosis of the lungs or lymphatic glands, and in gummatous disease of viscera.[2] The first process (*a*) is apparently to be explained as the direct effect —the expression of the "action"—of the causative agent of the disease or of the toxins which it produces. The second process (*b*) is to be regarded as the indirect effect—the expression of the vital tissue-reaction against the causative agent of the disease and its toxins, and the necrotic tissue-changes which they produce. The lymphogranulomatous process in the affected lymphatic glands may occasionally spread outside the capsule, giving rise to periglandular lymphogranulomatous growth and fibrosis. Sometimes the necrotic process, for instance in the liver, also involves tissue bordering on but actually outside the lymphogranulomatous growths.

In lymphogranulomatosis maligna, as the fibrosis or the necrosis, or both these processes progress in the affected lymphadenoid tissues, the lymphocytes in the circulating blood tend to diminish in number, though sometimes the progressive glandular fibrosis seems to be accompanied by attempts at compensatory regenerative hyperplasia of lymphadenoid tissue. As in most infective diseases, so also in lymphogranulomatosis maligna, there is usually more or less absolute or relative polymorphonuclear leucocytosis, but in advanced cases, especially cases with decided splenomegaly, there is often leucopenia, as, for instance, there was in the present case. In this connexion it should also be noted that in lymphogranulomatosis maligna the bone-marrow is often more or less involved, and probably sometimes more extensively than is commonly supposed. Dr. H. M. Turnbull kindly informs me that at the necropsy on one case (a boy, aged 14 years) he found very numerous pearly, yellowish-white, pin-head-sized nodules throughout the marrow of the humerus and femur. A few nodules of similar appearance lay in the marrow of the lumbar vertebræ, sternum and ribs. The marrow in all these sites was red, and Dr. Turnbull thought at the time that the bone-marrow involvement might have been the cause of the anæmia present in that case.

[1] F. Parkes Weber, "Paraplegia and Cauda Equina Symptoms in Lymphogranulomatosis Maligna (Hodgkin's Disease)," *Quarterly Journal of Medicine*, Oxford, 1923, xvii, pp. 1—5.

[2] Here it might be noted that, as in tuberculosis and gummatous syphilis, so also in cases of lymphogranulomatosis maligna, amyloid or similar changes in the viscera may occur.

Clinical Section.

President—Mr. G. E. Gask, C.M.G., D.S.O., F.R.C.S.

A Case of Progressive Neural Muscular Atrophy.

By Neil Hobhouse, M.D.

Wm. G., male, aged 7½.

Family History.—A paternal uncle had "something of this kind," which seems to have been a progressive paralysis involving the face. No clear account of it can be obtained.

There have been six other children; two died (one from diarrhœa, the other from osteo-myelitis). All others normal.

Previous History.—Hitherto healthy; no illness except measles in 1921. His mother is positive that he has never had any paralysis, and that he has never been unable to walk, even for one day.

Present Symptoms.—Some time during last winter, his mother noticed that he began to walk on the left toe and to drag the left leg. This gait has gradually become more marked; some months later she could see that the left thigh was thinner than the right. Last autumn she thought that his speech was becoming different and less distinct.

Physical Signs.—Cranial nerves inconstant, and atypical nystagmus on looking to the left. Articulation somewhat indistinct, but no definite weakness of any of the component movements can be made out. All functions of the arm normal. Abdominal reflexes all present; once or twice I have obtained them more easily on the right than on the left. Obvious wasting of the thighs, especially on the left. The right thigh and calf are $1\frac{7}{8}$ and $\frac{1}{2}$ in. respectively larger than the left. There is $\frac{3}{4}$ in. true shortening in the left leg. There is a slight degree of talipes equinus and pes cavus (left) and the foot cannot be brought to the right angle without some force. The knee-jerk and ankle-jerk are not obtainable on the left side. On the right side a faint jerk is obtainable with difficulty. Plantars flexor. Wasting most marked in quadriceps group and is also seen in the anterior tibial group. It is remarkably diffuse and no fibrillary twitches have been seen. There is slight shortening of the left gastrocnemius, but no spasm.

I have not been able to find any evidence of disturbance of afferent fibres. Patient very intelligent and unusually easy to test, and all sensory functions seem quite normal. No inco-ordination or ataxia. The electrical reactions of the muscles show no changes. Wassermann reaction negative. X-ray examination shows reduction in size of whole left femur, but no evidence of bony disease.

Diagnosis.—The only conditions which this case in any way suggests appear to me to be progressive neural muscular atrophy, Friedreich's disease, and an

old polio-myelitis. The points in favour of Friedreich's disease are the nystagmus, the changes in the feet, and above all, the absence of change in the electrical reactions of the muscles. Those against it are the muscular wasting, the absence of ataxia and inco-ordination, and of any lesion of the pyramidal tract. It does not seem justifiable to make this diagnosis in the absence of any evidence of disease of the cord.

The whole history is opposed to the diagnosis of polio-myelitis; there has never been any illness or paralysis, the whole course has been insidious and progressive, and it seems that the right leg is being affected later than the left. Most likely it is a case of progressive neural muscular atrophy of a somewhat unusual distribution, and I should expect to see changes in response to electricity develop before long. I cannot find any record of a case of this condition showing diminished growth of bone, but apparently there is no reason why this should not occur.

Strümpell has referred to a type of case which he regards as a combination of Friedreich's disease and the peroneal form of progressive muscular atrophy, and possibly this case of mine should be placed in this group.

Case of Adiposis Dolorosa.

By Bernard Myers, C.M.G., M.D.

The patient, Mrs. H., has been twice previously shown before this Section, the last occasion being nearly a year ago when the members expressed a desire to see her again during the present session.[1]

She is now aged 46, and weighs 17 st. 12½ lb. Her weight when she first came to the Royal Waterloo Hospital was 19 st. 7 lb., so that she has lost about 23 lb. in about two years. As a matter of fact most of this weight was lost soon after the pituitary treatment was commenced; there has not been much change during the last six to nine months.

The main point at present is that she has enjoyed excellent health during the last year, throughout which she has continued to take pituitary whole gland 3 gr. daily. She looks and feels extremely well, is full of vigour and has not felt any of the great weakness or other symptoms which were present before treatment. The bowels are open daily, the appetite is good, she sleeps well, and her periods are regular and normal. It will be remembered that previous to treatment the periods were very scanty, greenish in colour and irregular.

Most of the painful swellings have disappeared from the arms and legs, but there is still one moderately extensive area on the inner side of the left calf. However, this is smaller and less painful than those formerly present. It has been noted that in the formation of a painful lump the first appearance observed is redness of the skin over the area which is to become affected; then some tenderness manifests itself and this is followed by a lump which is easily palpable and painful, more especially if handled. The administration of pituitary gland appears to exercise a curative effect on these painful lumps.

An attempt was made last September to alter the sugar tolerance, which showed a pituitary curve, and for this purpose 9 gr. of pituitary whole gland were given daily for several weeks; but the result obtained by Dr. H. E. Archer,

[1] *Proceedings*, 1922-23, xvi (Clin. Sect.), p. 11.

of the Pathological Chemistry Department at St. Bartholomew's Hospital, showed no great difference from that which he previously obtained. The figures were the following:—

Specimen of blood	before taking glucose	...	blood sugar	=	0·095	per cent.
,,	,,	½ hour after taking 50 grm. glucose	,,	,,	=	0·14	,,
,,	,,	½	,,	,,	,,	,,	,,	,,	=	0·29	,,
,,	,,	1	,,	,,	,,	,,	,,	,,	,,	=	0·215	,,
,,	,,	1½	,,	,,	,,	,,	,,	,,	,,	=	0·19	,,
,,	,,	2	,,	,,	,,	,,	,,	,,	,,	=	0·15	,,

The curve has still the same shape as on the two previous occasions but is higher still, as on April 25, 1923, it was 0·073 per cent. before the glucose was taken and 0·211 and 0·127, half an hour and one and a half hours respectively after the patient had taken it.

Upon the last occasion the urine before test showed sugar to be absent, but after test there was a faint trace present, while on the previous investigation no trace of sugar was present in the urine either before or after.

Again, it has been obvious that she feels best when taking 3 gr. of pituitary whole gland daily, the larger amounts tending to upset her generally and causing headache. When given 1 gr. of the anterior body of the pituitary t.d.s. for two separate weeks she found that she did not feel so fit and strong during these periods, but with 1 gr. t.d.s. of the posterior body of the gland for two weeks she stated that she felt absolutely strong and well in every way, and indeed, quite as well as when taking the whole gland.

An Unusual Condition of One Eye in a Case of Exophthalmic Goitre.

By Bernard Myers, C.M.G., M.D.

H. H., aged 42, housewife, complained at the Royal Waterloo Hospital on November 17, 1923, of dyspnœa on exertion and of fluttering in the chest.

She stated that twelve years ago she noticed gradually increasing prominence of both eyeballs. The proptosis took six months to reach its maximum. It appears that the exophthalmos persisted until three years ago, when she received a severe blow on the right side of the nose. The effect was such that the right eye immediately lost its prominence and recovered an apparently normal aspect; there remained therefore a unilateral exophthalmos of the left eye.

During the last six months she has been feeling rather depressed. Upon the slightest exertion she suffers from flushing.

On examination: Patient thin, rather highly strung; left eye very prominent, right in nearly normal position. Skin moist and clammy; pulse 107, tension moderate, the rate subsequently found to vary between 90 and 100. Some fine tremors noted in fingers of either hand. All reflexes brisk and equal. Thyroid gland slightly enlarged, some pulsation felt in it. No complaint of dyspnœa on exertion. Appetite fairly good; bowels generally opened daily. From time to time she suffered from attacks of diarrhœa, which were not infrequently followed by constipation. No appreciable disease was noted in the chest or abdomen.

Upon examination of the proptosed eye, Von Graefe's, Stellwag's and Joffroy's signs were all shown to be present; the right eye did not show either of the two former signs.

During the last month the right eye has become a little more prominent, and it would appear as if it would shortly become proptosed again. Mr. Reginald Bickerton kindly examined the eyes and reported that the right vision = $\frac{6}{5}$ and the left vision = $\frac{6}{6}$, the vision of each eye being slightly improved by a small − cylinder. She read J1 with + 1 sphere and the cylinder. Both eyes showed exophthalmos, the left being the more prominent. In each eye there is some congestion of the conjunctiva of the eyeball and lids. All extra-ocular muscles show normal movements. The pupils were equal and reacted normally. The left disc and fundus were quite normal. The right disc and fundus were in a similar condition, except for a circumscribed patch of retino-choroiditis situated downwards and inwards from the disc and some three to four times its size, heavily pigmented. She states that when she was 18 years old the sight in her right eye became very dim, and she was under treatment for this for some weeks. This might have been an isolated tubercle, but it is now obsolescent.

I presume that this case would be regarded as one of exophthalmic goitre although the pulse is not as frequent as might be expected, nor are tremors obvious at present. Still, the exophthalmos, slight enlargement of the thyroid gland, her rather excitable condition, together with the flushings and occasional attacks of diarrhœa, appear to leave no doubt as to the diagnosis. It would be interesting to ascertain whether her basal metabolism was increased.

The sudden disappearance of the right exophthalmos after a blow, and its gradual reappearance three years later is interesting. As no abnormal condition of the cavernous sinus nor a tumour was in any way suspected it would appear that the blow caused sufficient inhibition of the action of the muscle fibres of Müller to lose their contraction and that the eye sank back. At present the muscle fibres are apparently contracting again. Mr. Bickerton states that contusions sometimes produce this effect and that a blow on the eyeball paralyses the iris muscle, the pupil remaining sometimes irregular and always semidilated.

I am indebted to Dr. Lauder, casualty officer to the hospital, for some of the notes above recorded.

A Case of Acquired Acholuric Jaundice.

By C. F. T. East, M.B.

Patient, a female, aged 40, married; no children.

She was first admitted to King's College Hospital, in May, 1919, complaining of jaundice. She said that she had first noticed the yellowness in October, 1918, and had sought treatment for it. Since that time she had had the jaundice more or less continuously, but it had been variable. She had also had some attacks of pain in the right side of the abdomen. Her ankles had been swollen at times and she had been rather short of breath. There was nothing worthy of note in the family or past history.

At this time she appeared rather yellow and there was a distinct yellow tinge in the conjunctivæ. She had a slightly raised temperature in the evenings. There was a hæmic murmur at the apex of the heart. The gums and palate were pale; the spleen was easily felt but the liver did not appear to be enlarged. She was rather tender in the gall-bladder region. Urine rather dark but otherwise normal in appearance. Fæces normal in colour.

A blood count showed the following changes: Red blood cells, 1,320,000 per c.mm.; hæmoglobin, 30 per cent.; leucocytes, 16,000; colour index, 1·15.

There was no abnormality in the differential count. 1860 normoblasts were seen per cubic millimetre, and there was marked anisocytosis.

Diagnosis: Pernicious anæmia; patient left hospital after a month's treatment with arsenic, feeling rather better.

In July, 1920, she was again admitted into hospital. During the interval she had felt better, but two months before readmission she had noticed that the jaundice had increased. Lately she had had attacks of retching accompanied by severe attacks of pain in the right side. She presented the same general appearance as before. The spleen, however, was much larger, reaching as far as the umbilicus and 2 in. below that level. The jaundice was observed to vary. When it was more intense, bile pigments, which did not usually appear in the urine, were present, and at all times urobilin was present in considerable quantities. The blood plasma showed an increase of bile pigments, the bilirubin index being 82 (normal 1 to 4).

Several blood counts (fig. 1) were made and they yielded the following results:

On July 2, 1920: Red blood cells, 1,740,000: hæmoglobin, 32 per cent.; leucocytes, 13,000; colour index, 0·9; polymorphs, 75 per cent.; normoblasts, 634 per c.mm.; lymphocytes, 22·5 per cent.; anisocytosis was present; eosinophils, 1·6 per cent.; hyalines, 0·3 per cent.; mast cells, 0·4 per cent.

On August 20, 1920, the following count was reported: Red blood cells, 2,132,000; hæmoglobin, 52 per cent.; colour index, 1·27; and on September 2, 1920, the following: red blood cells, 2,600,000; hæmoglobin, 52 per cent.; colour index, 1·0.

Two months later a count showed (November 11, 1920): red blood cells, 1,664,000; leucocytes, 10,000; hæmoglobin, 33 per cent.; colour index, 1·0; polymorphs, 80 per cent.; lymphocytes, 18·4 per cent.; eosinophils, 0·4 per cent.; mast cells, 0·4 per cent.; hyalines, 0·8 per cent.

Anisocytosis and poikilocytosis were present, and there was also polychromatophilia and punctate basophilia.

Patient was in hospital nine weeks and during this time she had several attacks of abdominal pain which were associated with marked jaundice and vomiting. At these times bile appeared in the urine and the liver became large and tender. She was treated with injections of collosal arsenic and this treatment was continued until February, 1921. She was then readmitted to hospital. Her general condition was unchanged, and she said that she felt better, and had had no attacks of pain. The spleen came down to the level of the umbilicus and the liver reached the same level as before. The urine contained urobilinogen but no bile pigments. Wassermann reaction positive.

The fragility of the red cells was estimated and it was found that hæmolysis began in a concentration of saline which equalled 0·79 per cent. The normal level for the beginning of hæmolysis is about 0·41 per cent. The bilirubin index was 33 (normal, 1-4).

A blood count showed: Red blood cells, 2,088,000 per c.mm.; hæmoglobin, 48 per cent.; colour index, 1·15; polymorphs, 82 per cent.; lymphocytes, 17·2 per cent.; hyalines, 0·4 per cent. One normoblast and one megaloblast were seen. Anisocytosis and poikilocytosis were present.

In January, 1922, she was again admitted with increase of jaundice and further attacks of abdominal pain. Her general appearance was much as before. There was marked enlargement of the spleen. The urine contained urobilinogen, the blood plasma showed a bilirubin index of 24. Several estimations of the red blood corpuscle fragility were made and these showed that hæmolysis began at about 0·8 per cent. saline. It was also found that the

end-point was much lower than normal, hæmolysis not being complete until 0·2 per cent. saline and under. Blood counts showed—

January 24, 1922: Red blood cells, 1,752,000 per c.mm.; hæmoglobin, 42 per cent.; leucocytes, 7,300 per c.mm.; colour index, 0·84.

The differential count was normal. No nucleated red cells were seen, and anisocytosis and poikilocytosis were present.

She left hospital in the middle of March feeling better. During this time she was put on a course of cholesterol by mouth, and this treatment was kept up till the middle of May. She had only one attack of pain during this time.

When next seen again in December, 1923, she said she felt very much better. Her pain had not returned for some time and she was not short of breath. A blood count showed as follows (and one may well understand the improvement which she felt): Red blood cells, 4,470,000 per c.mm.; leucocytes, 3,200; hæmoglobin, 80 per cent.; colour index, 0·9.

Normal differential count. No nucleated red cells seen. Estimation of the corpuscle fragility showed that hæmolysis began at 0·48 per cent. saline and was complete at 0·38 per cent. saline.

The result of van den Bergh's reaction was as follows: (*a*) Direct reaction negative; (*b*) biphasic reaction negative; (*c*) indirect reaction 2 units, indicating that a slight degree of hæmolysis was present.

The liver was not enlarged and the spleen came as far as about 2 in. below the costal margin. Altogether she looked very well indeed. She had practically returned to a normal state of health. At first the diagnosis of pernicious anæmia was well supported by the blood count, although at that time the absence of the leucopænia usually found in pernicious anæmia was a little abnormal. Later, the great size of the spleen, and the recurring attacks of jaundice with the presence at these times of bile pigments in the urine, made the diagnosis uncertain, but finally the demonstration of the excessive fragility of the red cells clinched the diagnosis of acholuric jaundice. The clinical features are such as appear in this disease, namely, the continual jaundice without bile in the urine; the attacks of pain in the right side of the abdomen due to biliary sand, accompanied by increase of the jaundice due to the obstruction, and the appearance of bile in the urine. One may also notice the enlargement of the liver at these times and the constant great enlargement of the spleen.

The fragility of the red cells is a very characteristic and interesting feature. In this case it was particularly marked. The form of the hæmolysis curves in the chart very kindly prepared by Dr. E. ff. Creed may be noticed (fig. 2). It may also be seen how the hæmolysis begins very gradually and then rises rapidly in amount as the concentration of the saline is decreased, and finally tails off, so that at very low concentrations there are still some cells not hæmolysed. This shows that besides the large proportion of abnormally fragile cells, there are also some cells which are abnormally resistant.

There is the excess of bilirubin in the blood plasma derived from the excessive destruction of the abnormally fragile red cells. The grave anæmia of Addisonian type is very interesting. With the exception of the rather high leucocyte count the blood count would very well pass for that seen in pernicious anæmia. It seems possible that the anæmia may have been due to the excessive destruction of abnormally fragile red cells, though it cannot be said whether there was an abnormal hæmolytic factor present as well; or whether the fragile cells fell an abnormally easy prey to a normal process of destruction. There was a vigorous attempt on the part of the bone marrow

toreplace the loss as shown by the abnormal red cells seen. It is interesting to note that there were abnormally resistant red cells present as is the case in pernicious anæmia.

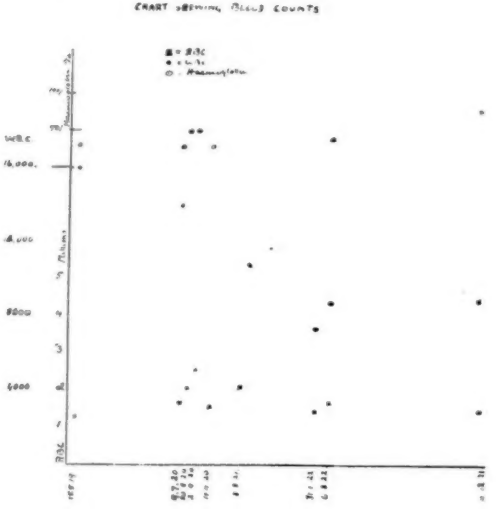


FIG. 1.

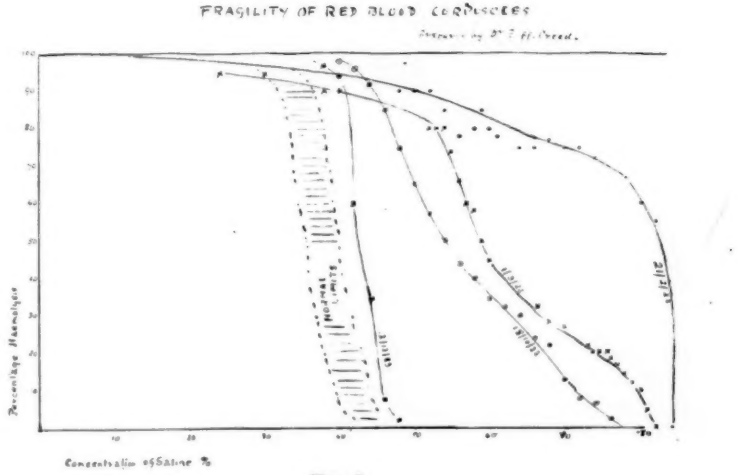


FIG. 2.

Now the influence that caused the output of these excessively fragile cells is practically removed. There is only a little abnormal destruction of red cells going on and their fragility is almost normal. The size of the spleen

has decreased and the anæmia has been compensated. At one time the question of the splenectomy was considered, but the patient was thought to be too ill to undergo this. Now she has returned to her almost normal condition; one cannot claim that treatment has effected this change. It remains to be seen whether this is a real recovery or merely a remission. There is still a trace of the old condition, and the degree of leucopænia is not normal. Time will show whether the toxic influence which brought about the condition has disappeared entirely. The charts show the changes in the blood counts and the fragility of the red cells during the illness.

I mu t thank Dr. Douglas Firth for allowing me to show this case and Dr. E. ff. Creed for his chart of the fragility observations made by him.

Dr. F. PARKES WEBER said that in many respects the case resembled a remarkable one shown by him on May 29, 1923, at the Section of Medicine, under the heading, "A Case of Acquired Chronic Hæmolytic (Acholuric) Jaundice, seen fifteen years ago, with a Blood Picture at that time resembling one of Pernicious Anæmia."[1] The patient in that case was a woman, aged 53 years; she afterwards recovered rapidly from her relapse under treatment by rest, and iron and arsenic. On leaving the hospital, in September, 1923, the erythrocytes had risen to 4,704,000 to the cubic millimetre of blood, and the hæmoglobin was 62 per cent. Her blood-serum gave a completely negative direct and indirect Hijmans van den Bergh reaction for bilirubin; there was no icteric tinge in the skin or sclerotics; the urine was free from excess of urobilin and urobilinogen; and the spleen was much less enlarged. There was no longer any excessive fragility of the red cells to hæmolysis, as tested by adding drops of the whole blood to graduated hypotonic sodium chloride solutions. This disappearance of the diminished resistance of the erythrocytes towards hæmolysis was likewise a feature in the present case. Owing to the history in the present case of there having been bilirubin at one time in the urine, together with great jaundice and severe pain across the upper part of the abdomen, Dr. Weber would suggest that at that time there was some actual obstructive jaundice (in addition to the hæmolytic jaundice) due to inspissated bile or actual cholelithiasis. Cholelithiasis had been proved to be present in some cases of hæmolytic jaundice as a disagreeable complication.

Aneurysm of the Ascending Aorta treated by Colt's Wisp.

By CECIL P. G. WAKELEY, F.R.C.S.

W. A., AGED 31, by occupation a blacksmith, first noticed a swelling in the front of his chest, in March, 1923, which gradually increased in size. The patient did not consult a doctor, although he suffered from a dull ache in his chest. On November 26, 1923, the pain in the chest became much worse and he was admitted to King's College Hospital.

On examination a swelling about the size of a large orange was seen on the anterior wall of the chest, slightly to the right of the middle line. On palpation the swelling felt firm and elastic; it pulsated and the skin over it was tense and red. The right border of the sternum and the second, third and fourth ribs were eroded. A skiagram demonstrated a large fusiform aneurysm of the ascending aorta. Wassermann reaction positive. The only form of treatment for such a large aneurysm was Colt's method.

On November 30, 1923, a large Colt's wisp was introduced deep into the aneurysm. The swelling immediately became solid, and the pulsation was changed from an expansile to a thrusting type. The wound healed per primam. It was thought that probably a second wisp might have to be introduced at a later date owing to the large size of the aneurysm.

[1] F. Parkes Weber, *Proc. Roy. Soc. Med.*, 1923, xvi (Sect. of Med.), p. 73.

A Case of Osteitis of the Lower Jaw.

By GRANT MASSIE, M.B., F.R.C.S.

PATIENT, a girl, aged 18, was admitted for a painful swelling of the mandible, which began at the age of 7, and had gradually spread from the right to the left side of the bone. This swelling was originally supposed to be a dental abscess; and in 1914 two operations were performed, bone being removed for examination on each occasion.

The pathologist who examined the first specimen, was of the opinion that it was malignant and reported that there were small columns of cells invading the bony trabeculæ. It appears that it was considered to be most probably an epithelial odontome. The second specimen, however, removed eight months later failed to confirm these findings and only a mass of bone and fibrous tissue was demonstrated.

The swelling remained for a long time localized to the right side and only during the last eight months has the left side of the bone been affected.

The jaw is almost uniformily involved along the alveolar margin, the remaining part being apparently normal. There is no pyorrhœa, although one tooth is carious. There is no evidence of syphilis, and the Wassermann test is negative.

A radiogram shows diffuse sclerosing osteitis, with no evidence of sequestrum formation. Dental films show general periodontal thickening.

Aortic Stenosis and Malignant Disease of Lung.

By DOUGLAS FIRTH, M.D.

PATIENT, a male, aged 56, complaining of shortness of breath for last month.

Heart considerably enlarged. No X-ray evidence of displacement. Systolic thrill and systolic murmur at base of heart, also aortic regurgitant murmur, and (?) secondary mitral regurgitant murmur. No history of heart trouble.

Left Lung.—Signs of emphysema and dry bronchial catarrh.

Right Side of Chest.—Movement defective; dullness everywhere except at the apex. Tactile vocal fremitus increased. No bronchial breathing, only defective entry of air, amounting at the base to complete suppression.

Chest Measurements.—Left, 16 in.; right, 16½ in.

Blood Wassermann negative. *Weight*, 7 st. 9 lb.

Pseudo-elephantiasis of Eyelid.

By W. R. M. TURTLE, M.B., B.S.

PATIENT, a female, L. L. B., aged 23.

History.—Sore on right cheek two years ago and about same time sore right side of scalp; attributed by patient to blows received from the man with whom she was living. The sores have healed and fresh sores have appeared in the neighbourhood of the old ones. A year ago the right lower eyelid began

to swell and it has gradually increased in size. Patient had a baby born dead five and a half years ago and a baby born alive four and a half years ago, which died at the age of 3 years from bronchitis.

Present Condition.—Ulceration and scarring on right side of forehead, face and ear; and there is pseudo-elephantiasis of right lower eyelid forming a pendulous tumour of the size shown in the photograph below.



Wassermann reaction + + +. She has had 15 gr. pot. iodidi t.d.s. for a month. On November 17, 0·3 gm. of novarsenobillon was injected. The swelling has been punctured by hypodermic needles, but during the last month it has not appreciably altered in size.

Clinical Section.

President—Mr. G. E. Gask, C.M.G., D.S.O., F.R.C.S.

A Case for Diagnosis.

By George Arthur, M.B.

(Introduced by Dr. Stanley Melville).

A. E. H., clerk, male, aged 22. Admitted to King Edward Memorial Hospital, Ealing, on November 11, 1922, in a state of collapse, pale, with dilated pupils, and an imperceptible pulse. He had simply collapsed while running after the ball at football, there being absolutely no question of his having been knocked over or injured in any way.

Later, the pulse appeared at the right wrist, and while it could be felt in both carotids, it remained imperceptible in the left subclavian, brachial and radial arteries.

The left chest was hyper-resonant, the cardiac dullness being absent, and the breath sounds on that side were almost inaudible, and on the right side harsh. Heart sounds were heard only over the second left costal cartilage. No apex beat. Pain in chest for three or four days after admission.

Previous History.—Always well up to December 26, 1921. On that date he caught a chill, and was away from work for a fortnight. A week after his return to work, while sitting at his desk, he was seized suddenly with shooting pains in his back and chest, and could scarcely walk, and was at home again for four or five weeks with intervals of pain and general weakness. These attacks recurred at intervals, but gradually decreased in severity, though he got out of breath easily.

For some weeks before admission to hospital he had played football regularly, but had noticed his breath getting shorter again. He has never been abroad.

November 17, 1922: Breath sounds now heard on both sides of chest. Faint friction sound towards left apex.

November 15, 1922: Skiagram shows a large, well-defined rounded opacity in left upper chest. (Skiagrams shown by Dr. Stanley Melville.)

November 17, 1922: *Wassermann reaction*, completely negative.

Blood count: Small mononuclears, 21 per cent.; large mononuclears, 12 per cent.; polymorphs, 52·3 per cent.; eosinophils, 14·7 per cent.

Blood count on December 21, 1922: Small mononuclears, 32·25 per cent.; large mononuclears, 8·5 per cent.; polymorphs, 54·75 per cent.; eosinophils, 3·5 per cent.; mast cells, 1·0 per cent. Hæmoglobin, 95 per cent.; red cells, 5,300,000 per c.mm.; white cells, 10,800 per c.mm.; colour index, 0·9.

Patient got up on November 27, 1922, still rather short of breath, and was discharged on December 27, 1922, able to get about quietly without any great distress.

Condition on discharge: No displacement of heart. Percussion note and breath sounds normal in front. At left upper chest behind, impaired percussion note and diminished breath sounds.

Subsequent history, July 30, 1923: Feeble pulse at left wrist. Breath sounds at left chest behind, feebler than on right. Skiagram shows that shadow in chest has disappeared, except for small area below inner end of left clavicle.

August 9, 1923: While sitting at his desk on the 7th, he was taken with severe pain in right side of chest. This gradually passed off, but returns on slight exertion. Upper left lung behind relatively dull to percussion. Breath sounds on right feebler than on left. Skiagram taken on 8th showed pneumothorax at right base.

August 16, 1923: Increased vocal resonance at left base. Breath sounds at right base very harsh. Weight, 9 st. $6\frac{3}{4}$ lb.

September 20, 1923: Weight, 9 st. 8 lb.

October 18, 1923: Weight, 9 st. $8\frac{1}{4}$ lb.

November 22, 1923: Weight, 9 st. 8 lb.

January 17, 1924: Weight, 9 st. $10\frac{1}{4}$ lb.

February 7, 1924: Weight, 9 st. $10\frac{1}{4}$ lb.

Patient is still upset easily on exertion. Thus on November 8, after hurrying here, the pulse was 108, and the superficial veins above and below clavicles turgid, but these went down during examination and the pulse dropped to 96. He is scoliotic, and the chest moves less on left side than on right. The respiratory murmur is heard at left apex in front, and the expiratory sound is prolonged. Behind, breath sounds are feebler on left side. Pulse is much smaller on left side.

DISCUSSION.

Dr. F. PARKES WEBER thought that the most reasonable explanation was afforded by the following supposition: The young man had extensive pleuritic adhesions on both sides, probably the result of an old attack of pleural miliary tuberculosis which had undergone (as such attacks often did) spontaneous cure. The rupture of an adhesion near the apex of the left lung (due to muscular effort) gave rise to a circumscribed hæmothorax (shown as a large round shadow in the Röntgen skiagram) which soon underwent spontaneous absorption, though apparently in connexion with it the left subclavian artery remained somewhat cicatricially involved. Afterwards, on the right side, the rupture of a subpleural emphysema bulla, which was adherent to the parietal pleura (resulting from a healed miliary tuberculous lesion) gave rise to a localized (circumscribed) so-called "spontaneous" pneumothorax on the right side. In a case published long ago by Dr. Newton Pitt the rupture of such a subpleural emphysema bulla (Dr. Weber supposed it was adherent to the parietal pleura) gave rise to a hæmopneumothorax.[1] The whole subject was intimately connected with that of so-called idiopathic spontaneous pneumothorax, and pneumothorax from slight efforts, in apparently healthy persons, the copious bibliography of which Dr. Weber had added as an appendix to his Mitchell Lecture on "The Relations of Tuberculosis to General Bodily Conditions and to Other Diseases" (London, 1921). The prognosis for most such cases was very good, but pneumothorax of similar nature (due to old healed tuberculous pleural lesions) undoubtedly occasionally occurred in patients suffering from active pulmonary tuberculosis—some of the latter patients, though they might

[1] G. Newton Pitt, "Case of a Rapidly Fatal Hæmopneumothorax," *Trans. Clin. Soc. London*, 1900, xxxiii, p. 95.

rapidly recover from their pneumothorax, afterwards dying from pulmonary tuberculosis.

Dr. G. NEWTON PITT said that when he saw this patient about two or three weeks after the onset of his attack, the collapse had passed off, but he was still dyspnœic on the slightest exertion. The left chest was hyper-resonant, with X-rays more transparent than on the right, breath sounds absent and the heart was displaced to the right. The condition, he (Dr. Newton Pitt) considered, was due to compression of the bronchus with an overdistended lung and not to a pneumothorax. He had previously drawn attention to the over-distension of the lung which resulted from compression without occlusion of a bronchus and the probability of its being diagnosed as a pneumothorax. The pulse was absent in the left subclavian and radial, but present in the carotid. The large curious oval shadow to the left of the sternum was sharply defined and the possibilities of its being due to an aneurysm, a hydatid, an abscess, a growth and a hæmorrhage were discussed. There was an eosinophilia of 15 per cent. It was decided that the shape almost excluded an aneurysm and a hydatid, but the diagnosis remained obscure. There was no evidence to show what was the condition of the pulse previously. Gradually the patient's condition improved and the X-rays showed the disappearance of the oval opacity. Recently he had developed a partial pneumothorax at the right base and the X-rays also showed a mottled condition of the lung. This he (Dr. Newton Pitt) considered to indicate a tuberculous process.

The most probable explanation of the oval opacity on the left side, which cleared up, would appear to be a hæmorrhage in the mediastinum which was encysted and did not break into the left pleura.

Dr. ARTHUR (in reply) said that while the patient was in hospital the questions of aneurysm, new growth, hydatid, abscess, and hæmorrhage had been considered, and a hydatid was thought to be the most likely explanation. The presence of an eosinophilia of 14·7 per cent. seemed to favour this.

The subsequent history, however, i.e., the clearing up the X-ray opacity, the general improvement in patient's condition, and the later occurrence of a definite pneumothorax on the opposite side, pointed to the condition having been one of mediastinal hæmorrhage.

An Acquired Heart Lesion simulating Patent Ductus Arteriosus.

By C. F. T. EAST, M.B.

PATIENT, a girl, E G., aged 10, admitted into King's College Hospital in February, 1920, under Dr. Still. She then had acute rheumatism, nodules being found on the knuckles, and the following murmurs being heard in the heart :—

A loud blowing systolic murmur, heard with maximum intensity at the apex, conducted in all directions, but best outwards and backwards. An indefinite diastolic bruit was heard in the pulmonary area. During the summer a presystolic murmur developed at the apex.

In the autumn of 1920 the lesions appeared to be mitral regurgitation, slight stenosis and slight aortic regurgitation.

In February, 1922, the patient was again in hospital with further attacks of rheumatism. Mitral regurgitation and aortic regurgitation were diagnosed, with roughening of the aortic valves, causing a systolic bruit.

In addition to this a new element had appeared. In the pulmonary area, at the middle end of the second left space, a loud blowing systolic murmur was

heard, which was continued into diastole. This varied in intensity with respiration, but not in character. There was no thrill. The resonance over this area was rather impaired.

In January, 1924, the findings were the following:—

Heart much enlarged. At the apex there is a systolic murmur of mitral regurgitation. Double aortic murmurs are heard in the aortic area, the diastolic being clearest to the left of the sternum. In the pulmonary area, where the resonance is rather impaired, a loud whizzing systolic murmur is heard, which is continued on into the diastole. It is not so marked on inspiration. There is an indefinite thrill in this area.

The point of interest in the case is this last murmur, which has the characters of a murmur caused by a patent ductus, but which is definitely an acquired lesion. A case of patent ductus arteriosus is shown for comparison.

The blood count is normal.

The electrocardiogram shows inversion of the T-wave in leads 2 and 3.

The X-ray shows some widening of the median shadow at the base of the heart, but plates subsequently taken showed no abnormality of the aorta.

I am indebted to Dr. Still for allowing me to show this case.

Congenital Pulmonary Stenosis and Patent Ductus Arteriosus associated with Mitral Regurgitation.

By C. F. T. East, M.B.

Patient, L. D., a girl aged 11. About fifteen months ago this child became easily out of breath on exertion. She has had vague pains about the legs and arms. Actually, she was brought up to hospital to Dr. Wiltshire's out-patient department by her mother for attacks of migraine, which were preceded by a mist before the left eye and accompanied by nausea and retching. She had rheumatic fever when she was 9 years old. Heart enlarged, the area of cardiac dullness extending about 5 in. from the middle line to the left and down as far as the sixth space. There is no thrill on palpation. At the apex there is a systolic murmur conducted out to the axilla. In the pulmonary area there is a loud systolic murmur which is conducted upwards. At the left of the sternum in the second space there is a blowing murmur which is systolic in time but continued on into diastole and is in fact almost persistent throughout the cardiac cycle. If she draws a deep breath and holds it, the diastolic part of the murmur is not heard.

The electrocardiogram and blood count are normal. There is no cyanosis and no finger-clubbing. It is thought that the congenital lesions of slight pulmonary stenosis and patent ductus arteriosus are present, associated with an acquired mitral regurgitation due to rheumatic endocarditis.

The case is shown for comparison with the preceding case, E. G., in whom a murmur is audible just above the pulmonary area beginning at systole and prolonged through diastole. This murmur has come into evidence during the last two years.

My thanks are due to Dr. Wiltshire under whose care this child was an out-patient.

Red-headed Albinos.

By DOUGLAS FIRTH, M.D.

THE father and mother of these children are first cousins. Out of a family of five children, one girl and four boys, the girl and two boys are albinos, and in each the hair of the head is red: the youngest albino boy is definitely hæmophilic, the other suffers from severe epistaxis. I have obtained no history of albinism or hæmophilia in the family other than this.

The late Dr. Pringle (Allbutt and Rolleston, "System of Medicine," 2nd ed., vol. ix, art. Albinism) states that the hair of albinos may have a yellowish tint, and quotes Folker as reporting a case in which the hair was red.

Dr. H. STANNUS said that this case of albinism, one of three in a family of five children, with red hair, was particularly interesting. These cases were uncommon in this country but on the other hand this type was known to occur among all races. Cases had been recorded from Papua, Malaysia, from among American Indians, and a few were found in the series of albinos recorded by himself from Central Africa. Those who were interested in albinism would of course find an exhaustive survey of the subject in the monograph on albinism from the Department of Applied Statistics, University College, London, by Professor Karl Pearson and his co-workers.

Though it was impossible to dogmatize in the absence of microscopic examination, it might be said that as a rule the bright red type of hair contained no granular pigment in distinction to some others which had a slight brown shade. After only a hurried survey of the case one would class it as one of complete albinism, that was to say hair without granular pigment, pigmentless skin, red reflex from the eye, &c. In these cases the colour of the hair was due to a diffuse pigment, a lipochrome with no granular pigment.

Sometimes it would be found that the distal portions of the hairs were darker than the more proximal parts; such hair sometimes darkened with age. Red hair was seen in red skinned members of dark skinned races, cases that were classed under xanthism, but it had to be remembered that every degree of variation might be met with, from complete albinism to normal pigmentation, and that the distribution of pigmentation might vary in all directions. Individual red hairs from the scalp of a black haired African showed the same characters as the red hairs of albinos. It was, however, only possible here to allude thus briefly to the clinical side of albinism. On the bio-chemical side there was a vast and interesting field of research still open for investigation. A further point of considerable interest was the presence of a hæmophilic diathesis in the family under discussion, this and albinism being both of course familial affections.

Case of Hepatic Cirrhosis Eight and a Half Years after the Disappearance of Ascites.

By F. PARKES WEBER, M.D.

THE patient, Mrs. M. H., then aged 47, was admitted to hospital under my care on July 15, 1915, with chronic ascites, which had already been tapped twice since the commencement of 1915, when it was first noticed. The liver was hard and greatly enlarged, reaching down to about the umbilical level. The spleen could not be palpated. Otherwise the patient appeared fairly healthy. There seemed to be no disease of the thoracic or of the other abdominal organs. The urine was free from albumin and sugar, and the bloood-serum gave a negative Wassermann reaction. There was no fever. Pulse 80. Respirations 24 per minute. About two years previously she had

ad an attack of hæmatemesis or hæmoptysis. There had never been any jaundice. Seventeen years previously she had undergone an operation for hæmorrhoids.

On July 20, 1915, paracentesis abdominis yielded 8,700 c.c. of clear ascitic fluid, of specific gravity 1007. She was tapped again on July 31 and August 13, 1915, and I regarded the case as one of hepatic cirrhosis with chronic ascites—possibly suitable for omentopexy and (later on) peritoneal drainage. However, after the patient left the hospital in August, 1915, no paracentesis abdominis or operative treatment was required, as the ascites gradually disappeared. When I saw her again on November 10, 1915, she was quite free from ascites, though the enlarged liver could be felt reaching down to the umbilical level. The brachial systolic blood-pressure was 165 mm. Hg. The only medicine she had been taking was that for keeping her bowels open (cascara sagrada and Carlsbad salts).

After eight and a half years I have now seen the patient again. There has been no return of the ascites. There has been no jaundice. Her liver now (February, 1924) forms a large, hard, rounded, somewhat irregularly-shaped, swelling, bulging forward in the upper part of the abdomen (more so, I think, than in 1915). I find no evidence of any other visceral disease. She has taken a little alcohol occasionally since 1915. Her general health appears to be at least as good as it was then, although she suffers from flatulent dyspepsia at times, and her remaining teeth are in a very bad state. Her urine is free from albumin and sugar. Her brachial systolic blood-pressure is 160 mm. Hg, pulse 68 to 80. Respiration 20 to 24 per minute. Examination of her gastric contents after a test-breakfast shows: free HCl 15; total acidity 21. Nothing abnormal can be found by Röntgen-ray examination of her stomach.

I regard the case as one of hepatic cirrhosis, probably not connected with alcoholic intemperance, in which the liver is large and has undergone much compensatory (regenerative) nodular hypertrophy. Probably considerable omental adhesions have occurred spontaneously, sufficient to equal the results of a successful omentopexy operation—an operation which in 1915 I actually thought of proposing.

I believe that a certain amount of chronic localized peritonitis—especially about the liver and spleen—is often present in cases of hepatic cirrhosis, and favours the development of peritoneal adhesions and new vascular channels, in the same way that a successful Talma-Morison operation (omentopexy, epiplopexy) does. In December, 1909, at this Section of the Royal Society of Medicine,[1] I suggested that in regard to chronic ascites, and the question of operative treatment beyond simple tapping, cases of hepatic cirrhosis might perhaps be roughly divided into the two following groups:—

(*A*) Patients who for some reason (for instance, the presence of old perihepatitis and perisplenitis and extensive spontaneous omental adhesions) have the collateral venous circulation well established, and do not readily develop ascites, but are, of course, liable to hæmatemesis from dilated œsophageal or gastric veins. The liver is generally decidedly enlarged in this group of cases.

(*B*) Patients with a poor collateral venous circulation, who develop ascites early. The main object of omentopexy and peritoneal drainage should be to convert patients of Class B into patients of Class A. I would further add that

[1] *Proc. Roy. Soc. Med.*, (Clin. Sect.) 1909-1910, iii, p. 80.

the chief *natural* collateral venous circulation in cases of hepatic cirrhosis is by the œsophageal veins, and a successful omentopexy operation ought to *assist nature*, by the formation of veins in the artificially produced omental adhesions, thus removing the necessity for the *excessive* dilatation of œsophageal veins, which would be likely later on to give rise to fatal hæmatemesis.

In the present case the patient has spontaneously, I think, become a member of Class A.

For comparison I will shortly allude to a few other cases of hepatic cirrhosis that I have observed in which ascites has disappeared without any operative interference beyond paracentesis abdominis.

My attention was first drawn to the whole subject in question in 1898 by making a post-mortem examination[1] on a man, aged 44, who had been several years previously (in 1892), under treatment by Dr. Gee, at St. Bartholomew's Hospital, London, where the diagnosis of cirrhosis of the liver with ascites was made. When the patient died, in 1898, the necropsy showed that he really had cirrhosis of the liver, but that the paracentesis abdominis at St. Bartholomew's Hospital had been followed by the cure of the ascites and by the formation of extensive peritoneal adhesion. The man had evidently had chronic peritonitis in addition to his hepatic cirrhosis. In favour of the view that the peritoneal effusion, for which the patient was treated in 1892, was of an inflammatory nature was the relatively high specific gravity (1020) of the fluid first drawn off, and the patient's tendency at that time to have fever in the evening. The patient had discharged himself from the hospital and had battled against his ascites, much in the same way as my above-described patient (Mrs. M. H.) did, who when she left the in-patient department, in August, 1915, was determined to get on as long as possible without having her ascites again tapped.

In a woman (J. M. B.), aged 41, admitted under my care in July, 1909, cure of chronic ascites followed repeated paracentesis abdominis and an exploratory laparotomy at which nothing special was found beyond the ascites. The last tapping was on November 11, 1909, when 3,000 c.c. ascitic fluid were removed. On November 15, 1909, when she left the hospital, there was still moderate ascites. After that no further tapping was required, and when I saw her again on January 30, 1911, she looked remarkably well and there was no ascites. The liver could not be felt, but the spleen seemed to be enlarged and fixed by perisplenitic adhesions to the abdominal parietes. Unfortunately she had two hernias, an old umbilical hernia and one that had developed in the laparotomy scar. Probably cardiac weakness had been one element in the causation of the ascites in this case.

As a last instance of the disappearance of ascites in hepatic cirrhosis, I will mention the case of a man (J. W.), aged 47, who was admitted under my care on January 28, 1915, with considerable ascites, moderate jaundice and a history of previous alcoholism and probably also previous syphilis. There had been a suspicion of gall-stone colic not long before admission. His blood-serum gave a strongly positive Wassermann reaction for syphilis. The ascites disappeared (perhaps partly owing to treatment with potassium iodide). About two and a half years later the patient died of cancer of the tongue, and the necropsy showed typical hob-nail cirrhosis of the liver, a gall-bladder full of gall-stones, extensive peritoneal adhesions rich in blood-vessels, and no ascites. No opera-

[1] I reported the case in the *St. Bartholomew's Hospital Reports*, London, 1898, vol. xxxix, p. 321, under the heading: "Cirrhosis of the Liver—Effect of Peritoneal Adhesion in Arresting the Symptoms of Hepatic Cirrhosis."

tion of the omentopexy class could have produced a better collateral circulation than Nature (almost left to herself) did in that case. It is possible that the inflammation connected with the cholelithiasis may have favoured the development of the vascular channels (including the newly formed vessels) concerned in the collateral circulation.

Case of Patchy Gangrene of the Toes due to Vasomotor Injury.

By P. Jenner Verrall, F.R.C.S.

This case has been previously shown at the Section of Orthopædics; further light on the diagnosis is now sought from this Section.[1]

Patient, a male, aged 23, who in 1918 injured his right leg in an omnibus collision. There was no fracture or skin lesion, but there was great swelling of the limb with immediate paralysis of all muscles below the knee. I first saw him one and a half years ago when the paralysis remained and there were small black gangrenous patches on the toes. Loss of sensation was confined to the toes and part of the adjacent dorsum of the foot and the electrical reactions of the muscles were normal. The whole leg was somewhat blue and cold. I was informed that the patches appeared one and a half years after the injury.

Under treatment the gangrenous patches healed except for discoloured spots but in spite of contrast baths, &c., the paralysis remains and the leg still swells if allowed to hang down. In July last I stripped off the coats of the popliteal artery, but up till now this has had no effect. The artery is normal in size and there is good pulsation in the posterior tibial artery. The possibility of malingering and dermatitis artefacta has been excluded to my satisfaction.

I take this to be a case of vasomotor disturbance with associated functional paralysis. Thrombo-angeitis obliterans is excluded by the normal size of the arteries. The patient is not a Jew and he does not smoke to excess. Burrows (*British Medical Journal*, February 2, 1918) gives an exhaustive description of arterial injuries with which my own experience agrees. Neither he nor I have seen this condition as a result of injury to arteries in the distal limb segment and the anæsthesia here is not of a stocking type. It has been suggested that there is an arterial lesion higher up but I can see no evidence of this. I should be much obliged for suggestions as to diagnosis and treatment.

Dr. F. Parkes Weber remarked on the muscular atrophy in the affected (right) lower extremity, together with the coldness, lividity, weak arterial pulsation, glossy skin, and partial anæsthesia, in the foot. All this, he maintained, could only be explained by supposing that at the time of the accident in 1918 the main artery supplying the limb was gravely injured. Perhaps the internal coat was ruptured, or the internal and middle coats, as happened in a case reported by Sir Anthony Bowlby in the *Transactions of the Pathological Society of London* for 1891 (vol. xlii, p. 79).[2] In

[1] See *Proceedings*, 1923, xvii (Sect. Orth.) p. 13.

[2] For this reference Dr. Weber was indebted to Sir Humphry Rolleston.

Bowlby's case there were other injuries which caused the patient's death, but before death collateral circulation had evidently been partially established in the right upper extremity. At the necropsy it was found that the internal and middle coats of the right subclavian artery had been completely divided (as if by the application of a ligature) and the vascular lumen had been blocked. In the present case, Dr. Weber thought, the collateral circulation had never been sufficient, and the patient's present symptoms were due to that circumstance. He would compare the case to one which he (Dr. Weber) had described in the *American Journal of Medical Sciences* for May, 1894, (vol. cvii, p. 534) about four years after the original injury. Mr. H. Burrows[1] had admirably drawn attention to the anæsthesia following injuries to the main artery of a limb. In his (Dr. Weber's) case the anæsthesia of the foot and lower part of the leg was at first, as in Mr. Burrows' cases, of the "stocking-type" or "glove-type," involving only the distal part of the limb. In hysterical anæsthesia of similar distribution the proximal border of the anæsthetic area was, Dr. Weber believed, more sharply defined than in these cases of organic arterial (ischæmic) origin. When patients with anæsthesia of arterial (ischæmic) origin had partially recovered the anæsthesia was probably in most cases no longer strictly of the "stocking" or "glove" type. It was not so four years after the injury in his (Dr. Weber's) case and it was not so at present in Mr. Verrall's case. There remained the question, of course, whether Mr. Verrall's patient would be able gradually to make more use of the affected limb if he tried to do so and if he got a "job." The patient appeared unfortunately somewhat too contented with his present (workless) condition.

Case of Lymphadenoma.

By J. Paterson Ross, F.R.C.S.

Patient, a male, A. J. H., aged 26, a nursery hand, who has always lived in the country.

History.—In 1917, while in the army, suffered from attacks of retching, leading occasionally to vomiting. These attacks passed off, but he never felt really fit, being easily tired and depressed.

In April, 1923, he consulted his medical man, complaining of a return of nausea and vomiting, loss of weight and night sweats. He was referred to the tuberculosis officer, who could find no conclusive evidence of tuberculosis.

In August, 1923, he became rapidly worse, with pain in the chest, mostly on the right side, cough and shortness of breath. For a few days at a time he would feel slightly better, but then for a fortnight he would be very ill, with vomiting, headache and an exacerbation of the chest symptoms. He noticed this periodicity himself.

In December, 1923, he was admitted to the Royal Chest Hospital, under the care of Dr. L. R. Shore, being still under suspicion for tuberculosis. No evidence of tuberculosis could be found, and Dr. Shore suspected a mediastinal tumour or a mass of glands compressing the bronchi, especially on the right side.

In January, 1924, he was transferred to St. Bartholomew's Hospital in care of the Surgical Professorial Unit, and at that time a gland was found enlarged in the posterior triangle of the neck on the right side. This gland was

[1] Burrows, "Paralysis following Arterial Injuries," *British Medical Journal*, 1918, vol. i, p. 199.

removed, and the section shows the appearances characteristic of lymphadenoma.

Since his admission to the Royal Chest Hospital he has been known to have fever of the Pel-Ebstein type, and with each attack of fever the glands which are palpable become enlarged, and fresh masses appear. He now has glands palpable in both sides of the neck, just above the clavicles, in both axillæ, and small glands in both groins. He has a large liver and palpable spleen, and a mass in the region of the right loin. His blood count is not abnormal, and the Wassermann reaction negative.

The case is shown because of the difficulties in making an early diagnosis —obstruction to the bronchi and gastric disturbances being the only outstanding early features—because of the well-marked Pel-Ebstein febrile phenomena, and because in his work he was subject to the effects of mildew from tomato plants, which floats about like fine dust, and is alleged to cause bronchial catarrh. His tonsils, fauces and pharynx are very healthy looking, but the bronchial irritation may help to explain the fact that the bronchial glands were the first to be attacked in this case. The involvement of other glands appears to be taking place by centrifugal spread from the chest.

Clinical Section.

President—Mr. G. E. Gask, C.M.G., D.S.O., F.R.C.S.

A Case of Diabetes Insipidus with Infantilism.

By C. F. T. East, B.M.

Patient, a male, aged 29. In 1917, while serving in German East Africa, he contracted malaria. After three months with malaria, he had a particularly severe attack with unconsciousness and delirium. About this time the symptoms of diabetes insipidus came on. The onset was sudden and marked with the usual symptoms of severe thirst and polyuria. Shortly after this he noticed that his pubic hair was falling out, and this soon disappeared. The axillary hair also disappeared, and the growth on his face which had previously required shaving every other day, at the most required shaving only once a week.

Before the onset of the illness he had been normal in every way. Now he looks very young for his age. The hair on the face is very scanty, and the eyebrows are distinctly sparse. The hair on the scalp is normal. There is no hair in the axillæ or on the pubes. The testes are infantile in size, but testicular sensation is present. Sexual activity has almost entirely disappeared. He is thin and easily tired, and the skin is rather dry. He suffers from typical diabetes insipidus. An X-ray photograph of the pituitary fossa does not show any abnormality. The Wassermann reaction is negative.

Injections of pituitrin given hypodermically relieve the polyuria. He takes as a rule 1 c.c. before going to bed, and so gets a night's rest undisturbed. This appears to be a case with a lesion of the posterior lobe of the pituitary body, no doubt due to the attack of malaria, which was of the cerebral type. The improvement under pituitrin suggests the likelihood of this localization. The pre-puberty condition to which he has returned seems to indicate that the rest of the pituitary body has been affected as well.

Dr. F. Parkes Weber remarked that the association of diabetes insipidus with alopecia was most unusual, although both alopecia totalis and alopecia areata were sometimes more or less connected with nervous diseases or nervous shocks. He (Dr. Weber) was not astonished to hear that nothing abnormal in the region of the pituitary fossa could be detected by X-ray examination of the base of the skull, nor was anything abnormal seen in a case of diabetes insipidus recorded by Dr. Weber in 1916.[1] The patient in that case was a man, aged 37, who suddenly developed diabetes insipidus about two years before his death (from pulmonary tuberculosis). At the necropsy the pituitary fossa was found to be not much, if at all, enlarged, but the posterior lobe was

[1] F. Parkes Weber and H. Schmidt, *American Journal Medical Sciences*, Philadelphia, 1916, cliii, p. 892.

relatively enlarged and of a yellowish-brownish colour, and on microscopical examination was seen to contain large clumps of cells resembling xanthoma cells, which doubtless gave it its peculiar colour, reminding one of the colour of the suprarena cortex. Some would probably go so far as to name the condition in that case one of "xanthoma of the posterior lobe of the pituitary gland." He (Dr. Weber) had not heard of any other case in which a similar condition was found at the necropsy.

In the present case shown by Dr. East he suggested that a pituitary preparation made from *both posterior and anterior lobes* should be tried, for defective action of the anterior lobe (hypopituitarism) might be a causal factor in the patient's alopecia.

Congenital Absence of Sternal Portion of the Pectoralis Major.

By Douglas Firth, M.D.

The patient is a boy, aged 11, with absence of the sternal portion of the left pectoralis major. The condition was only discovered a few months ago, when the boy was sent to hospital for slight scoliosis.



Congenital absence of sternal portion of (left) pectoralis major. (Dr. Hobbs' case.)

Dr. F. B. Hobbs called attention to the X-ray appearance of the chest in such cases. He said that from the fact that a thick layer of muscle was absent, it was obvious that the rays were able to penetrate the chest with greater facility on the affected side, so that the radiogram showed a marked difference in density between the two sides. Unless this condition was kept in mind one might be led to conclude that the relatively dense shadows on the normal side were indicative of some pathological change. He said he had recently seen a case in which the radiographer had reported: "Increased opacity right side, suggests tuberculous disease."

It was obvious, however, that the difference in appearance in the radiogram on the two sides was entirely due to the absence on the left side of the sternal portion of the

pectoralis major, which could be clearly distinguished on the right side by its sharply defined lower border. The clavicular portion of the muscle was present so that in the radiogram (herewith) there was no difference between the two apices as there was in the middle third of the chest.

Dr. A. HOPE GOSSE showed a Case of Mitral Stenosis and Heart Block.

Notes on Case of Pott's Disease with Nervous Phenomena.

By J. PATERSON ROSS, F.R.C.S.

R. S., AGED 30, carpenter.

History.—1915: While in France had an attack of pleurisy with effusion. A large quantity of clear straw-coloured fluid was drawn off. Complete recovery. Since that time occasional attacks of pain in the back, necessitating his being placed on light duty.

1918: Discharged from Army A1.

1920: Pain in back much worse, and angular deformity of lumbar spine first noted. Sanatorium treatment in spinal (poroplastic) jacket for a year.

1922: Transferred to Papworth, where he was given work to do. After two months broke down completely and had more pain. Was kept at complete rest on his back for eight months, and in a spinal jacket for four months, but for the past year he has been doing regular work, at first as a carpenter and later as a foreman.

November, 1923: Pain in back for two days associated with a "cold." Since then gradually increasing weakness and stiffness of legs, especially during morning and evening, and a feeling of constriction round his knees after a long day's work.

February, 1924: Medical officer observed deep reflexes increased in legs, and believing him to be spastic, sent him to St. Bartholomew's Hospital for investigation as a case of possible compression of the spinal cord. He has never had root pains, and no trouble with his sphincters. No pain in the back since November. Occasional "stiffness" after a long day's work. The patient states he is "highly strung" and often feels nervous.

Examination revealed a kyphotic deformity in the lumbar region, the spines of the second and third lumbar vertebræ being prominent; signs of healed tubercle at both apices and thickened pleura on the left side, but no signs of active pulmonary tubercle; the deep reflexes all over the body were increased. No other abnormality of the central nervous system was discovered; there was no evidence of any lesion of the cauda equina or spinal cord as a result of tuberculous spinal disease.

Skiagrams of the vertebral column show a healed lesion of the bodies of the second and third lumbar vertebræ with bilateral abscess formation and calcareous deposits in the psoas muscles.

The patient is shown for the interesting "functional" nervous signs simulating organic disease; and because he is a case of healed tuberculous spinal caries, with abscess formation, which had been treated by complete rest under ideal conditions for only eight months. He has been improving rapidly since he has been told that there is no disease in his spine or elsewhere to cause paralysis of his legs.

Three Cases in which Nervous Symptoms were due to Dilatation of the Deep Thigh Veins.

By T. Stacey Wilson, M.D.Edin., F.R.C.P.

The three cases I am about to describe are of considerable interest because they show how easily the actual cause of severe or troublesome nervous symptoms may be overlooked.

The first case might well be mistaken for pure neurasthenia, the second showed symptoms of muscular weakness, more especially in one leg, and in the third, a case of valvular disease of the heart, the patient showed incapacity for physical or mental exertion, which was suggestive of an overtaxed heart. All three patients were cured with dramatic rapidity as soon as dilatation of the deep thigh veins was prevented by the use of elastic thigh supports.

Case I.—Charles D., aged 50, complained of extreme tiredness after an ordinary day's work, and also of fullness and flatulence after meals, and, recently, of being nervous and distressed with a tendency to weep. On one occasion he was overcome with an attack of weeping as he was walking along the main street of the town near which he lived, and felt greatly humiliated by standing for a minute or so crying in the public highway.

In this case the diagnosis was suggested by the fact that after walking a quarter of a mile or so his legs would begin to ache, but the pain would not increase with a continuance of the exertion, as in intermittent claudication, but he could walk five or six miles if he chose to bear the pain and the intense weariness which the exercise would cause. Another diagnostic feature was that on returning home, utterly tired out after an ordinary day's work, he was more conscious of leg weariness than of head or back weariness.

The physical examination did not reveal anything abnormal. There was nothing to suggest varicosity of the leg veins, although there were a few slightly dilated superficial veins on the thighs.

The symptoms, however, pointed so strongly to his neurasthenia being due to irritation of the nerves of the thighs by dilatation of the deep veins that he was ordered elastic supports for the thighs.

The result was most satisfactory, for the treatment gave immediate relief to his symptoms. He stated in a letter, written nine months after beginning to use the thigh supports, that he now no longer became "desperately tired" as formerly; and that in this respect he was about 80 per cent. better than before he began the treatment.

With regard to his neurasthenic symptoms, he could also report an 80 per cent. improvement. His attacks of depression were less frequent, less severe, and of shorter duration. He also reported a 60 to 70 per cent. improvement in his digestion, following the general improvement in nerve tone, and not as the result of any special treatment.

Case II was that of a Birmingham business man, who for the last few months had discovered that he was less able to take exercise than formerly, and had found that a round of golf—of which he was fond—was now more than he could manage without his experiencing extreme tiredness. He also found that his left leg was distinctly weaker than his right leg.

A careful examination failed to show any evidence of nervous disease, except that there was definite weakness of the left thigh muscles, and that he could not step up on to a chair without very considerable assistance when using his left leg, although he could do so when using his right leg.

There were no other symptoms of dilated deep thigh veins, neither was there any dilatation of the superficial veins such as is often noticeable where the deep veins are at fault. Nevertheless, in the absence of any symptoms of nervous disease it seemed probable that there might be some local cause for the muscular weakness, and that dilatation of the deep thigh veins might exist. He was ordered thigh supports, and they at once cured all his muscular weakness, and in a few days he was able to resume his former activities and to enjoy a full round of golf as much as formerly.

It is a point of great interest to know that irritation of the nerves of the thigh through dilatation of the deep thigh veins can cause definite muscular weakness of the thigh muscles, apart from any definite evidence of neuritis. It is possible that pain may have played some part by inhibiting muscular action, but the patient did not appear to be prevented by pain from using his leg. He simply felt it to be muscularly weak and that walking tired it unduly.

Case III is also one in which the true cause of the weakness might very easily have been overlooked.

The patient was a lady, aged 27, who was engaged in teaching. I first saw her in the spring of 1920. She was then governess in a lady's house, and was suffering from cardiac dilatation and rapidity due to mitral stenosis, accompanied by some symptoms which suggested Graves' disease.

I advised six months' rest in bed, and gave a good prognosis so far as her ability to earn her living and enjoy life was concerned.

The heart decreased to its normal size and regained its vigour before the end of the six months, and shortly afterwards she recommenced teaching in a school in London, which did not involve any afternoon work. After eighteen months she began to undertake afternoon teaching as well, but found that only an hour or two of this seemed to overtax her strength and cause a considerable amount of tiredness.

This exhaustion, after a very moderate amount of work, persisted in spite of all the care she could take of herself. In June, 1923, she came to Birmingham to see me, as I felt that the heart might again be getting dilated. I was, therefore, surprised to find that all evidence pointed to the heart being, if anything, stronger than it was at her previous visit some six months earlier.

This fact induced me carefully to inquire into the exact nature of the tiredness and exhaustion from which she had suffered, with the result that I came to the conclusion that the thigh veins were probably the cause of her trouble, and I therefore advised the use of thigh supports. The result was most gratifying to both patient and myself, and in October, 1923, she wrote to me as follows :—

> " Three months ago I was unable to go through my day's work without the very greatest fatigue. As the day went on I could feel my strength ebbing away and by evening I felt always completely ' used up ' and unfit for either work or pleasure.
>
> ' From the time I started to wear the thigh supports I have been a different person. I was almost immediately able to enjoy a day's work, and during my August holiday could easily walk six miles instead of a difficult two or three.
>
> " I cannot express sufficient thankfulness for the relief which the wearing of these thigh supports has brought."

The history of this case emphasizes a point which must be most carefully attended to when cases of this type are being treated, namely that the elastic thigh supports must only exert very gentle pressure, and not the firm pressure

which an ordinary elastic stocking exerts upon varicose veins of the leg. One pair of supports which were made too tight for this patient gave her no relief whatever, and it was only when the supports were made of the proper strength that they proved curative. The pressure applied by the elastic bandage ought not to exceed about 3 or 4 oz., and this may be obtained by extending a 15 in. piece of an elastic webbing bandage by about 1 in.[1]

A Case in which Loss of Memory suggestive of Double Personality was due to Colon Disturbance.

By T. Stacey Wilson, M.D.Edin., F.R.C.P.

This case is one of extreme interest for it seems to afford unmistakable evidence that interference with some of the higher functions of the brain may be brought about by reflex disturbances of intestinal origin.

The patient, a commercial traveller, aged 47, consulted me in June, 1915, for certain alarming mental symptoms which were associated with headaches but with no other symptoms of illness, except occasional attacks of reflex vomiting. He stated that for six or eight months he had been liable to attacks of a peculiar headache, which would come on about 9.30 or 10 a.m. and were invariably followed within a few minutes by a complete loss of memory, which would last from one to three hours. During this time his actions appear to have been normal but to have left no impression on his consciousness. He would, as a rule, regain consciousness of his actions at the time of his mid-day meal, but would be wholly unconscious of the way in which he had spent the morning. On looking at his pocket-book, however, he would find full and satisfactory notes of the orders which he had booked and of the accounts which had been paid and, so far as he could ascertain, there was nothing peculiar about his actions during those hours of which he had no remembrance. These peculiar attacks at first used to come on once or twice a month, and had gradually increased in frequency until at the time he consulted me, they were occurring two or three times a week and he felt extremely anxious as to his future.

He said he could fully understand how a man might forget his own identity, and was afraid lest this might happen to him. He was quite normal in conversation, and there was no evidence of want of mental balance of any sort.

The only facts which were elicited by a careful physical examination were that his colon was decidedly hard, and somewhat tender on pressure, and that he had no pain or other conscious symptoms due to this cause. Such an amount of abnormal muscular activity in the colon was calculated to cause pain, or reflex disturbance of the stomach, or of the circulation in place of pain. In the absence, however, of any other symptom it was justifiable to consider that the mental disturbance was the only manifestation of the colon abnormality.

The probability of the colon origin of this mental disturbance was increased by the fact that the headache, which always preceded the loss of memory, began thirty to forty minutes after the morning evacuation of the bowels. The patient was quite definite in his statement as to this relationship and had evidently carefully observed the connexion between the two occurrences.

[1] This subject is dealt with in my book "The Early Diagnosis of Heart Failure and other Essays" (London: J. Murray).

These two facts, namely, the evidence of hardness and tenderness of the colon and the close relationship of the commencement of the attacks to the act of defæcation pointed to the desirability of treating the case as one of disturbance originating in the colon.

He was given an intestinal antiseptic consisting of the perchlorides of mercury and iron with 20 minims of tincture of hyoscyamus, three times daily, and was advised to cut out from his dietary all fibrous or hard vegetable material.

The results of the treatment fully supported the diagnosis, for from the time he began taking the medicine he did not again lose himself while on his business rounds. He did, however, have one attack of loss of memory shortly after beginning to undergo the treatment. This was apparently due to the effects of a very heavy day's work, which involved a journey from Brighton to London on a Saturday morning, and then one to Woolwich and back just in time to catch the last train from London to Brighton. On the Sunday morning he remembered going to the chapel he usually attended and seeing the minister enter the pulpit at the beginning of the service, but from that time onward his memory was blank until he awoke to consciousness of his surroundings in the dining-room of his hotel just before mid-day dinner.

He had no return of his loss of memory but on my inquiring some years later, I found that occasionally he had had recourse to the mercury, iron, and hyoscyamus mixture at times when he was not feeling well, and was fearing lest his old trouble might return; and he found that it always relieved him.

It is not at all infrequent to meet with cases in which mental depression, suicidal melancholia, or obsessions, which bring the sufferer to the borderline of insanity, are clearly due to colon disturbance, and are dramatically cured by appropriate treatment; but this is the only case I have met with in which the disturbance of the colon seemed to be quite certainly the determining factor in the production of a symptom so definitely psychical as this, in a patient who was otherwise of perfectly normal mentality.

Dr. F. PARKES WEBER suggested that perhaps the temporary loss of memory, &c., which followed actions of the bowels in this case might be compared to the extreme fatigue, distress, or somnolence which followed fluid actions of the bowels in certain cases. These symptoms were apparently of toxic origin and tended to occur especially in rather feeble individuals who were accustomed to have their bowels opened every other day with purgatives. The symptoms seemed to be due to absorption of the abnormal fluid material left in the rectum after the bowels had acted. Such persons usually felt at their best on the days between those on which the purgatives acted. If in such cases a surgical operation had to be performed, it was a question if it ought not to be performed on one of the days on which the patients usually felt at their best.

Clinical Section.

President—Mr. G. E. Gask, C.M.G., D.S.O., F.R.C.S.

Pains of Tabes Dorsalis persisting throughout, *inter alia*, a Lower Limb which was Amputated at the Hip-joint in 1910.

By Hildred Carlill, M.D.

F. H., aged 53. The clinical diagnosis of the disease of the left hip-joint in 1910 was, first, sarcoma, later tuberculous disease. The pathologist's report of the specimen was septic arthritis. It is likely that the condition was Charcot's disease.

Syphilis is said to have been acquired at the age of 20. Salvarsan never given. Unequivocal evidence of tabes dorsalis at present date, including Charcot's disease of the right tarsus, since 1910. Knee-jerk healthy; ankle-jerk, absent.

Wassermann reaction negative in both serum and cerebro-spinal fluid; the latter is otherwise healthy in every particular.

Pathological Laughter associated with High Blood-pressure in a Woman aged 66.

By Hildred Carlill, M.D.

Mrs. K. C., aged 66. One of a family of eleven, nine of whom are living. She has five children. Family history and previous personal history reveal nothing bearing on the case.

Two years' history of progressive, causeless, involuntary, non-pleasurable laughter, in attacks. Onset gradual and without obvious cause. The faculty of normal healthy laughter is retained. Central nervous system otherwise healthy.

Systolic blood-pressure greater than 270 mm. Hg. Cardiac enlargement, without bruits.



FIG. 1.—At rest.



FIG. 2.—Normal healthy laughter.



FIG. 3.—Abnormal paroxysmal laughter: not induced by pleasurable emotion and causing merely distress.

Case of Juvenile Myxœdema.

By Douglas Firth, M.D.

The patient is a girl, aged 14, who has suffered from the disease eight years. At the age of 6 it was first noticed she was not growing out of her clothes.

Her infantilism is of the Brissaud type, but she shows more signs of hypothyroidism than usual.

Her height is 3 ft. $7\frac{3}{4}$ in., and weight 3 st. 9 lb., this being the normal height and weight of a child of 7 years; for a child of 14 years the normal height is 4 ft. $11\frac{3}{4}$ in., and weight 6 st. 12 lb.

Patient is intolerant of thyroid extract, which causes headache, diarrhœa and vomiting.

Case for Diagnosis.

By J. L. Livingston (introduced by Douglas Firth, M.D.)

E. S., female, aged 20. *History*: Has suffered from indigestion and flatulence for one year. Began to suffer from indigestion at 14 years of age. Eyesight became weak when she was 15 years old. First appearance of menses and sexual characteristics at 16 years of age. Amenorrhœa, loss of pubic hair, loss of subcutaneous fat have been present for one year.

Optic Examination.—Minute nodules along the course of the retinal vessels.

Wassermann reaction negative. Cerebro-spinal fluid normal.

Achlorhydria. Urine contains a trace of albumin and a few granular and waxy casts.

Diaphysial Aclasis or Periosteo-osteo-dysplasia (Multiple Exostoses) with Shortness of Forearms.

By F. Parkes Weber, M.D.

It is now generally recognized that multiple "exostoses" must be regarded not as an example of true tumour formation but as an inborn disorder in the growth and development of the long bones. This disorder may occasionally be familial, like the other developmental bone-disorders, achondroplasia (chondrodystrophia fœtalis), multiple enchondromata (chondrodysplasia and "hereditary deforming chondrodysplasia"), and osteogenesis imperfecta (fragilitas ossium congenita et tarda). Sir Arthur Keith[1] has proposed to call the disorder in question *diaphysial aclasis*, a term suggested to him by Mr. Morley Roberts, because it is mainly due to a want of pruning or modelling of the diaphyses (generally the ends of the diaphyses) of the long bones; it affects only those parts of the skeleton which are developed from both cartilage and membrane.

The present case is that of a young man (P. P.), aged 23 years, who is the eighth of a family of twelve children. Six of the family, including the patient, are living. Of them, he is the only one known to have any bone disorder,

[1] Sir Arthur Keith, "The Nature of the Structural Alterations in the Disorder known as Multiple Exostoses," *Journal of Anatomy*, Cambridge, 1920, liv, pp. 101-115.

excepting an elder sister, now aged 35 years (the third child in the family), who at the age of 18 years underwent an operation on one leg, apparently for an old rachitic deformity.

The exostoses and other disturbance in the development of the long bones are well shown in the accompanying illustrations, from photographs (figures 1 and 2, kindly taken for me by Dr. E. Bock), and Roentgen skiagrams. The skiagrams show exostoses (various degrees) of the radius, ulna, femur, tibia and fibula on both sides of the body. Apart from the exostoses, which are situated at the ends of the diaphyses of the long bones, the chief deformity is

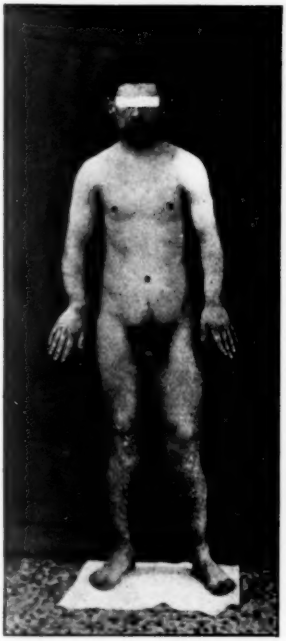


FIG. 1.—Showing the deformity of the upper extremities, and general good development.

in the forearms[1] and wrists. The ulna on both sides, owing partly to absence (agenesis) of its distal epiphysis, is too short and does not reach down to the wrist, so that there is deviation of the hand to the ulnar side. This deformity and shortening of the forearm is more marked on the right (fig. 2) than on the left side. In both forearms (fig. 3 and 4) both the ulna and the radius (the ulna more than the radius) are slightly bowed, with their concave sides towards one another; and a striking feature in the skiagrams is the shadow evidently due to a buttress of periosteal bone-formation on the concave (radial) side of the

[1] Cf. Keith, *loc. cit.*, p. 110 and fig. 11.

shaft of each ulna, as if caused by the interosseous membrane dragging on the periosteum; there is also skiagraphic evidence of a little similar bone-formation on the concave (ulnar) side of each radius.

In other respects the patient seems to be physically well developed (fig. 1) and to have enjoyed good health. He can use his hands well for writing and all ordinary purposes and work, but he complains of psychasthenic symptoms and has no employment. Roentgen-ray examination of the sella turcica region



FIG. 2.—Showing the deformity of the right upper extremity.

of the skull (Dr. James Metcalfe) shows nothing abnormal. There is no Roentgen-ray or other evidence of any exostoses of the head.

Sir Arthur Keith kindly informs me that he has seen over twenty cases of diaphysial aclasis and that the onset of the disorder varies in different cases from fœtal life onwards. In my present case it seems to have first showed itself at two years of age.

It should be noted that multiple chondromata (chondrodysplasia) may be likewise in some way causally associated with shortness of the affected limbs. In the case of a boy (V. K.), aged 13 years, whom I showed on February 13, 1920, the chondrodysplasia was associated with unilateral dwarfism of limbs.[1] That

[1] F. Parkes Weber, "Unilateral Dwarfism of Limbs connected with Congenital Multiple Chondromata," *Proc. Roy. Soc. Med.* (Clin. Sect.), 1920, xiii, pp. 54-57.

case was perhaps similar to one of Mr. J. E. Adams, with shortening of limbs on one side of the body[1] and one demonstrated by Dr. H. C. Cameron and Mr. W. H. Trethowan, in April, 1918,[2] in which there were multiple enchon-



FIG. 3.—Skiagram of right forearm.

dromata with a difference in size between the two sides of the body (a difference noticed already at the child's birth).

In regard to multiple exostoses, though I have used Sir A. Keith's term, "diaphysial aclasis," it is clear that the developmental disorder in question is

[1] J. E. Adams, *Proc. Roy. Soc. Med.* (Sect. for Study of Dis. in Child.), 1919, xii, p. 5.
[2] Cameron and Trethowan, *ibid*, 1918, xi, p. 45.

not merely due to a want of "pruning" or modelling of the diaphyses of the long bones. In some cases, as in the present case, there is agenesis of the distal epiphysis and distal end of the ulna in one or both forearms. There is likewise in the present case a remarkable buttress-like periosteal bone-formation, to

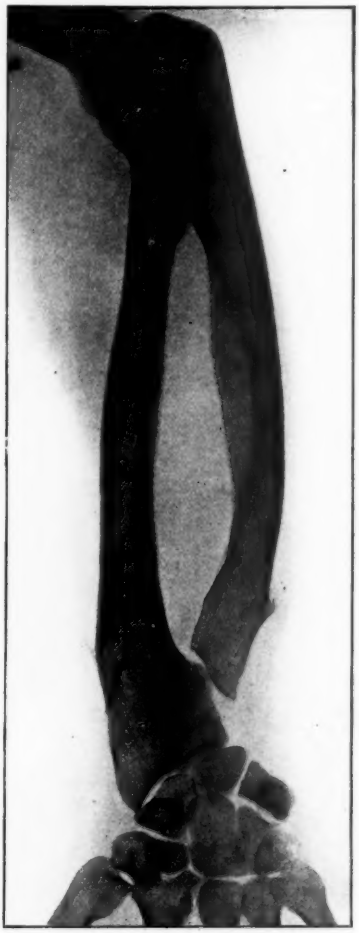


FIG. 4.—Skiagram of left forearm.

which I have drawn attention, on the concave (radial) side of the diaphysis of both ulnæ, and to a slight extent on the concave (ulnar) side of each radius, possibly (as I think) due to a dragging on the periosteum by the stretched interosseous membrane. I venture to suggest *periosteo-osteo-dysplasia* as a

suitable name for developmental multiple exostoses and the accompanying disturbance in the growth of the long bones. This would fit in well with the use of the term *chondro-dysplasia* as applied to the similar developmental disorder, multiple chondromata or multiple enchrondromata. I see, however, that in the *Oxford Medicine* E. A. Locke (1921) has grouped together multiple exostoses and multiple chondromata under the heading, "Hereditary Deforming Chondrodysplasia," the name employed by A. Ehrenfried, in his first paper on the subject (1917).[1]

Case of Achalasia of the Cardia relieved by Operation.

By A. F. HURST, M.D., and R. P. ROWLANDS, M.S.

I.—Dr. A. F. HURST.

PATIENT, S. H., a male, aged 38 years.

History.—First attack was in 1916, when away in France, a piece of meat "sticking in his gullet" for sixty hours. He could not bring it up, but was able to bring back water, which he drank in large quantities, hoping that in this way the obstacle would be moved. Eventually he spat up the piece of meat and was "all right" for two weeks.

Then he experienced a tight feeling round the chest after meals. Three months later he had to empty his œsophagus regularly after every meal.

I first saw him at Netley, in February, 1917, where he began to use the mercury tube. He was able to do eighteen months' work between his discharge from Netley and his first admission to Guy's Hospital in March, 1920, using the mercury tube daily. He lost weight steadily, however, and when admitted in 1920 weighed only 8 st. 9 lb,. as against a normal weight of 10½ stone.

1920.—At Guy's Hospital he was given a mercury tube larger than any he had used before.

1921.—The condition persisted and he returned to hospital in 1921 and again in 1922, on each occasion being thinner than previously. Forcible dilatation with Plummer's bag was tried, but without any improvement resulting in his condition.

1923.—In May his œsophagus had become so dilated that it held 510 c.c. of fluid without causing the patient much discomfort.

1924.—Readmitted in January; weight 9 st. 1 lb; this had been down as low as 8 st. 10 lb.

Operation.—On February 15, the operation described below was carried out by Mr. Rowlands.

The patient did well and swallowing was easier from the first. A small fistula formed, but closed in ten days. Since the operation he has been putting on weight. He weighs 9 st. 9 lb., which is more than he has weighed since 1917. He is able to take fish and solids fairly well.

The operation confirmed the view put forward in 1915[2] that so-called idiopathic dilatation of the œsophagus is not due to cardio-spasm, but to

[1] A. Ehrenfried, *Journ. Amer. Med. Assoc.*, Chicago, 1917, lxviii, p. 502.

[2] *Proceedings*, 1915, viii (Clin. Sect.), p. 22.

absence of normal relaxation (achalasia) of the cardia when each peristaltic wave reaches it.

II.—Mr. R. P. Rowlands.

Operation.—Intratracheal anæsthesia was given by Dr. E. A. Scott in order to lessen the movement of the diaphragm. A long, oblique incision was made 1 in. below the left costal margin, the fibres of the left rectus were divided and the superior epigastric artery tied. The seventh costal cartilage was divided close to the sternum and also the seventh and eighth about 2 in. lower down, the intervening portions being removed. This allowed a very free retraction of the left costal margin and gave a good view. The left lobe of the liver was mobilized by dividing the left lateral hepatic ligament and separating the liver from the diaphragm for 3 in. The left lobe was folded backwards and drawn to the right to display the cardiac orifice of the stomach. The stomach was then drawn downwards and to the right, the peritoneal reflections from it to the diaphragm at the cardia being divided and the œsophagus gradually drawn down so that ultimately a length of about 1½ in. was visible in the abdomen. A large mercury tube was then introduced from the mouth, but was arrested in the œsophagus, about 1½ in. from the stomach. The oblique muscular fibres at the cardia were separated and the deeper circular fibres were cut across along the front of the œsophagus for at least 1 in. The sphincter was so thin and wide that it was difficult to recognize it; it was certainly not hypertrophied. In order to make sure that none of the fibres were left undivided, the mucous membrane of the œsophagus was opened about 1½ in. above the stomach; the hole was closed with two fine catgut sutures. The mercury tube then passed easily into the stomach.

It was noticeable that the part of the œsophagus visible was not dilated but natural in size. The stomach was smaller than normal. The parietal incision was closed, except that a small rubber drain was left at its outer angle and extending towards the cardia. The patient was kept on rectal salines and glucose for the first three days, only a very little water being given by the mouth. Fluids were then given by the mouth; two days later they began to leak through the lower part of the abdominal wound. Mouth feeding was stopped for two days and the patient was put back on rectal salines, but he disliked these so much and became so intolerant of them that feeding by the mouth was begun again, thicker food (such as milk puddings and porridge) being given with great advantage. Hardly any leakage occurred, and within a week the sinus had healed and the patient was able to swallow almost any kind of food.

The morning after the operation he knew that his obstruction had been corrected, for no saliva had accumulated in the œsophagus and the usual feeling of distension of the œsophagus had disappeared. He had been accustomed to bring up half a pint of saliva every morning.

Conclusions.—It appears to be much better to resect a portion of the seventh and eighth costal cartilages than to divide the seventh, eighth and ninth ribs near their angles, as recommended by some surgeons, for this entails a definite risk of pneumothorax.

Division of the left hepatic ligament helps very much, as it allows the left lobe of liver to be drawn aside, and in this way an uninterrupted view of the cardia is obtained.

It is of vital importance to recognize that in achalasia of the cardia the sphincter at the lower end of the œsophagus is very wide and thin, and is not hypertrophied.

Case of Dermatitis Exfoliativa and Generalized Melanoderma.

By HERBERT FRENCH, C.B.E., M.D.

GUSTAVE B., aged 60; a Dane who formerly went to sea, but who has worked in shipyards at Hull for over twenty years.

The skin trouble began in 1921, when he was doing some painting with grey and green paint; up till then his only previous illnesses of moment had been yellow fever on the West Coast of Africa in 1883; a duodenal ulcer, for which gastro-jejunostomy was performed in 1908; an attack of severe conjunctivitis in 1915, lasting four months; and in 1918 an operation for anal fissure. The present condition dates from March, 1921, beginning with a sore place on the dorsum of his left hand which he attributed at the time to the effect of a scratch from a piece of tin whilst he was painting; this area grew to the size of a half-crown and several more spots occurred on his fingers and left arm. Each of the places irritated him very much and he was always scratching them. They were treated with an ointment which he described as being like Stockholm tar. Without apparent cause similar spots occurred later on the inner side of the left thigh and behind the left knee-joint, with intense irritation, and when he scratched the skin watery fluid exuded, though there was no vesicular eruption. About a month from the time of development of the sore place on his hand, his whole body became affected by what was then thought to be a "nettle-rash." He has done no work since, and all the time has been troubled by intense irritation of the skin; sometimes associated with extensive desquamation, always with a tendency for him to scratch, and when scratching is continued always with watery liquid appearing over the parts scratched deeply. At first there was no obvious pigmentation, but the skin trouble had become so much worse in 1923 that in June of that year he was admitted to Hull Infirmary and had treatment by numbers of different ointments and applications; it was at this time that he first had any swelling of his legs. The swelling had been variable and generally only slight, but he began to find himself weak in the legs, having to use a stick for support in walking about. His hair, which used to be thick, became thin and scrubby, and towards the end of 1923 he gradually began to assume his present colour. He himself attributes the change in colour to his scratching so violently, owing to the intense irritation. The condition of his skin and its colour are obvious without description; the only positive findings amongst many investigations that have been made in an attempt to find the cause are: first, that he has achlorhydria; secondly, that he has eosinophilia to the extent of 16 per cent.; thirdly, that the pigmentation, which has been universal, is distinctly fading upon the face, knees, hands and feet.

The diagnosis suggested is dermatitis exfoliativa of unknown origin with intense pigmentation as the result of the dermatitis; the pigmentation therefore being an accidental rather than an essential feature of the complaint.

A small portion of the skin has been excised and examined microscopically, and the section shows marked thickening of the epidermis, increase in pigmentation, and cellular infiltration of the corium. The pigment is thought to be obviously melanin.

Case of Early Recklinghausen's Disease in a Girl aged 10.

By Herbert French, C.B.E., M.D.

H. N., aged 14. A case of multiple subcutaneous nodules. Diagnosis suggested: early Recklinghausen's disease.

Small hard lumps, some single, some in groups, began to develop over the patient's back three years ago; they were painless but slowly increased in number until the whole of the back, the front of the chest, the neck, and to a less extent the limbs, had these subcutaneous nodules grouped upon them. In the limbs they are tiny, more or less linear in distribution as though along the course of nerves. On the back they are multiple and irregular, the largest as big as hazel-nuts, but many are tiny subcutaneous nodules, smaller than small peas. They are less numerous on the chest and abdomen; some can be felt beneath the skin of the forehead and face; there appear to be none in the hairy part of the scalp, but there are many in the nape of the neck reaching to the occiput. All are subcutaneous, freely movable, painless. The patient's health is good. There appears to be no visceral disease.

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VOLUME THE SEVENTEENTH

SESSION 1923-24

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1924

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Section of Comparative Medicine.

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The Integration of Medicine.

PRESIDENT'S INTRODUCTORY ADDRESS.

By The Right Hon. Sir CLIFFORD ALLBUTT, P.C., K.C.B., M.D., F.R.S.

IF for years slowly and almost silently our work makes its way we must be content; our experience of the world teaches us to be content; but happily, now and then, after long hewing in the dark forest, we break into the light; we find ourselves almost suddenly upon a peak, our way open and bright before us, and our cause justified before men. Such a festival is our meeting to-day.

The barriers of convention, which ought to be slender and occasional, are of all the most inveterate and impregnable. Of such have been the barriers devised by medieval perversities which, to its grievous injury, have split up medicine into fractions. As physic was divorced from surgery and mind from body, so the diseases of animals and plants were separated from those of mankind. The folly of the division of the medicine of the hand from the medicine of the bottle has now become so glaring that our next festival may be on the blowing up of this rampart; indeed, the gynæcologists have exploded their end of it already. But it is a big business to transform a medieval castle, with its baileys, barbicans and keep, into a modern domain.

Let us rejoice then together that by the liberality, foresight, and generosity of the Royal Society of Medicine we are enabled to-day to celebrate the unity of medicine as a biological study. That you have made me your President on this memorable occasion, for in the history of medicine this day will take a memorable place, is a privilege for which I am deeply grateful to you. We have the mind of James Paget with us; and that of Sir John Bland-Sutton, happily still in the body, with us also. By him I have felt that this chair ought to have been occupied; for, while I have been talking about comparative pathology, he has been working in the field; but he has been called to a higher place, a distinction on which we all cordially congratulate him. For personal help in our campaign I should also like especially and gratefully to mention General Sir Frederick Smith, Professor F. T. G. Hobday, Mr. W. M. Scott, of Bridgwater, and Professor T. B. Wood, of Cambridge.

A Preliminary Survey.

I think you will agree with me that we must not swarm upon our new estate and try to do too much at first. If we do we may tire ourselves, overrun our work, and tread down our crops. Let us survey our ground carefully and make good by degrees. Many of our supporters are very busy men, and may be discouraged by too long papers or too many meetings. The secretaries will, I am sure, do their best to see that the papers laid before you are concise, relevant, and not too abundant.

Professor W. H. Welch, of Baltimore, has written to me in these words: "Your first thoughts will be those of comparison . . . to shed cross lights reciprocally from the pathology of one kind of living thing upon another, watching the variations of functions in various biological systems." Consider indeed for a moment what, without the comparative method, would have now been the state of those other sciences which have worked by this method for many years past. Without the comparative method, what of anatomy, or of anthropology, or of embryology; or what again of philology, of history, or even of theology? Without the method they would never have attained to the master conceptions which have lifted those studies from the plane of hobbies, or of literary amenities, or of mere erudition, to the rank of scientific ideas. Incidentally it is true that pathologists have already done much by their experimental researches—as into the properties of the cell, in the study of infections, and so forth—to gather in some comparative pathology, by parcels; but this is a straggling collection of knowledge; it gives us no general view of the whole, nor co-ordination of its parts.

Pathological Biology.

A certain portion of the time of this Section will be devoted to the utilitarian side of our subject; to the study of the diseases common to man and animals—many of which may be carried by animals to man, and conversely; to the ravages of disease in our herds and our crops; and, on the other hand, to the respective immunities and defences of plants and animals, as, for example, the immunity of the ox tribe to glanders, mentioned by Professor Hobday at the Glasgow meeting of the British Medical Association, or again to our experience of anthrax serum, effective in man, in animals ineffectual; and again to the maintenance of the balances of species in the economy of nature. But, as I wrote in the *Times* in 1906 (June 5), while not neglecting these instant needs we shall not permit our attention to be distracted from the pursuit of the larger and more disinterested researches: from a pathological biology which hereafter shall give us not only a control of several diseases, but an insight into large and fertile principles, foresights and controls over wide fields of nosology; interpretations of the various comings and goings of the manifold kinds of disease in all living things—plants and animals, of specific kinships underlying superficial differences, and of disorders of the more complex animals by the light of elementary changes in the simplest animals and in plants in which the plasticity, I had almost said the pregnancy, of protoplasm shows us in manifold ways its marvellous endowments. We must have a keen eye to practice, whence indeed many of our most fertile problems arise; but we must aim also at the building up of the great principles upon which depends the practice of the future. I may recall the following reflection of the Agricultural Correspondent of the *Times* of 1906 (presumably Dr. Fream): "From a pathological point of view they (diseases of sheep) are a perfect mine

of wealth, for they are fraught with scientific problems of the highest interest and importance, and are most suggestive of what may turn out to be a new light on the pathology of many of the contagious and infectious diseases of man and the lower animals." At this moment there appears in the *Proceedings of the Royal Society* (Ser. B, vol. xcv, p. 228, Sept. 1, 1923) a very interesting study of that strange monstrosity, achondroplasia. Few human achondroplastics manage to get born: of the few were a certain Egyptian god—who perhaps did not need a uterine phase—Æsop and Quilp. They are dwarfs with big heads and bodies, short arms and legs, and fin-like hands. Now Mr. Crew has illustrated the pathology of these monsters by certain monstrous calves of the Dexter breed, and postulates a perversion of the pituitary gland as the cause of the monstrosity. Moreover, he points out that this deformity may even shed some light upon the species problem.

I ought not to close an address, even so informal as this, without at least an allusion to the name of Pasteur, who threw open vast fields of comparative pathology; to mention the name of the master is sufficient to turn your hearts silently to the Centenary Commemoration of one of the greatest of men. I will conclude these remarks with the following quotation from another master of biological science, John Hunter: "In the course of a variety of experiments on animals and vegetables I have frequently observed that the results of experiments in the one have explained the economy of the other, and pointed out some principle common to both."

I now ask you to join me in wishing long life and prosperity to our new Section. In Benjamin Franklin's now proverbial phrase, "Hard work is still the road to prosperity, and there is no other."

Mr. H. R. BEETON said that as a layman who had been privileged to be present on this interesting occasion, he was glad of an opportunity to express the general gratification at the important steps in the history of medicine involved in the inauguration by the Royal Society of Medicine of a Section of Comparative Medicine. Not only were we all richer for the promise of increase in knowledge, but he (the speaker) had reason to know the greivous disabilities under which Animal Medicine had laboured for so long as a consequence of the ignorant isolation to which it had been condemned. He was not acquainted with the steps which had led to this event, but for a long time he had watched, with great interest, the exemplary devotion and persistence with which the President had advocated the cause of Comparative Medicine, and he (the speaker) could enter into the satisfaction with which Sir Clifford was entitled to contemplate this measure of its realization. It would, indeed, have heen extraordinary, in these days when physical science was revealing so dramatically the unity of nature, if the investigation of human and animal disease had continued to be pursued as separate studies.

Cryptorchidism in Animals and Man.

By FREDERICK HOBDAY, C.M.G., F.R.C.V.S., F.R.S.E.

BEFORE commencing I should like to be allowed to say how very much I appreciate the honour of being asked to introduce the first discussion before this new Section of Comparative Medicine.

The subjects which may be brought forward before a Section of this kind are so varied and numerous that the reader of a paper may be legitimately excused if he is puzzled to know from which side to select his title. There are so many sides to comparative work, so many pathological conditions and surgical operations which have analogies, and so many others which have

differences; for although the veterinarian may perform a similar operation on his patients as is done by the human surgeon upon man, it is often done with a totally different object.

I have chosen the subject of cryptorchidism partly because it is an abnormality which is as common in the domesticated animals as in man, if not commoner, and in this connexion I want us first to compare notes particularly on the hereditary aspect of the question; and, partly, because I have the experience of over thirty years in operating for the relief of such cases in animals.

The results of this trouble are looked at by the medical and veterinary practitioners from a totally different point of view, as a successful operation in veterinary practice always means an increased monetary value of the patient to its owner, whether that value is represented in the horse tribe by the pecuniary aspect on account of the resulting docility, or whether it is from a sentimental point of view, as in the cat and dog (in the former, to do away with the objectionable smell of the urine which always accompanies the male, or in the case of the dog to do away with his irritable temper or to prevent him wandering). Thirdly, because I had the promise of the attendance here this evening of several well-known surgeons who have made a special study of this abnormality in man, and I am hoping on that account that the discussion which will ensue will bring out many points of mutual interest. There is much in common in the subject of cryptorchidism in animals and man, but there are also some very interesting contrasts and variations. With the human surgeon it is a matter of importance that the testicle shall be saved if possible; with the veterinarian it is of much more importance that this offending organ should be removed; this being necessary in our patients on grounds of economy, safety (both to man and other animals), and practical utility.

Heredity.—Our first reason in pedigree animals concerns the question of heredity, for there is no condition with which we have to deal in veterinary surgery which is more inherited than that of the undescended testicle. The tendency for a horse with one testicle retained and one in the scrotum to produce progeny having similar defects is well recognized not only by the veterinary surgeon but by every intelligent breeder of pedigree stock; and it is well illustrated in districts where a cryptorchid stallion has been allowed to be used at stud. That the abnormality can be passed on through the female line is also well recognized, and a filly foal which has been got by a unilateral cryptorchid sire must always be an object of suspicion if put to the stud. The same tendency is well recognized by dog breeders, and in examining a male dog for stud purposes the veterinary practitioner would always draw the careful attention of the prospective buyer of the risk he ran in using at stud a dog or cat whose testes were not both normally in the scrotum.

We all know full well the sequel which results to an animal on account of the removal of its testicles, how it becomes quieter in its habits with other animals of its own species, and more docile and more amenable to the will of man, and the successful removal of the hidden testicles has created an opening for the specialist in veterinary surgery in a similar manner to the way in which these operations have given opportunities in human surgery.

We term the animal with the hidden testicle a cryptorchid just the same as you do, and it may be unilateral or double. We have, too, "monorchids" when one testicle is entirely anatomically missing, and occasionally one meets with an "anorchid" in which both testicles are anatomically absent. The

agricultural community know an animal of this kind better under the name of "rig" or "ridgling," and the value of such a beast is so much deteriorated that it may even become entirely unsaleable, or, at any rate, its price, if the prospective purchaser discovers the defect, is less than one-half or one-third of its proper value. I have here specimens or sketches from actual specimens in which the condition of monorchidy and anorchidy are definitely manifested, and I have also photographs of cases of arrested development accompanied by

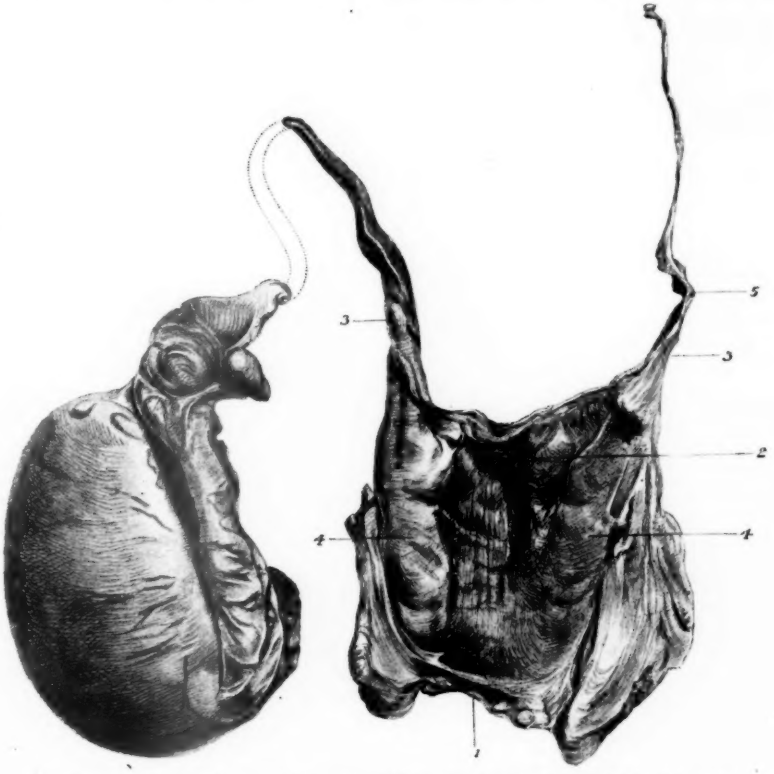


FIG. 1.—Abnormal organs of a monorchid horse, showing the scrotal testicle and the portion of the generative organs present on the other side beyond the fundus of the bladder. (1) Fundus of the bladder. (2) Fold of peritoneum uniting the vasa deferentia. (3) Vas deferens. (4) Bulbous portion of the vas deferens. (5) The vas deferens on this side was pervious as far as this point.

cryptorchidism (figs. 1. 2). An animal with this defect is usually known to the farmer by the name of "Will Gill," and is often spoken of as an hermaphrodite, but although I have met with, and operated upon, some thirty of these cases they have always proved to be males and I have always found testes present either in the inguinal canal or in the abdomen. They have all been true cases of arrested development.

The diagrams which I now show on the screen are taken from Colin's "Veterinary Physiology," and illustrate well anatomically the position of the undescended testicle in the fœtus at various stages and explain some of the reasons why the condition of cryptorchidism may result. For example, the peritoneal attachment may be abnormally short or abnormally long at a certain period of fœtal life. With the former the result might be that the testicle would never descend from its position in the lumbar region, but become almost a fixture, or it might descend a little way but not sufficiently to reach the internal inguinal ring. If abnormally long it might not reach the internal inguinal ring just at the time when this aperture would be sufficiently relaxed to admit of its passage and it might not reach it at all but be pushed out of its place by some of the internal organs. The testicle itself may be abnormally large, being cystic or otherwise diseased, and the epididymis is frequently found to be excessively large or mis-shapen. A short vas deferens or spermatic artery, too, may cause retention. The inguinal canal may be so narrow, or its entrance or external exit so small and abnormally contracted, just at the period when the testicle approaches, that this latter organ cannot gain

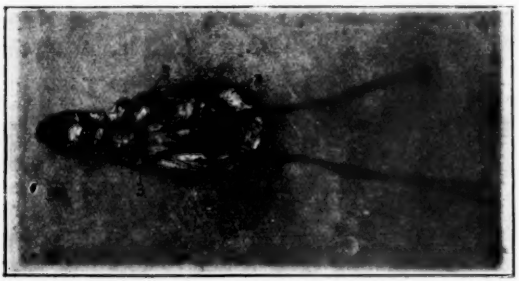


FIG. 2.—Abnormal genito-urinary organs of an anorchid colt. (1) Small masses of fat in the position of the testes. (2) Bladder. (3) Vesiculæ seminales.

admittance; or, if admitted, cannot pass through into the scrotal sac. The gubernaculum testis, by which the testicle is drawn through the canal towards the scrotum, may be paralysed so that its natural function as a guide is useless; and, lastly, there may be some abnormal contraction of the skin of the scrotum by which, although the testicle has reached the external inguinal ring or even passed through it, the organ is either tightly held there or forced under the skin of the prepuce, abdomen, or thigh.

Although very inadvisable to use at stud an animal which has only one testicle visible, it is well known that such an animal can procreate his species, but when the testes are definitely in the abdomen such an animal is always sterile; at any rate, up to the present, I have never been able to find a case otherwise. When examined microscopically after removal it is not rare to find spermatozoa in testicles which have remained in the lower part of the inguinal canal, but in those found in the upper part and in those taken from the abdomen itself this is very exceptional. In fourteen instances Professor McFadyean microscopically examined and reported upon testes which I had

personally taken from the abdomen, and spermatozoa were discoverable twice. In eleven taken from the inguinal canal five contained spermatozoa ; three of these were in the extreme upper portion of the canal and all were beyond dispute in such a position that they could be termed "inguinal" testicles. They were quite out of sight even when the patient was chloroformed and cast on its back. This point is worth drawing attention to, and it has been commented upon by Sir John Bland-Sutton in a chapter which he very kindly added to a little book which I brought out on this subject some years ago. He states that "in this book the author furnishes evidence that in horses testes retained in the abdomen or inguinal canal contain spermatozoa. In man this is rarely the case. After careful observations extending over many years I only once found spermatozoa in an undescended testis." Fertility, however, depends upon many things, including the number and state of maturity of the spermatozoa. The actual presence, therefore, of a few spermatozoa in the semen does not necessarily imply power of propagating species. In the horse in particular, it is always a wise plan to convert the animal into a gelding on account of the treacherous disposition which may come on at any time and which is usually an accompaniment of the cryptorchid. He is not only treacherous and uncertain in temper but he is almost invariably a continual nuisance to his owner, endeavouring on every possible occasion to mount any four-footed animal which comes within his range. It is impossible to turn him out to grass as he will never stop in one field, and one never knows when his antics will cause an accident either to himself or to some other animal. For this reason, therefore, it is necessary to perform the operation of castration, and this is briefly described in the following way :

I use the horse for my illustration because this is the animal on which we are called upon to operate most frequently for cryptorchidism. The patient is fasted for about twenty-four hours before the time of operating, water being allowed in limited quantity until some five or six hours prior to the actual event. Not having quite the sterilizing facilities of the human operating theatre we select a good straw bed or as clean a place as possible in a grass field, and our patient is cast and secured by the aid of a rope, chloroform being then administered. The skin is prepared by being cleansed with petrol and painted with tincture of iodine or iodized chloroform. Here I would like to mention that for the past twenty years I have hardly ever washed the skin of an animal before opening the abdomen and I have no cause to regret omitting this procedure, and I speak from an experience of more than 4,000 laparotomies.

A shallow incision about 4 or 5 in. long is made through the skin almost directly over the inguinal canal, care being taken not to injure any of the large inguinal vessels which lie immediately underneath. The inguinal veins are often so intensely varicosed that the inexperienced operator has been known to mistake them for testicles and to cause hæmorrhage which may even be fatal. Once the skin is cut through the tissues are pulled apart and the remainder of the operation of finding the testicle is done by the fingers, without again having recourse to the knife. The hand is then introduced with the fingers in the shape of a wedge and carefully rotated past the large veins into the inguinal canal. If the testicle is present it is grasped and withdrawn, being removed with the écraseur. Sometimes the epididymis alone is in the canal, and the body of the testicle is in the abdomen and cannot be withdrawn without penetrating the wall of the latter. In such a case, if moderate traction is insufficient, the better plan is to enter the abdomen. On several occasions in cryptorchid horses I have found the distance between these two to be as

long as 5 or 6 in., and frequently it has been necessary to use the écraseur twice, first to remove the body of the testicle through the abdominal wound, and secondly to remove the epididymis by way of the inguinal canal. Inexperienced operators have on numerous occasions removed the epididymis alone thinking that this represented the abnormal cryptorchid testicle, and the result has been that in a few months the horse has again become as troublesome as ever. I must admit faults in this direction in years gone by myself and the following instance is typical as an illustration. In July, 1899, when operating on a cryptorchid horse I removed the epididymis only, finding it at the extreme top of the inguinal canal exterior to the abdomen. The colt appeared quite cured of his troublesome habits for about six months, and then became as bad as ever. In the spring of 1901 the animal became quite unmanageable and dangerous, and a further operation was decided upon. On June 15 that year the abdomen was entered and a full-sized flabby testicle, minus the epididymis, was extracted. Recovery was uneventful and the colt became perfectly tractable and quiet. If the testicle is not found in the canal the abdomen is entered through the abdominal muscle, which is penetrated by the aid of the finger nail. Sufficient space is made at first to admit the fore and middle-fingers only, with which search is made for the missing testicle. If found it is withdrawn. If unsuccessful the whole hand is introduced and a careful search made. The anatomical guides, if difficulty is experienced, are the spermatic artery or the vas deferens, as illustrated by the illustrations on the epidiascope taken from Sir John McFadyean's "Anatomy of the Horse." Once the testicle is found and withdrawn it is removed by means of an écraseur or emasculator. In consistency an abdominal testicle is very flabby as compared with those found in the scrotum.

In the dog and cat the site for the operation is in the median line, and as a general rule the testes are readily found floating loose among the intestines. In the bull, pig, dog and cat it is not uncommon to find a misplaced testicle subcutaneously some little distance away from the inguinal ring, a condition which is rarely, if ever, found in the horse. In the horse the position in which the missing testicle is found, if not in the inguinal canal, is usually just within the wall of the abdomen, a short distance from the internal inguinal ring. It may be floating loose amongst the intestines, and it may be close up under the loins.

Abnormalities.—The cryptorchid operator must always be on the look-out for abnormalities, and it is for analogies in these that I particularly ask my confrères engaged in human medicine to speak. In the horse we have a most wonderful variety both in size and characteristics, and this applies not only to the older animals but to the two-year-old or even the yearling colt. Some of the specimens I have here to-night have been taken from quite young animals, one especially which I obtained last week, and which Sir Arthur Keith and Professor Shattock have kindly examined—a cystic dermoid containing both hair and bone was taken from the abdomen of a cart colt only 12 months old (fig. 3). These abnormalities vary from the size of a walnut to the size of an ordinary Rugby football, and contain such foreign bodies as worms, hair, cartilage, osseous or dental structures, and various kinds of tumour tissue. They may be very cystic or very hard and cirrhotic. They may be entirely degenerated and adherent to the peritoneum or to some abdominal organ. One case has been recorded in which the retained tumour was 79 cm. in length, 69 cm. in breadth, and the whole testicle weighed 100 lb. In the dentigerous cyst depicted on the epidiascope the testicle was $6\frac{1}{2}$ in. in length, there was

gland tissue in the centre and a dermoid cyst at either extremity. In the anterior portion there were plates of bone and cartilage, and in the centre of the cavity there was a roundish mass about the size of an orange containing several teeth in various stages of development. One in particular was an almost



FIG. 3.—A typical dermoid from the abdominal testicle of a horse.
For this I am indebted to Professors Williams and Taylor, M.R.C.V.S., formerly of the Edinburgh Veterinary College.

perfect molar, being $2\frac{1}{2}$ in. in length and grooved in the usual way. Six of the other pieces were distinctly recognizable as molar teeth, microscopical examination showing that the three usual constituents, enamel, dentine, and cementum, entered into their composition. The posterior extremity of the

testicle contained a cyst having a wall chiefly composed of bone and cartilage. Inside this cavity were two smaller ones containing a coil of hair, black in colour, and mixed with the débris usually found in these cases. True dermoid cysts are as common as those of the dentigerous variety, and in the one which I now show you there was found when incised five separate cavities, four of which contained hair, in colour black, brown, and grey, and some of them measured 7 in. in length. In the centre of the whole mass there was an irregular bony plate, and the rest appeared to be fibrous tissue (fig. 4).

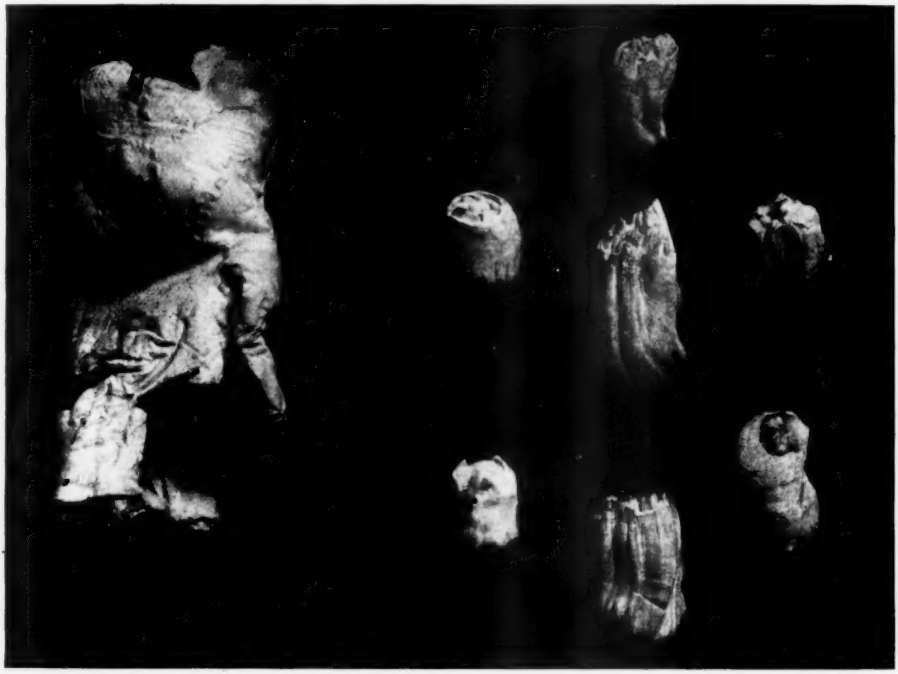


FIG. 4.—Molar teeth (in various stages of development) removed from a dentigerous cyst taken from a cryptorchid horse's abdominal testicle by Mr. Inglis, F.R.C.V.S. On the left is seen the curious misshapen testicle itself.

For this illustration and description I am indebted to Professors Williams and Taylor, M.R.C.V.S. (*Veterinary Journal*, 1901).

Cystic testicles have usually to be reduced in size before they can be withdrawn. This can generally be done with the finger nail, the contents escaping into the abdominal cavity, or a trocar or hollow needle with a rubber tube attached may be used. Dewar and Anderson have reported a case in which a cystic testicle removed from the abdomen of a two-year-old horse weighed 3 lb. 2 oz., and when emptied weighed only 5 oz. 1 dr. It measured over 18 in. in diameter one way and over 16 in. the other. I myself have met with one

which was quite as large as an ordinary Rugby football, and which I was unable to rupture as it kept slipping away from me. It had a cord about 18 in. in length, and this I pulled into view in the form of a loop, severing it with my écraseur and allowing the balloon-shaped body to remain in the abdomen. It was impossible to remove it without making an enormous hole, and this procedure I have adopted on several occasions under similar conditions.

Of the various kinds of tumour tissue, I have had personal experience of sarcoma, embryoma, fibroma, and lipoma, the microscopical opinion being given by experts in every case. The lipoma was exceptionally interesting on account of its rarity, as I understand from Professor Shattock that it was the

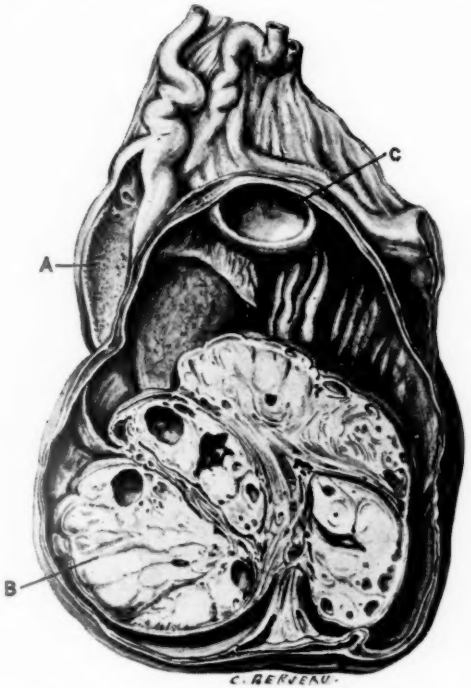


FIG. 5.—Embryoma of the testicle. (A) true testicular tissue; (B) tumour tissue; (C) cyst.

first of its kind which had ever been recorded, and that it has never yet been seen in man. One half of the specimen is in the Royal College of Surgeons Museum, and the other half is here to-day.

The next three illustrations on the epidiascope are typically illustrative of some of the abnormalities met with in the horse by the cryptorchid operator. For the pathological examination and description I am indebted to Sir John Bland-Sutton.

Fig. 5 shows a cystic embryoma removed from a shire colt 3 years of age The left testicle was in the scrotum and weighed 10½ oz., the right one in the

abdomen weighing 25 oz. The latter was as large as a cocoanut. The bulk of the tumour consists of a large cavity filled with fluid, and on the floor there is an embryoma replacing the paradidymis. The same body (A) lying on the wall of the cyst represents the body of the testicle, B is made up of embryonic tissue containing secreting glands, tracks of bone and cartilage. A cystic testicular embryoma of this nature is stated by Sir John Bland-Sutton to be a rarity.

Fig. 6 shows a cystic testicle removed from a shire colt 13 months old. The left testicle was in the inguinal canal and the right in the abdomen. The latter was cystic and weighed 2½ lb. after the fluid had been removed.

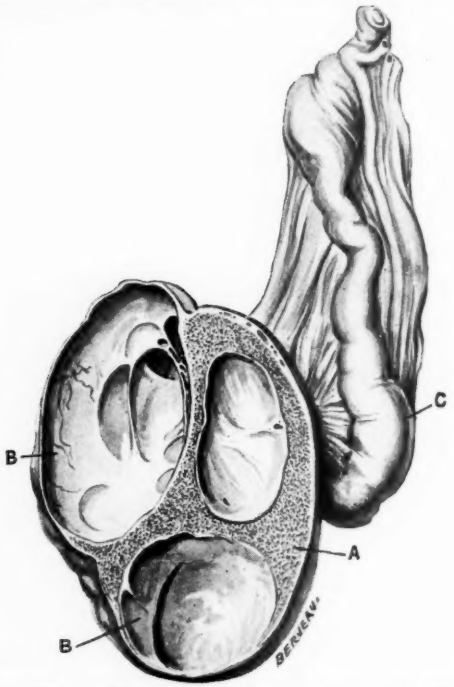


FIG. 6.—Cystic testicle from the abdomen of a horse. (A) testicular tissue; (B, B) cysts; (C) epididymis.

The body of the testis is as large as a turkey's egg and contains three cysts filled with yellow fluid separated by narrow strands of tissue containing seminiferous tubules. This has nothing in common with the condition known as general cystic disease of the testes in man, for in the latter the disease arises in the paradidymis between the body of the testes and epididymis, and although the secreting tissue of the testicle is compressed by the tumour the cysts never invade it.

The third one was from a bay shire colt 2 years old. The right testicle was in the abdomen and weighed 1½ lb. Microscopically the tumour contained the mixed elements of an embryoma.

Another foreign body which is frequently found in the testicle of the cryptorchid horse is the worm *Strongylus edentatus*. It may be in the envelopes of the testicle or it may be in the tissue itself, and I have a specimen here to-night showing the latter condition (fig. 7). In the human subject I understand that living acari, *Histiogaster spermatagus*, even to the number of 800, have been found in the contents of a cyst of the testicle.

I have dealt principally until now with cryptorchidism in the horse and shall only briefly allude to this condition in the other domesticated animals, as, except in the case of the dog, it is not a condition which can be said to be very commonly met with. We do, however, get it in the bull, when the abdominal site of incision for its removal is usually made in the flank. Similarly in the ram, as illustrated in the pictures which you have before you. The pig, too, is opened in the flank, much in the same manner as when operating on the female for ovariotomy. Of the dog, through the kindness

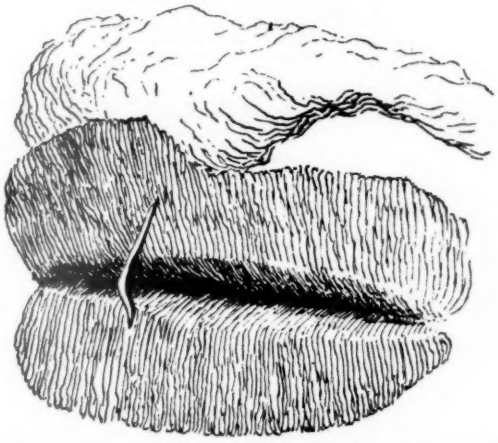


FIG. 7.—Abdominal testicle laid open and showing specimen of *Strongylus edentatus*, *in situ*.

(From a sketch made at the time by Mr. Santy, M.R.C.V.S.)

of Sir John Bland-Sutton, I am able to show you a most peculiar condition of torsion of the cord of a retained testicle.

The question of cryptorchidism in man I have purposely barely touched upon as I know that it is best left in the hands of Mr. McAdam Eccles, who will open the discussion. I read in text-books of human surgery that almost every variety of neoplasm may occur in the human testicle but they are not very common. This you see is somewhat in contradiction to what we find in the horse. I read also that the benign connective tissue tumours (lipoma, fibroma, chondroma and osteoma) are seldom encountered. Sarcomata are fairly common, occurring in children and early adult life: more frequently in undescended testicles than in those normally placed, and generally of the round-celled or spindle-celled variety. I understand that a large proportion of the tumours of the testicle are carcinomata. Dentigerous cysts are not at all infrequently met with in the ovaries of women, but they are comparatively

rare in the testicles of man. The directly opposite condition exists in the domesticated animals, for experience teaches us that dermoids and dentigerous cysts are comparatively common in horses, whilst I have yet to see for the first time this condition in the ovary of the mare, and I speak from an experience of over 1,200 cryptorchid cases in the horse alone and nearly 500 ovariotomies performed on vicious mares. I do not attempt to explain this but I draw

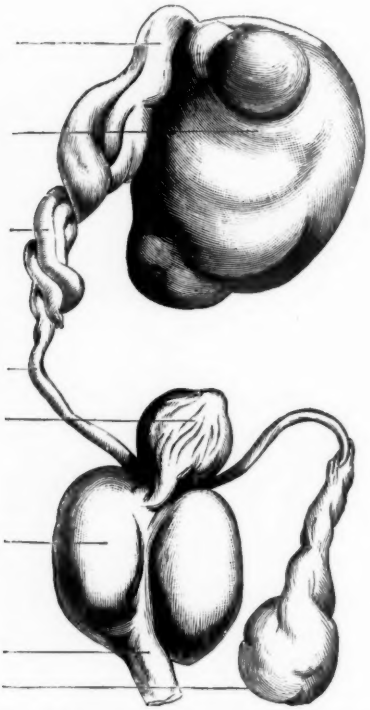


FIG. 8.—Torsion of the cord in a canine cryptorchid testicle (Sir John Bland-Sutton).

particular attention to this fact as a point for discussion, and I again especially emphasize the hereditary tendency of cryptorchidism in animals; as I understand that, although I read that about one in every 500 men has his testicles misplaced, the question of it being hereditary as far as man is concerned is not generally accepted.

The illustrations are taken from the author's "Castration and Ovariotomy of Animals," published by W. & A. K. Johnston, Edinburgh.

DISCUSSION.

Mr. W. McAdam Eccles.

I shall confine my remarks strictly to abdominal retention of the testis in man, and I shall deal with the subject under headings.

(I) *Etiology.*

The ovary remains within the abdomen. The testis migrates from that cavity. Why? The clitoris remains a clitoris. Its homologue in the male becomes the penis. Why? The answer to the latter question in no small degree will answer the former.

The inter-relation influence of the endocrine glands we now know has a profound effect on the formation of secondary sexual characteristics, and if every case of abdominal retention of testis was bilateral, some imperfection of this inter-relation might be a sufficient explanation of the arrest. But internal secretions act everywhere through the blood stream, although it is evident they have a selective action on certain particular organs.

When we have one testis only remaining within the abdomen whilst the other has migrated fully, and has developed fully for its age, we are faced with a much wider problem, and one to which, I confess, I see no present answer.

One may say that the vas, vessels, and other constituents of the spermatic cord are too short to allow the testis to pass beyond the confines of the abdominal cavity, but this does not help us much, for what factor is at work which has prevented the elongation of the spermatic artery on the one side, whilst the artery on the other has its due length? With regard to heredity: I have asked the parent or guardian of every case, or the patient himself, for family history of like condition in grandfather, father, or brothers, and not more than 2 per cent. give it. This small number may be due to the fact that cryptorchidism in an individual is not blazed abroad.

(II) *Function.*

It is a question whether an arrested testis is ever fully developed. I am quite sure it is wrong to speak of these testes as atrophied. An atrophied organ is one which, having been of full size and development, retrogresses. But in the abdominal testis in man there seems to be a better chance of development and spermatogenesis than in the testis which is arrested in the inguinal canal.

There are quite a number of cases on record in which a man with double abdominal retention of testes has procreated children, but there are very few in which double inguinal arrest has permitted of parentage.

Most abdominal testes have the power of producing an internal secretion, but perhaps not of the same type as that of a fully developed and fully migrated organ.

(III) *Morbid Anatomy.*

A testis within the abdomen is liable to all the diseases to which a fully migrated testis is prone.

(i) Inflammation of an abdominal testis may lead to mistakes in diagnosis if care is not taken. Thus, gonorrhœal epididymitis of a testis lying in the right iliac fossa may readily simulate acute appendicitis.

(ii) Cysts of the testis which has remained within the abdomen are rare in the human subject. Simple cysts of the epididymis have been described. "Dermoid" cysts of the ovary (teratomata) are by no means rare, but I have never seen a true "dermoid" of a testis lying in the abdominal cavity. This is an interesting contrast with the horse, in which a dermoid cyst with teeth and other dermal structures is fairly frequently seen, as Mr. Hobday has shown.

(iii) Neoplasms of the abdominally retained testis undoubtedly occur, but it is still open to question whether malignant disease of the testis arrested within the abdomen is proportionately more frequent than that of the fully migrated organ. In the museums but few are found, although there may be numerous examples from the scrotum. Malignant teratoma, carcinoma, sarcoma, and chorion-epithelioma have all been seen, and are all highly malignant, becoming rapidly disseminated.

(iv) Scrotal hydroceles and herniæ may be associated with completely retained testes.

(IV) *Treatment.*

(*a*) In unilateral abdominal retention with a fully migrated testis on the opposite side, the hidden testis may be left alone. As a rule there is no "vicious" tendency in the human to necessitate its removal.

(*b*) In bilateral retention, nothing can be done. It is impossible to bring either testis into the scrotum, which by the way is almost invariably formed ready to receive its proper contents, but has not been distended by them. To remove a pair of abdominal testes would be to remove organs which may have some function through internal secretion.

(*c*) Inflammation or enlargement of an abdominal testis calls for its prompt removal.

Sir JOHN BLAND-SUTTON.

A strange fascination surrounds the descent of the testis, and though it has been the subject of careful study and contemplation from the time of Haller and Hunter to the present day, we know nothing concerning the cause of its transition from the loin to the scrotum. Theory has of course been busy, but there is nothing that surely satisfies.

Hunter made it the subject of very careful study, traced the course of the testis from the loin to the scrotum, and detected the curious muscular bands which are supposed to steer it through the inguinal canal. The notion that the functional imperfection of a retained testis is due to its non-descent is false.

Hunter, in his famous paper, insists that the non-descent of the testis is due to its imperfection, and this opinion stands.

The new knowledge of the power exerted by the internal secretion of ductless glands has not altered the situation, because one testis may enter the inguinal canal, be extruded into the scrotum, and exercise its function to the highest degree, but its companion may be retained in the loin, or does not venture beyond the confines of the inguinal canal. This is sufficient to exclude a hormone supplied to the blood by the secretory action of the testis. The transition of the testis is associated with those ill-understood movements sometimes called the re-adjustment of the peritoneum that occur at a late period of fœtal life.

Migration of the testes is not a mere question of weight or pull; there is

something more subtle, something akin to that curious impulse which causes birds to migrate, eels to transfer themselves from freshwater to the depths of the Atlantic Ocean for spawning purposes, and the extraordinary impulse which compels Palolo worms to leave the water in the reefs around the islands of Samoa and Fiji to seek the sand of the seashore in multitudes on two days in October and November, the days being those in which the moon is in her last quarter.

Cryptorchidism in horses differs from this condition in man on account of the liability of the retained testis to be occupied by a tumour, usually a dermoid. In man a dermoid is rare in a descended testis, but extraordinarily rare in an undescended testis, whereas in horses it is fairly common.

Our knowledge of testicular dermoids dates from 1855, when Verneuil published his classical paper. Thanks to Mr. Hobday's surgical exploits, I have had excellent opportunities of studying such tumours, and can confirm in every particular Verneuil's discovery, that such tumours are not tumours of the testis in the strict sense, but that they arise in the tissue lying in relation with the hilum, and occupying a position between the body of the testis and its epididymis. This holds good for man, and in at least one example published by a trustworthy surgeon, such a dermoid was so slenderly attached that it was removed without damage to the secreting tissues of the testis.

The cause of such tumours, like the source of the impulse of testicular migration, remains an unsolved problem, but I am sure it is one which will repay investigation. Such a study involves the careful histological examination, in equine fœtuses, of the tissues which ultimately become the paradidymis. I can recommend the investigation to some enthusiastic student of embryology.

Professor HOBDAY (in reply)

again emphasized the hereditary tendency of cryptorchidism and insisted on the importance of future investigation of the comparative prevalence of dermoid and dentigerous cysts in the retained testicles of horses as compared with this condition in man. He also referred to the comparative rarity of these cysts in the ovaries of mares as, contrasted with, its frequency in human ovaries.

The cause for these differences required further elucidation and research.

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Section of Comparative Medicine.

President—The Right Hon. Sir CLIFFORD ALLBUTT, P.C., K.C.B., M.D., F.R.S.

DISCUSSION ON NUTRITIONAL DISEASES IN ANIMALS.

Professor E. MELLANBY, M.D.

ALTHOUGH some of us taking part in this discussion to-day have been investigating diseases of nutrition in animals for years past our main object has been to solve the problem of similar diseases in man and of course to throw light, if possible, on to the normal working of the body. The formation by this Society of a Section devoted to the study of Comparative Medicine gives us the opportunity of considering the subject of nutritional diseases in animals from another and probably a more logical standpoint, for we are at once faced with the question: What has recent work, mostly carried out on animals, done to prevent disease and improve the health of animals other than man? A superficial reading of veterinary and agricultural literature would suggest that much of the recent work on the influence of qualitative dietetic changes has not been made full use of by those dealing with the lower animals.

When we have an opportunity for stocktaking—such as is afforded to-day—it must strike anybody interested in the problem of animal nutrition as almost incredible that our point of view as to the relation of nutrition to disease should have changed so much during recent years. It seems but yesterday that we regarded animals and man either as well fed or starved. Starvation was practically synonymous with malnutrition. Now we know that a fat animal is just as likely to be in a state of malnutrition as an underfed animal. But while we have learned much from the investigations of recent years as to the actual production of disease by incorrect feeding as opposed to underfeeding, it would appear probable that we are still only touching the surface of the subject and it is difficult to grasp how important this relationship will ultimately prove to be.

The awakening and development of interest in this aspect of nutrition have resulted partly from the recognition that factors of diet previously regarded as physiological entities, such, for instance, as proteins, are biologically different and, probably to a greater extent, from the pathological and physiological investigations which led up to and proceeded from the discovery of vitamins. It is unnecessary to dwell on this early work except to remind you of the outstanding importance of the original work on the pathological side of Eijkman on beri-beri and that of Holst and Fröhlich on scurvy. In each case the cause of the disease was ultimately tracked down and found to depend on the absence from the diet of a specific vitamin—the antineuritic and the antiscorbutic vitamin respectively. The quality of food now became of prime importance in nutrition.

I do not speak with any intimate knowledge of the subject, but I gather from the literature that scurvy is not a common disease among animals and is found only occasionally among pigs and rarely among dogs. In one well-known veterinary text-book of recent publication, among other forms of treatment for scurvy recommended, I find "grain, mixed with acorns, horse chestnuts, calamus or gentian root, powdered oak bark, &c.," and for dogs, "good fresh meat, bouillon and milk." Anybody who has seen cases of real scurvy in man clear up on giving orange juice or other foods rich in anti-scorbutic vitamin would not have much doubt either as to the main cause or best treatment of the disease.

It seems unlikely that beri-beri or the polyneuritis brought about by diets deficient in water-soluble B vitamin would be common in animals. The wide distribution of this vitamin in foodstuffs and more especially its presence in cereals makes a deficiency in the diet of animals unlikely unless the cereal of the food is converted into a form, such as white flour or polished rice, devoid of the vitamin. For this reason and also because birds appear to require a greater amount of vitamin B, hens are more likely to suffer from this disease. In fact it was the development of symptoms of polyneuritis in his domestic fowls that attracted the attention of Eijkman to the disease and brought about the first successful experimental studies. A deficiency of this vitamin in the diet may also be more important in the case of children, especially those eating certain patent foods, than is usually thought.

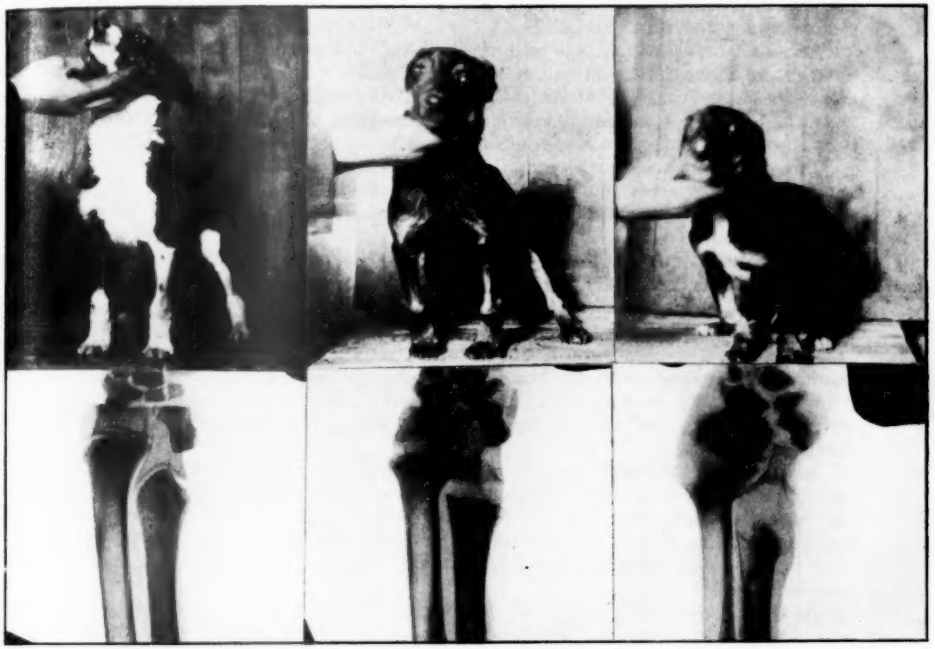
I wish now to confine my attention to an aspect of nutrition which I feel will ultimately prove of importance, more especially as regards the feeding of animals. I refer to the large part played in dietary by cereals.[1] I wish to provide evidence that cereals are capable of producing abnormality both in the structure and the functions of tissues, that some cereals are worse than others in this respect, and that their baneful effect can be entirely antagonized by other factors of the diet and environment. It is true that evidence of these facts has been mostly obtained by experiments on puppies, and, therefore, it is possible, but not likely, that the facts may only concern dogs and cannot be extended to other animals, especially to herbivorous animals. In the absence of evidence proving or disproving that the facts can be so extended, I shall assume that they are of general application to carniverous, omnivorous and herbivorous animals.

If puppies are fed on diets which are very good except for a deficiency in fat-soluble vitamin, then the growing bones tend to become badly calcified and soft and the animals develop rickets. The severity of the rachitic changes and the resulting bone deformity vary with the amount of cereal in the diet. If, for instance, the basal diet consists of definite amounts of separated milk, meat, olive oil, orange juice and yeast, and bread be added in different amounts such as 50 grm., 100 grm. and 150 grm. daily in the case of three puppies, then the animal eating 150 grm. will get worse rickets than the one eating 100 grm., and this, again, worse rickets than the one eating 50 grm. Thus we see that white flour tends to produce defective bone calcification and other associated changes when the diet is deficient in fat-soluble vitamin—a vitamin abundantly found in cod-liver oil and other fish fats, milk, eggs (yolk), butter, suet, and green vegetables, but deficient in most vegetable fats.

It might be suggested that white flour has this action specially developed because in preparation it has lost its pericarp and germ and with these most of its calcium and phosphorus, and that hard bones would more likely be produced

[1] Detailed evidence of the new facts described in this paper, with the radiographs, &c., shown at the meeting, is now being prepared for publication.

by cereals containing an abundance of these substances, since this hardness depends on the deposition of calcium and phosphorus in the form of calcium phosphate. If this suggestion be true then cereals containing more calcium and phosphorus ought to have smaller rickets-producing effects than white flour. But this is not the case.



A B C

Photographs and radiographs of wrists of three puppies, A, B, C, of same litter taken after thirteen weeks of experimental diets.

General diet eaten daily by all included 200 c.c. separated milk, 10 grm. meat, 5 c.c. orange juice and 5 grm. yeast.

In addition A received 50-100 grm. oatmeal (cooked into porridge) and 3 c.c. cod-liver oil and 7 c.c. olive oil. B received 50-100 grm. white flour (cooked in same way as oatmeal) and 10 c.c. olive oil. C received 50-100 grm. oatmeal and 10 c.c. olive oil.

The 3 c.c. of cod-liver oil (A) has almost completely antagonized the oatmeal effect seen in C.

The rickets in B (white flour) is not nearly so advanced as in C (oatmeal).

Suppose we feed a litter of puppies about 5 to 6 weeks old, and give to each the basal diet of separated milk, meat, olive oil, yeast and orange juice and sodium chloride, and, in addition, to one oatmeal, and to the others barley, rye, rice and white flour respectively, all in equal quantities and equally cooked, then the intensity in rickets will greatly vary. The puppy getting the oatmeal will be the worst, and that receiving the white flour the most

normal of the litter, while the remaining puppies eating barley, rye and rice will be in a state of abnormality intermediate between the oatmeal and the white-flour puppies. This relative effect of white flour and oatmeal is well seen in figs. B and C. Thus of all the cereals so far tested, oatmeal has been found to be the most rickets-producing under these experimental conditions. Nor is this action of oatmeal to be explained by its being a manufactured product, for crushed oats and groats also produce most severe rickets under the same conditions.

Another surprising fact has been derived from these experiments. Suppose diets of the above described nature have in one case white flour as the cereal, and in the second case white flour and wheat germ (20 per cent. of the white flour), it will be found that the puppy taking the wheat germ will get the worse rickets, that is to say, wheat germ also contains a substance potently rickets-producing.

Two questions arise concerning these results with cereals: (1) What is the explanation of this variable toxic effect of cereals? (2) How can the effect be antagonized by other food stuffs and conditions?

No definite answer can be given to question (1), but it is important to recognize that some of the most obvious possibilities do not explain the facts. In the first place oatmeal and wheat germ both contain much more calcium and phosphorus than white flour, so that their greater rickets-producing effect cannot be due to an absolute deficiency of either of these elements. Nor is it possible to explain their action on the basis of a *relative* unbalance of calcium and phosphorus, for the Ca : P ratio in oatmeal, wheat germ and white flour is respectively 1 : 5·7, 1 : 14·8, and 1 : 4·6. The complete irregularity in this ratio does not suggest that a feasible explanation can be obtained along these lines.

It used to be thought by some clinicians that carbohydrate had a definite rickets-producing effect, but, although there is undoubtedly some truth in this, the variable carbohydrate content of different cereals cannot explain the varying rickets-producing effect, for oatmeal, the worst offender contains only 67 per cent. starch as compared with the 75 per cent. of this substance in the more innocuous white flour.

Another important constituent of both oatmeal and wheat germ, which it appeared possible might be the offending factor, is nucleic acid. I have not up to the present obtained any good evidence that nucleic acid is the element responsible for these pathological changes. Thus it will be seen that none of the above suggestions explains the variable action of the cereals, and for the moment I can only suggest that there is something toxic and more especially abundant in oatmeal and wheat germ, and that rice and white flour contain much smaller quantities of this toxic agent.[1]

I shall now demonstrate by means of radiographs that this action of cereals can be profoundly influenced by several conditions. First, let us see how simple salts containing calcium and phosphorus influence it.

Salts.

Sodium acid phosphate, in quantities up to 1 grm. per diem, added to the diet described above, with oatmeal as the cereal and deficient in fat soluble vitamin, has practically no effect.

Calcium phosphate has a slightly beneficial effect under the same conditions.

[1] It may be added that evidence of a toxic agent in wheat germ has previously been obtained by McCollum, Simmonds and Pitz (*Journ. Biol. Chem.*, 1916, xxv, p. 105). No evidence of this toxicity was found, however, by Osborne and Mendel in rat-feeding experiments (*Journ. Biol. Chem.*, 1919, xxxvii. p. 557.)

Calcium carbonate (up to 1 grm. daily) has a greater beneficial effect than calcium phosphate; if the puppy is of small breed and eats but little oatmeal, the calcium carbonate effect may result in the formation of fairly good bones.

The rickets-producing effect of cereals may be completely antagonized by fat-soluble vitamin.

Fat-soluble Vitamin.

(*a*) *In Food.*—The worst oatmeal effect can be completely prevented by adding small quantities of cod-liver oil containing fat-soluble vitamin. Thus in the puppy A (see fig.) had received 3 c.c. of cod-liver oil and 7 c.c. of olive oil daily (in addition to the usual basal substances) as compared with the puppy C, which had recived 10 c.c. of olive oil. The tremendously potent effect of the anti-rachitic vitamin of 3 c.c. of cod-liver oil daily is evident in the greatly improved calcification of the bone of A as compared with C.

(*b*) *Ultra-violet Rays* (*Sunlight*) *Mobilizing the Vitamin.*—Recent evidence shows that probably ultra-violet rays either of the sunlight or from a special lamp (mercury vapour-lamp in this case) striking the skin activate the fat-soluble vitamin present in the body and so bring about the same increase in activity of certain functions as results from ingestion and absorption of food-stuffs containing this vitamin.[2] Thus it is now known, as the result of the observations of Huldischinsky and others, that ultra-violet rays, striking the skin, bring about calcification of bone and so tend to prevent or cure rickets, just as does the administration by mouth of cod-liver oil, egg yolk, milk or other sources of antirachitic vitamin. Cereals in the diet and exposure to ultra-violet light have, however, antagonistic effects on some physiological processes including bone calcification. When the diet is deficient in fat-soluble vitamin and contains much oatmeal, the effect of the ultra-violet rays is small, but as the cereal is reduced the rays become more effective and the animals exposed to them tend to be more nearly normal. It has been pointed out by others how fat-soluble vitamin and sunlight can be substituted for each other up to a certain point. I wish to emphasize that it can be equally well demonstrated that cereal and sunlight antagonize each other. In my experience diet is the dominant factor, but the action of dietetic substances can be aided or interfered with by exposure to the ultra-violet rays.

So far I have illustrated my remarks by photographs showing the intention of nutritional factors in so far as they affect bone calcification. It would be wrong, however, if I gave the impression that it is only the calcification and growth of bone that is influenced by these potent interacting food factors. May Mellanby has, for instance, shown the effect of diet on the structure of the teeth, and their arrangement in the jaws. I shall only refer to one small part of her work and show you that the structure and calcification of the teeth and the jaws is also greatly influenced by the calcifying vitamin, and in the opposite sense by cereals. In this case also the more cereal eaten the worse the teeth and, among cereals themselves, oatmeal produces the worst teeth; wheat germ also tends to produce badly-formed teeth.

Recently Clifford, Surie, and myself have demonstrated to the Physiological Society that the structure and formation of the voluntary muscles can also be greatly influenced by the same dietetic factors. Thus it is easy in the case of growing puppies to produce all degrees of activity and lethargy, from extreme restlessness and great running speed down to absolute paralysis, by small variations in the diet. On the whole the muscle changes, which can be

[1] Hume, *Lancet*, 1922, ii, p. 1318; Goldblatt and Soames, *ibid.*, p. 1321.

[2] *Deutsch. Med. Woch.*, 1919, xlv, p. 712.

detected microscopically and by chemical analysis as well as by the effect on their gross contracting power obvious in the living animal, run *pari passu* with the bone changes, so that defective bone calcification goes with weak muscles and perfect bone formation with muscles of great strength and contractile power. Such a relationship would of course be expceted on teleological grounds for it ensures that powerful muscles should have correspondingly strong bony support.

No doubt other systems of the body are as profoundly influenced as those I have mentioned by diet but I think I have said enough to show that some of the ordinary articles of diet of man and animals have a great effect on bodily structure and function. The point I wish to emphasize to-day is the detrimental effects produced by over-feeding with cereals and cereal products and the way by which their bad effects can be antagonized.

If we assume that these results can in a general way be extended to herbivorous and other animals such as horses, cattle, pigs, &c., we must remember the distribution of the vitamin elements in the foodstuffs specially eaten by these animals. Green stuff is not only a source of fat-soluble vitamin but is considered to be the original source of this substance in animals. So it is probable that any action exerted by certain fats and other substances known to contain this vitamin which have been tested in the puppy experiments would also be exerted by green stuff including clover, lucerne and grass in the case of herbivorous animals. We have, as a matter of fact, tested the antirachitic action of cabbage (chopped and steamed) in the case of puppies, and it can be definitely stated that it does antagonize the cereal effect described above to some extent. The alimentary canal of dogs is not, however, suited for testing and observing the best effects of vegetables on nutrition and no doubt the action would be even more potent in the herbivora.

Such profound pathological conditions as rickets and osteomalacia found in pigs, sheep and horses (and possibly licking disease of cattle, associated with bony changes) have no doubt the same ætiology or a nearly allied ætiology to these diseases in puppies and human beings—an ætiology implied in my previous remarks. But these gross changes, except in pigs, seem to be relatively rare. What must be much more common are the slighter changes affecting bone, muscle and no doubt other organs, which follow on the dietetic errors indicated. I suspect that some of the lameness and muscular weakness in horses could be traced to the same cause, i.e., actual or relative overfeeding with cereals, especially oats and cereal products, and deficiency or absence from the diet of hay, clover, lucerne and other sources of the fat-soluble vitamin. If, in addition to a diet containing excessive cereal, there is confinement in dark stables, stalls or styes with no access to light, it can be readily understood how, on the basis of the facts described above, all manner of morbid conditions may arise. Parenthetically I may add that I have often thought that the diet of race-horses in training might be expected to ensure the losing rather than the winning of races, and the breakdown in health of the animals so commonly experienced is not surprising. In the treatment of lameness in horses by rest and turning out to grass, I think it probable that the curative results are not so dependent on the rest as on the larger amount of green-stuff and diminution in cereal and cereal products eaten under these conditions. Combined with this dietetic change the possibility of the animals benefiting by the sunshine may also be of importance. The suggestion that some of the pathological muscular and bone changes in animals are due to wrong dieting such as I have described is also supported by the condition known as "miller's lameness," a disease recognized as developing in horses fed freely on wheat offal. The

experimental results on wheat germ indicate the possibility of a development of this nature. The wheat germ effect is of special interest because this substance is known to contain a large amount of vitamin B as well as some vitamin A. For this reason the more progressive agriculturists and stock-raisers might advocate its value for feeding purposes. If so, it must be remembered that the germ also contains a substance or substances capable of bringing about pathological changes and that if it forms a large part of the diet, special precautions, such as supplying additional chalk and greenstuff, must be taken to antagonize the toxic action of the germ constituent.

For an opening address on the subject of "Nutritional Diseases in Animals" my remarks may be considered too narrow in their scope. There is, for instance, one wide aspect of the subject which I have failed to mention, although it will, no doubt, prove ultimately to be of supreme importance. I refer to the effect of diet on resistance to infection. From my own experiences with experimental animals, in which I have seen the much greater incidence and virulence of pneumonia and other lung troubles, of distemper, of mange and other skin diseases in animals on diets showing qualitative defects as compared with those on physiologically better diets, I am sure that this subject requires investigation. I hope that others taking part in this discussion will speak at greater length on this important problem.

In conclusion, the points I wish to emphasize are that diets containing too much cereal are capable of bringing about widespread and severe pathological abnormalities; that these changes, while produced to some extent by all cereals tested, are more particularly called forth by oats and oatmeal and wheat germ (and possibly by the germ of other cereals); that the toxic effect of cereals may be completely antagonized by fat-soluble vitamin contained in cod-liver oil, milk, eggs, &c., and that in the case of herbivorous animals a similar vitamin in grass, hay, clover, lucerne, cabbage and lettuce, &c., probably exerts the same beneficial action; that the cereal effect can be antagonized to some extent by calcium salts, especially calcium carbonate, and that out-of-door existence involving exposure to sunlight and sources of ultra-violet rays by mobilizing the fat-soluble vitamin stores in the body also helps to neutralize the toxic cereal effect.

Dr. Harriette Chick.

One of the most important questions which arises in considering such laboratory experiments as those just described by Professor Mellanby is whether, and to what extent, the results obtained with one animal are applicable to the case of other animals and to man.

Ten years ago the supposed existence of vitamins was based upon two, apparently isolated, sets of facts. First, the researches of the Dutch investigators, Eijkman and Grijns, showed that an exclusive diet of polished rice (i.e., deficient in vitamin B) when fed to birds, would cause a condition of polyneuritis presenting a close analogy with human beri-beri, a disease also confined usually to rice-eaters. In the second place Holst and Fröhlich, in Christiania, showed that by depriving guinea-pigs of the fresh element in their diet (vitamin C) a disease could be induced, which might be regarded as the physiological equivalent of human scurvy. The conclusions of these workers were subjected to much discussion as to whether these animal diseases were really ætiologically similar to the human disorders.

The importance of the *fat-soluble vitamins* (vitamin A and the antirachitic vitamin) was later discovered by McCollum and his colleagues working with rats, and by Mellanby working with dogs.

There was thus a schedule involving three or more vitamins and a series of corresponding "deficiency diseases," but evidence was lacking that all or any were important for any particular animal. The evidence has come piecemeal. With the lapse of time different animals have been shown to present more points of similarity with each other in this respect and with man, than was at first apparent.

Carefully conducted experiments showed that deprivation of vitamin B would cause paralysis in rats as well as in fowls and pigeons. Monkeys as well as guinea-pigs were found to develop the characteristic hæmorrhages and bone lesions of scurvy if fresh food (vitamin C) were eliminated from the diet. In rabbits on a diet devoid of vitamin C, the characteristic pathological changes can be detected in the bony tissues, although there is an absence of hæmorrhages, and while rats appear to be entirely indifferent to the presence or absence of vitamin C in their food, it has yet been found impossible to breed them satisfactorily upon diets from which this vitamin is absent.

In the further study of the fat-soluble vitamins (vitamin A, antirachitic vitamin) further research has also brought a whole series of animals into line with each other. The ophthalmia first described by McCollum and his colleagues, in rats upon diets deficient in vitamin A has also been observed in poultry (Emmett and Peacock) and in the human infant (Bloch). Defective calcification of bone in the young growing animal has been found associated with lack of fat-soluble vitamins in puppies (Mellanby), in rats (McCollum and his co-workers, Korenchevsky and others), in poultry (Emmett and Peacock) and in children. And, further, the action of light in correcting a deficiency of these vitamins in the food has been demonstrated in all these cases.

One other aspect of this subject is also illuminative. The varying susceptibility of different animals to lack of these vitamins makes it impossible to argue from one animal to another as regards the necessary requirements. But the *relative* values of different food stuffs in respect of one or other vitamin, as determined by quantitative experiment upon any one animal, will also be applicable to another.

An instructive instance of this is the case of human beri-beri, where the classical instance is found among persons receiving a diet consisting too exclusively of polished rice. It was soon apparent from experimental work with birds that this could only be a special case of a general rule, and that beri-beri might be expected to occur upon a diet consisting almost exclusively of any "overmilled" cereal, especially if tinned foods should form a large part of the rest of the diet (Chick and Hume). Practical instances of this became common in the late war. Our soldiers, living largely on tinned meats and bread baked from white wheaten flour, developed beri-beri in the Near East and in Mesopotamia. In the siege of Kut, beri-beri was confined to the British troops and disappeared when their white flour ration was exhausted and they were obliged to eat bread baked from barley flour or from the coarsely milled wheat of the Indian troops.

Another example is afforded by the relative value of the juice of limes and lemons for the prevention of scurvy. One of the more striking results obtained in the experimental study of scurvy was the low value of lime juice for prevention and cure of the guinea-pig disease. The inferiority compared with lemon juice was demonstrated in case of both the fresh and preserved juices. The efficiency of "lime juice" for prevention of human scurvy was so strong a tradition that this experimental result was only accepted after repeated confirmation (Chick and Hume). A historical inquiry made by Mrs. Henderson

Smith into the use of these juices in the Navy and Mercantile Marine has, however, completely confirmed the experimental results. When scurvy was eliminated from the Navy by the introduction of "lime juice" at the beginning of the nineteenth century the juice used was that of *lemons* from the Mediterranean. The juice of the West Indian *lime* was adopted for national and economic reasons only in about 1860, and since that date the introduction of steam traction and cold storage and the shortening of voyages has relieved the strain which the long periods at sea had previously put upon the ship's dietary. Instances, however, did occur in which *lime* juice was trusted to avert scurvy, as, for example, in the Arctic Expedition of 1875, and experience showed that it proved worthless in comparison with lemon juice. The Expeditions of 1850 and 1853, in search of Sir John Franklin, were equipped with *lemon* juice and escaped scurvy under conditions far more severe than those encountered by the expedition of 1875.

Mr. Henry Gray

said that Professor Mellanby had brought a most interesting subject for discussion before the Section. He did not, however, consider that any reasonable person would feed his dog on a diet like that employed by Professor Mellanby for his dogs, to induce rickets. Practical agriculturists and stockbreeders followed a system that had been handed down to them from time almost immemorial, and that was the reason one rarely witnessed nutritional diseases in their young animals. The food they gave their animals was generally grown on the soil on which they bred and reared their stock. But more recently there had arisen a class of amateur farmers or stock-raisers who did not possess that traditional knowledge, and they were induced on the grounds of economy to feed their young stock on all kinds of by-products, which generally lacked those elements necessary to build up the framework of the young animal and to maintain it in a state of health. It was, therefore, in the livestock of that class of stockbreeder that nutritional diseases principally came under observation. It was the same with dogs of amateur breeders.

In the case of puppies of the large breeds, it did not matter how well they were fed; rickets would be induced in them unless they were allowed free exercise. Continual confinement in a small space or on the chain was a fruitful source of its incidence.

Blaine, a veterinary surgeon, said, as far back as 1817, that rickets in dogs was due to lack of good food, fresh air and free exercise; if these were provided rickets could be prevented, and if present, cured. In later years he added heredity as a factor, but probably he had in this respect confused bassetism with rickets.

Though fresh air, daylight and sunlight were beneficial to health, they would not by themselves prevent rickets nor osteomalacia, nor bring about a recovery. He was certain of that in the case of dogs. He had known ponies that had been in coal-mines for twenty years without seeing daylight or receiving direct fresh air since they descended; and yet those animals had remained in perfect health and were in excellent condition.

Food grown on the soil in the locality in which animals were born and reared, played some part in the proper nutrition of such animals. French army veterinary surgeons found that when their horses were taken from Algeria and Europe to Cochin-China, they did not thrive on the native-grown fodder and grain. They developed osteomalacia. It was the same with their

mules imported from Egypt, Syria, &c., into Madagascar. To prevent this trouble they had to import fodder and grain from the countries from which the animals were obtained or reared.

Sheep and cattle frequently developed an enzoötic form of osteomalacia when living in the open and grazing on living grass and other plants. It was seen more in the sunny or during dry months of the year, and again, in certain years and not in others. It had been observed in every continent and in various insular countries. It had also been observed in sheep and cattle in Great Britain grazed on the luxuriant pasture of reclaimed moorland. Before the moorland was reclaimed the animals grazed on the sparse herbage did not develop the disease. Chemical analysis of the soil and plants ruled out the lack of lime theory. The incidence of disease could not very well be put down to lack of vitamins in the living plants.

If hereditary predisposition in certain breeds was admitted, heredity must therefore be included as a factor in its incidence. There was a hereditary imperfect osteogenesis which had been persisting for over 2,000 years in such Oriental breeds as the Pekingese and Japanese spaniels, their crosses and their descendants. In a very great number of instances the fontanelles remained open, and in some instances the bony covering of the upper part of the cranium was either absent or else translucent and very thin. There was no development or very slight development of the sagittal crest. The teeth were always fewer in number and irregularly placed in the dental arcade. The alveoli were imperfectly developed and the teeth seemed embedded in cartilage. The orbital fossa was so shallow that the eyeball could not be retracted. The cornea was predisposed to degenerative changes. This took place even when these breeds were being fed on a diet similar to that of the more robust or recent breeds.

Exercise or work had a very beneficial influence on nutrition. Horses worked moderately and regularly thrived much better and maintained a superior condition than when worked irregularly or kept in idleness on a superabundance of highly nutritious food. Bone disease, so frequent in horses, was mostly due to overwork or repeated fatigue. Hereditary predisposition also played a very great part in its incidence. But there was no doubt forcing young Shire horses for size and exhibition purposes without giving them a corresponding amount of exercise or work predisposed them to develop a very porous or brittle condition of bone and increased their liability to bone disease should they be put to hard work later on in life. As a rule farm horses were put to work on the land at two years old and they were kept to it until they were four, five, or six years old, when most of them went to work in towns. Such horses were less liable to develop bony troubles than their more luxuriously maintained fellows. There was not the least doubt that exercise or work within moderate limits was beneficial to muscular and bony development in young animals: it aided in general nutrition.

Colonel R. McCarrison

said there was a general impression that the polyneuritis which was produced by feeding pigeons on polished rice was the same condition as beri-beri. In his opinion this was not the case, for beri-beri in man was characterized by cardiac enlargement, which was as important as the polyneuritic symptoms: whereas in polyneuritis columbarum there was atrophy of the heart. He had recently seen in his laboratory a number of

cases of a beri-beri-like condition in pigeons and in these also there was enlargement of the heart with degenerative changes in its musculature. In both pigeons and man beri-beri was associated with insufficiency of vitamin B rather than with complete want of this factor. While insufficiency of this vitamin was at the basis of true beri-beri, he considered that some other factor was responsible for the heart condition.

Rice was largely grown and eaten over a wide extent of India, yet the vast preponderance of cases of beri-beri occurred in a very limited area of the North-East Coast Division of the Madras Presidency where the malady was endemic. Of 565 cases of beri-beri which occurred in the Indian Army between the years 1900 to 1914, no less than 492 were in men recruited from Madras. Long residence in an endemic locality seemed to impart some stigma to the individual which rendered him liable to the disease.

With reference to the influence of deficient dietaries on muscle, to which Professor Mellanby had referred, he emphasized the importance of such changes in the musculature of the gastro-intestinal tract and attributed the great prevalence of gastro-intestinal disease to faulty food, which also predisposed to infection, and was probably an influence in promoting the occurrence of new growth. In the last connexion, he had observed in pigeons that diets deficient in vitamin B and ill-balanced in other respects, favoured the occurrence of epithelioma contagiosum, that is to say, favoured the operation of an invisible virus which possessed the specific property of causing new growth of epithelial tissue.

Mr. W. Hamilton Kirk

alluded to the question of the internal secretions in connexion with the ætiology of rickets. He said that in view of the fact that one puppy only, in a litter, might be found suffering from rickets, though all were fed and exercised in precisely the same way, it seemed that the cause must be sought in the individual, and less importance attached to external influences. He thought such a fact rather upset the "toxicity-of-foodstuffs" theory. Some foods might in fact have a disease-producing tendency, but where many animals received the same diet and only one developed the condition, how were we to reconcile the two facts? Was it not feasible that a deficiency in one or other of the hormones might be the factor which prevented the body from properly utilizing or assimilating the essential elements of its food? Some of these elements were the various salts, such as lime, and whilst there might be a deficiency of lime salts in the bones, there was often a concurrent abundance of lime in the body—as evidenced by the amount excreted. Was it not a question of inability to assimilate the lime? It was well known that some of the ductless glands were intimately concerned with bony growth, the pituitary and thymus for instance; and whilst most speakers had agreed that the ætiology of rickets was as yet unknown, it seemed in his (Mr. Kirk's) opinion that further investigation of this field might result in considerable enlightenment.

So far as dogs were concerned (speaking from the practitioner's point of view) such foods as rye, oats, and wheat were never fed to dogs. They were essentially carnivorous, and, after weaning, were reared mostly on meat. The toxic product from these cereals, then, would again be ruled out as a cause of rickets occurring in every-day canine practice. Rickets might be induced through their agency in the laboratory, but what of private practice? Cod-

liver oil had long been recognized as a curative remedy by his profession, and for over seventy years veterinary surgeons had used and recommended it. Lastly, he would mention another curious fact relating to nutrition. Horses which were regularly worked and received a working diet, commonly contracted azoturia after brief periods of rest (such as over Bank Holidays, &c.), if their working ration of corn were persisted in. The reason for this had always been obscure, and he (Mr. Kirk) wondered whether anyone present could offer a possible explanation.

Section of Comparative Medicine.

President—The Right Hon. Sir CLIFFORD ALLBUTT, P.C., K.C.B., M.D., F.R.S.

The Pig and Human Disease.

By THOMAS W. M. CAMERON, M.A., B.Sc., Ph.D., M.R.C.V.S.

(Department of Helminthology, London School of Tropical Medicine.)

THE pig, as an associate of the human community, has, since the days of Elia's Chinaman, been regarded merely as a potential article of diet and not seriously considered until recently as a disseminator of disease to man. There exists, however, a formidable number of diseases common to the pig and his master.

In spite of his undeserved reputation as a lover of filth and dirt, he is very frequently given ample opportunity of contaminating food and water. In some of the less civilized of our own rural districts he is allowed the free run of the cottage or kept in a state of indescribable filth in close proximity to the house.

Among the domesticated animals, after cattle, pigs are most frequently affected with *Tuberculosis*, which seems to be usually contracted from cows' milk and is accordingly generally of the bovine type. However, both human and avian strains are sometimes found; but these are more frequently localized than the bovine form, which rapidly becomes generalized in pigs and often attacks the bones. Unless a strict system of meat inspection is enforced, there is a considerable danger, not only from uncooked and undercooked pork, but from contamination of other articles of food by the pork. It is now realized that even the bovine strain is very dangerous to children, and it is claimed that about half of the cases of primary abdominal tuberculosis in children are due to this form. Meat, of course, does not lend itself with the same facility to the transmission of the disease as does milk; but its possibilities in this respect are by no means negligible.

Aphthous fever, or foot-and-mouth disease, is almost as serious a condition in pigs as in cattle, but as the disease in man is dangerous only in children, to whom it is conveyed by cows' milk, it is probable that the pig is not an important factor in its transmission to human beings. The fact, however, must be borne in mind that man may be infected through handling the animals.

Variola suilla, or swine pox, is a rare, though serious, disease in pigs, said to be contracted from, and transmissible to, man. It is very contagious and causes fairly high mortality, but it must not be confused with swine erysipelas, which is a distinct disease (in no way related to human erysipelas). Swine pox occurs in Europe and is said to have been present in England.

Swine erysipelas is a contagious disease in pigs, which is widely distributed throughout this country. It is due to the *Bacillus rhusiopathiæ*—an organism closely allied to or identical with the bacillus of mouse septicæmia. A number of cases are on record of men who were engaged in handling infected pigs contracting the disease—one of the inspectors of the Ministry of Agriculture has been infected on three occasions in this way. It is not definitely known whether the bacillus may enter the unbroken skin or whether slight abrasions are necessary. There is a slight but very painful cellulitis with a distinct line of demarcation. The neighbouring lymph glands are usually swollen and there may be exfoliation of the epidermis. The disease usually terminates within a month. In the pig, there are frequently serious heart lesions but it is unknown if such effects are produced in man.

Anthrax, primarily a disease of herbivores, is also found in swine, although these animals are relatively insusceptible.

Rabies also occurs in pigs, although by no means so commonly as in carnivores, and when one considers the large canine teeth and the pugnacious disposition of the boar, it must be counted as a serious danger to man and other animals.

Actinomycosis is also found in this animal but there is no authentic evidence that this disease has ever been transmitted from animals to man.

While the living pig can transmit many bacterial diseases, its flesh may also be a serious danger. Pork and beef are the most common sources of meat poisoning. Meat may be rendered poisonous in several ways—through contamination by the butcher from fæcal material, by the post-mortem or ante-mortem invasion of the tissue by bacteria, or by infection subsequent to its leaving the hands of the butcher. It is important to remember that many organisms of the coli-typhoid group found in man are normally present in the intestine of the pig, and that in the very common disease of swine fever, one of these organisms (*Bacillus suipestifer*) is so frequently present in the blood and tissues that it is called the "swine fever bacillus." It is now accepted that the cause of this disease is a filterable virus, and that the bacterium is a secondary invader. If the pigs in the early pre-clinical stages of the disease are slaughtered, the bacillus may be already in the tissues and it is at this stage that the animals are frequently killed in order to minimize financial loss. The meat-poisoning bacilli are generally accepted to be *Bacillus enteritidis gaertner* and *aertrycke*, the latter being probably identical with *Bacillus suipestifer*. *Bacillus paratyphosus* is believed not to be a meat-poisoning bacillus. Meat poisoning in Europe, due to *Bacillus aertrycke* is more frequently associated with the consumption of raw or partly raw pork, than with any other form of meat.

In addition to ante-mortem infection of tissue, it is easily seen that pork may be infected by the butcher with fæcal material, and the practice of not skinning the animal before cooking, so common in this country, increases the danger. If the viscera are left too long in the carcase, especially in hot weather, the danger of meat being infected through migrations from the intestine is obvious.

In most cases of meat poisoning the meat has been obtained by emergency slaughter, i.e., the animal suffering from an untreatable disease is killed in order to save as much as possible. This practice must always be regarded with the deepest suspicion, but must not be confused with slaughter because of accidents, which is on account of a recent and afebrile condition and belongs to a different category. Strict supervision by skilled meat inspectors has done

much to prevent trouble from this source in Britain, but there is considerable slackness in some districts.

Bacillus botulinus is the cause of a more serious form of meat poisoning. It is an anaërobe and causes a profound systemic toxæmia rather than a gastro-enteric irritation. Pigs are naturally very resistant to botulism, but are by no means immune to it. The organism has been isolated on several occasions from the intestine of the normal pig and elsewhere; and for this reason pork, including ham, is an important, although by no means the sole, source of the disease in man. It is more frequently associated with preserved pork than with the fresh material, and sausages, salted pork and ham are peculiarly fitted to act as culture media for the bacillus which produces a very powerful toxin. It is this which is so dangerous to the consumer. Tinned pork and tinned sausages may also be infected, and though "sterilized" at a temperature sufficiently high to kill the bacilli, this temperature may not have been high enough to destroy the toxin.

The gas gangrene group of organisms are well represented in the intestine of the pig, as in all the domestic animals; and although little attention has been paid to their effect on this animal, they appear to exert but slight pathogenicity. It is not, of course, suggested that man is liable to be infected by the pig, but their occurrence is of interest.

On the ground of the animal parasites, an even more formidable indictment may be drawn up.

Balantidium coli is a normally non-pathogenic ciliate commonly found in the intestine of the pig, and is the cause of a serious form of dysentery in man. The parasite is excreted in its cystic form and ingested by man with food or water contaminated by pig fæces. Apparently cysts are not excreted by man, who is therefore incapable of spreading the disease, which must in every case originate with the pig.

Among the ecto-parasites of the pig transmissible to man is the porcine variety of *Sarcoptes scabiei*, a parasite which is more common abroad than in this country. The pig is also an occasional host of *Pulex irritans*, the human flea; but so far as man is concerned, its most formidable ecto-parasite is *Tunga penetrans*, the "jigger," a flea producing sores in the feet and legs of man in the tropics. The parasite is normally found on pigs, but occurs on other animals as well. The female of the species, after fertilization, seeks the skin of an animal, burrows into it, and there becomes embedded in an inflammatory pocket with the posterior region of her abdomen projecting. She swells to the size of a pea, and passes numerous eggs to the exterior. The inflammation may lead to ulceration or even gangrene and loss of the affected part. When all the eggs are deposited the female dies and shrivels up. Control can at best be incomplete when man alone is treated.

It is altogether outside the scope of this paper to make a detailed study of all the entomological questions which concern man and the pig. There are two points, however, which are important from our point of view. In the first place, the pig, in common with the domestic herbivores, may sometimes be very useful as a counter-attraction for insects, such as mosquitoes, which not only prove irritating to man, but are capable of infecting him with protozoon parasites. Secondly, there is a certain resemblance between the insect and arachnid endo-parasites of both animals. Little is known about myiasis in the pig, and not very much about the subject in man. Both, however, unlike the herbivores, have no specific œstrid parasites, such as bots, warbles, and nostril-flies — *Dermatobia hominis*, the "macaw worm," is

really a parasite of cattle, only occasionally found in man. Man and pigs may, however, occasionally be parasitized by larvæ of other flies. For example, the rat-tailed larvæ of *Eristalis tenax* have been reported from the human gastro-intestinal tract in some twenty cases. I am able to record a similar case from the pig from Scotland. The larvæ which breed in dirty water, on this occasion were found in the stomach, where they had caused a fatal gastritis. Other cases have probably occurred, but have been overlooked.

The so-called tongue-worm of the dog (*Linguatula rhinaria*), which is really a degenerate arachnid, is occasionally found in both pigs and man. The parasite lives in the nasal cavity of the dog, and the eggs deposited by the female pass to the exterior and are swallowed by some suitable intermediate host—man, pig, or some other animal—in the stomach of which the embryo hatches. Passing through the intestinal wall, it encysts in some organ. The resulting larva ultimately leaves this resting place and migrates to one of the body cavities, causing more or less damage *en route*, which sometimes even results in the death of the host. The method by which the larva reaches the dog and develops into its adult form is unknown. The adult as well as the larval form occasionally occurs in man and presumably man is liable to be infected from the pig and other intermediate hosts as well as from the dog. The parasite is widely distributed and is probably much more common than is generally supposed.

Helminths.—A large variety of helminth parasites is common to both hosts. No flukes of the domestic animals can be transmitted directly to man as in every case at least one intermediate host is necessary. But the pig acts as a definitive host for a number of these parasites; and in a campaign for their eradication from an area the pig must be borne in mind.

Several species of the genus *Paragonimus* have been noted in the lungs of pigs in Asia and America: but it is difficult to say to which of the four recognized species they should be referred. It is probable that both man and the pig are infected by all four.

Among the liver flukes of the pig, we find *Fasciola hepatica*, occasionally found in man, and *Clonorchis sinensis*, a very common human fluke in Asia. *Dicrocœlium dentriticum*, a not uncommon liver fluke of sheep and other ruminants, is sometimes found in man and the pig.

Metagonimus ovatus, a small heterophyid fluke, and *Schistosoma japonicum*, one of the blood flukes, also occur in both hosts.

In the intestine of swine occur *Gastrodiscoides hominis* and *Fasciolopsis buskii*. The former, a small amphistome, is a normal parasite of the pig in Asia and an abnormal one in man. The latter, a large fluke very similar to the common liver fluke in external appearance, is also a swine parasite which lives exceptionally in man, in whom it is decidedly pathogenic.

The armed tape-worm of man—*Tænia solium*—which, fortunately, is becoming more uncommon, finds its intermediate host in the pig, in which it causes a condition known as "measles of pork." The intermediate stage, *Cysticercus cellulosæ*, is fairly resistant to cold and may be found alive in frozen pork. It is of course destroyed by thorough cooking.

It is of passing interest to note that the hydatid stage of *Echinococcus granulosus* is common in the liver of the pig. The adult worm is of course confined to carnivores; but the hydatid occurs in man, and although he cannot be infected directly by the pig, the presence of the cystic stage in that animal has an important bearing on prophylactic measures. Unless destroyed, the hydatid may be swallowed by a dog or some such animal, and the parasites reaching maturity in that host are ready to infect man.

The common worm of the pig in all countries is a species of Ascaris morphologically identical with *Ascaris lumbricoides* of man. It is generally accepted that it does not form a separate species; but it has recently been suggested that human and porcine strains exist. This question is still *sub judice* and in the meantime the pig should be considered as a potential source of infection to man. Infection with Ascaris is caused by swallowing ova containing infective embryos, and contamination of garden produce can easily be caused by pigs.

Another important human worm, *Necator americanus*, may also be found in the pig. At least forms almost indistinguishable from the human forms have been reported. Ackert and Payne described what they considered to be a new species, *Necator suillus*, from pigs in Trinidad, where the infection is very common. Gordon, however, in a critical review of this supposed species, came to the conclusion that it is not a valid one. Both he and Goodey, working independently in this country, were unable to infect pigs with *Necator americanus* of human origin. As only one experiment was performed in each case the matter cannot yet be regarded as settled. It is quite possible that *Necator suillus* is a small strain of *Necator americanus* which has become more susceptible for pigs than the human strain. Opinions among helminthologists are divided as to whether physiological strains exist or whether they are merely host variations. The pig, of course, harbours a special genus of hookworm, *Globocephalus*, peculiar to itself, but this hookworm on superficial examination might easily be taken for a human species. *Ankylostoma duodenale* of man has also been reported from pigs by several authors; but in some at least of these cases, this has been the result of a similar misidentification.

Ackert and Payne have found experimentally that normal human hookworm eggs ingested by pigs were able to hatch and develop into infective larvæ in the fæces passed by the pig. It is obvious, therefore, that, apart from the possibility of his being a host for the adult forms, the pig is an important disseminator of hookworm to man, and it would be wise to take cognizance of the pig in all schemes for the eradication of the disease, as even if he cannot harbour the adult he is capable of transporting ova of human origin from open latrines to fields and ponds where development may take place and new human hosts may be infected.

Another nematode found in man and carried by the pig is *Trichinella spiralis*. This worm is normally found in rodents and is acquired by the pig through eating infected rats and mice, or even other pigs. The worms reach maturity in the intestine, but the larva leaves this habitat as a rule, and finds a lodging place in the muscles of the pig, where it encysts. However, the fæces as well as the flesh of swine infected with *Trichinella* are infective for other animals—a fact of some importance, as it largely increases the chance of infection of man and other animals by contamination of food stuffs. Man is infected through eating improperly cooked pork or food thus contaminated. The presence of this worm in pork necessitates a strict system of meat inspection—in some countries a special staff is employed to search for this parasite alone—and owing to rigid methods of control the disease is rapidly decreasing.

Strongyloides stercoralis, a parasite of man associated with diarrhœa and enteritis in that host, has recently been recovered from pigs in Trinidad. The pigs were infected from human fæces, so that there can be no doubt of the validity of the species.

Metastrongylus elongatus, a thread-like strongyle worm commonly occurring in the lungs of the pig in all parts of the world, has been found in man, but it

is not a common parasite in that host. *Mecistocirrus digitatus*, a related worm from the intestine of ruminants and the pig in Asia, has also been reported from man on one occasion, under circumstances, however, not altogether free from doubt.

Dioctophyme renale (*Eustrongylus visceralis*), a somewhat larger nematode parasitic in the kidney of the dog, has been noted from a similar habitat in both man and the pig—a report of scientific rather than practical interest.

Gnathostoma hispidum, the spiny stomach-worm of pigs, has recently been reported in man. In this host, however, it was found in an abnormal position, i.e., subcutaneously, where it was the cause of a "creeping disease."

The thorny-headed worm of swine, *Macracanthorhynchus hirudinaceus* (*Echinorhynchus gigas*) has been reported as present in man; but if the reports are correct, infection could only have been caused by eating the intermediate host—cockchafers and other beetles. The occasional occurrence of of this practice among human beings, intentionally as well as unintentionally, attaches a certain amount of plausibility to the reports.

It will be seen that a large number of human diseases are intimately connected with the pig. Some of these exist through the mistaken idea that a pig likes filth. It has been demonstrated that it pays to keep these animals under clean sanitary conditions, just as it pays with all other animals. Others have resulted through an undue intimacy between pig and man, particularly in peasant communities. Still others have been caused by extreme carelessness in the disposal of infective offal.

Prophylaxis is simple and obvious—attention to the usual principles of hygiene with extreme care to prevent access to human excreta, will do much to eradicate these diseases from the pig and prevent their spread to man. The practice of eating pork overcooked, almost universal in this country, has probably done much to eliminate *Tænia solium* and *Trichinella spiralis* and to reduce the chances of infection with tuberculosis and other bacterial diseases.

Apart from the strictly hygienic point of view, other aspects of this question are of interest. In the first place the large number of diseases and parasites common to both hosts seems to indicate that, in some cases at least, the pig might play the important part of a laboratory animal—a somewhat clumsy one it is true, but one which might prove more suitable than some others. In the second place, attention should be directed to the pig (as well as to other domestic mammals) by those charged with the prevention of disease. And, finally, the diseases proper to the pig, as well as those of human origin found in that animal, urgently require investigation. Of all the domestic animals, the pig has probably received the least scientific attention; and I hope that I have indicated that very beneficial effects to human medicine might result from investigations into porcine diseases and their pathology.

Mr. DAUBNEY said that he was essentially in agreement with Dr. Cameron on most of the points covered. In the matter of the appearance of *Bacillus choleræ suis* in the blood-stream of pigs during the comparatively early stages of swine fever, he (Mr. Daubney) recalled that it was the practice in America, when blood was being obtained from swine fever pigs for purposes of hyperimmunization, to test such blood for the presence of *Bacillus choleræ suis* whenever any lesions of the intestine were present in the slaughtered pigs.

With regard to the incidence of *Trichinella spiralis* and *Tænia solium* in man, it was pointed out that our comparative immunity in this country appeared to be due to our habit of eating pork products only after they were well cooked. In this way infection of pigs with either of these parasites had become comparatively rare in Great Britain. It was interesting to notice that the experiments of Ransom and his

collaborators had been the means of devising several methods by which pork products which were to be eaten uncooked could be rendered safe for human consumption. Standard modes of preparation involving freezing, curing and smoking of various products of this nature were now enforced in the United States.

On the question of the specific identity or otherwise of the common *Ascaris* of man and that of the pig, he would point out that, since under experimental conditions a negative result (so far as intestinal infestation was concerned), was not at all an unusual occurrence when attempting to infect pigs with *Ascaris* from other pigs, one could not lay too much stress upon negative results when attempting a cross-infection. For this reason it appeared a difficult matter to produce direct experimental evidence either confirming or refuting the view that the same species was present both in man and in the pig.

Demonstration of some of the Pathological Results of Helminthic Infections in Animals.

By R. T. LEIPER, F.R.S., and T. W. M. CAMERON, M.A., B.Sc., Ph.D., M.R.C.V.S.

THE authors pointed out that lesions due to worm parasites were liable to occur in any part of the body. Thus in the respiratory tract there is found *Oslerius osleri*—a small filarid worm—which causes nodule formation on the trachea of the dog; and various species of Paragonimus in the lungs of carnivores and omnivores which result in the formation of verminous cysts. In the intestinal tract many helminths occur. Several examples were exhibited: (*a*) *Spirocerca sanguinolenta*, forming large tumours in the stomach of the dog; (*b*) *Œsophagostomum radiatum*, the cause of "pimply gut" or nodular disease of the intestine in cattle; (*c*) *Echinorhynchus gigas*, perforating the intestine of pigs and causing peritonitis; (*d*) *Anoplocephala perfoliata*, responsible for serious ulceration of the large intestine of horses. Outside of the alimentary and respiratory tracts there were also found lesions caused by helminths. The examples shown were (*a*) *Dioctophyme renale*, destroying the kidney substance in dogs; (*b*) *Dirofilaria immitis*, occluding the heart in the same host; (*c*) *Eleophora poeli*, in nodules, and causing aneurysms in the aorta of bovines; (*d*) *Onchocerca*, causing worm nests in Australian cattle. Pathological specimens illustrating the lesions caused by these parasites were shown. Professor Leiper pointed out that these were only a very few of the conditions caused by helminths; and that the importance of the parasites in animals and human pathology was generally very much under-estimated.

Section of Comparative Medicine.

President—The Right Hon. Sir CLIFFORD ALLBUTT, P.C., K.C.B., M.D., F.R.S.

The Bull-dog Calf: A Contribution to the Study of Achondroplasia.

By F. A. E. CREW, M.D., D.Sc., Ph.D., F.R.S.E.

(From the Animal Breeding Research Department, the University, Edinburgh.)

THE condition of achondroplasia has long been known to human obstetricians and pathologists. As early as 1817, Romberg had described a human fœtus with remarkably short limbs; others were recorded by Chaussier (1819), Weber (1829), Busch (1836) and Dumenil (1856). These writers, though satisfied that the condition which they were describing was not rickets, hesitated to give it a name. Virchow in 1856 described another case and definitely diagnosed rickets and by so doing attracted the attention of the medical profession to the condition. Winkler (1871) suggested the name rachitis micromelia, in order to call attention to the outstanding peculiarity of this supposed variety of rickets, namely, the remarkable shortness of the limbs. In 1876 Parrot described the condition as a discrete form of disease of human fœtuses, distinguishable from rickets and from congenital syphilis, and gave to it the name achondroplasia. The characteristic features of achondroplasia, according to Parrot, were: (1) micromelia; (2) abnormal shape of the cranium; (3) absence of thoracic deformity; and (4) marked thickening of the skin.

In spite of Parrot's writings, the condition was generally regarded as either rachitic or syphilitic in nature until Porak confirmed and extended Parrot's conclusions. He showed that the condition was hereditary, that it occurred among animals other than man, and that it had existed from very early times. Kaufmann (1893) described fourteen cases and suggested the name chondrodystrophia fœtalis. The name achondroplasia has the advantage of priority, and custom sanctioned its adoption, but that suggested by Kaufmann is the more descriptively accurate. Up to 1900 only achondroplasic fœtuses had been studied; in that year Marie described the condition as exhibited by the living adult. Apert (1901-2), and Méry and Labbé (1902) described further cases and distinguished between achondroplasia, cretinism, and myxœdema. Durante (1902) described two distinct forms: true achondroplasia, and periosteal dysplasia. But even now the group-name achondroplasia accommodates a considerable number of pathological conditions which may or may not be different modes of expression of a polymorphic disease. Such very different conditions have been described as micromelia, chondritis fœtalis, osteogenesis imperfecta, pseudo-chondritis, cretinoid dysplasia, micromelia chondromalacica,

osteoporosis, periosteal aplasia, and chondrodystrophia fœtalis with its three varieties: hyperplastica, hypoplastica, and malacica. In this paper achondroplasia is referred to as a group name.

The human achondroplast presents the characteristic features of the condition. The trunk is shortened in some cases; the limbs are short and thick and distinctly curved, with the convexity outwards. The head is larger than usual, with bulging forehead and parietal eminences; the bridge of the nose is depressed. The skin is thick and wrinkled. The foot is rotated inwards, the vertebral column straight, and the back flat, though the buttocks and abdomen are prominent. The condition is reminiscent of the earlier fœtal proportions, the limbs being short in relation to the trunk. Another persistent fœtal character is the *main en trident* (or "bident"), the second and third metacarpals forming an angle of about 40° with one another, instead of the usual 32°. In the newly formed fœtal hand the metacarpals form an angle of ± 90°, so that the digits are widely separated. This divergence diminishes during the course of further development, but less in the case of the achondroplast than in the normal. Still another fœtal condition is mirrored in the achondroplast pelvis, which is smaller than the normal.

The great majority of achondroplasts are stillborn, but a few survive to become robust muscular adults. The adult is of low stature and of great muscular development; he or she has a normal-sized trunk, a large head, short limbs of the rhizomelic type—the proximal segments being more shortened than the distal—thick short hands and feet, and spread digits when extended. The shortness of the limbs is not due to any crookedness or bending of the bones, as in rickets or osteomalacia, nor is it the result of multiple fractures, as in osteogenesis imperfecta, nor of the congenital absence of any bone: all the usual bones are present but they are much shorter than normally.

Achondroplasia in the human is a hereditary disease. An achondroplasic race cannot exist because an achondroplasic woman cannot come to a normal confinement and in the absence of surgical interference both mother and child must perish. Cæsarean section is necessary in order to save both, and craniotomy must be carried out if the mother alone is to be saved. Micromelia and foreshortening of the facial region, however, are familial characteristics, and they may be regarded as low grades of the condition of achondroplasia. In different cases a human achondroplast has had an achondroplasic father, mother, brother(s), sister(s), grandfather; father and brothers and sons, an achondroplasic co-twin, a normal co-twin. An achondoplasic parent of either sex married to a normal mate may have normal children. Rischbeith gives a long series of such cases. It is of interest to note that Catherine de Medici, Natalie, sister of Peter the Great, and the Empress Ann of Russia, tried without success to raise a race of these dwarfs by arranging their intermarriages.

In the case of the human, there would seem to exist different grades of this condition. The lowest grade is seen in the case of an adult of low stature whose arms and legs are short in relation to the trunk; the highest grade is that seen in the stillborn fœtus which exhibits several, if not all, of the following characters: Shortened limbs and base of skull, depressed nose, harelip, abnormality of the hard palate, narrowed foramen magnum, umbilical hernia, anasarca, hydramnios, prominent abdomen, thickened skin, abundant subcutaneous fat, apparent lordosis, brachycephaly, turned-in feet, *main en trident*, shortened veretebral column.

The pathology of achondroplasia has been described in detail by Emerson, Murk Jansen, Keith, Shattock, Sartorius and others. The condition is not one of arrest of cartilage formation, it is one of arrest of bone formation in cartilage. The parts in the posterior fossa of the skull are arrested in their growth, there is considerable contraction of the foramen, and a great shortening of the basi-occipital and basi-sphenoid, followed by a contraction of the naso-pharyngeal space. The ossification of the pre-sphenoid is also arrested and the distance of the pituitary fossa from the fronto-ethmoid junction is greatly reduced, with the result that the nasion is drawn inwards. The limbs show the condition of micromelia, the humerus and femur are affected more than the ulna and tibia, and the site of the lesion is limited to those lines where bone is replacing cartilage.

The Dexter.

The Dexter is a breed of the smallest cattle in Great Britain. Formerly it was indigenous to the south and south-western districts of Ireland, but of late years it has become increasingly popular in England. It is highly valued for its general hardiness and utility, being an economical feeder, an excellent milker, and a ready fattener, of pleasing appearance and thriving well on the poorest of land. It is used profitably as a cross in the production of "small beef." Its general appearance, as defined in the terms of the standard of excellence laid down in the "Kerry and Dexter Herdbook," is as follows:—

Head short and broad, with great width between the eyes and tapering gracefully towards the muzzle, which should be large with wide distended nostrils. *Eyes* bright, prominent, and of a kind, placid expression. *Neck* short, thick and deep and well set into the shoulders, which, when viewed in front, should be wide, showing thickness through the heart, the breast coming well forward.

Horns: These should be short and moderately thick, springing from the head with an inward and slightly upward curve.

Body: Shoulders of medium thickness, full and well filled-in behind; *hips* wide; *quarters* thick and deep, and well sprung, flat and wide across the loins, well ribbed-up straight underline; *udder* well forward and broad behind, with well-placed teats of moderate size; *legs* short (especially from knee to fetlock), strong and well placed under the body, which should be as close to the ground as possible. *Tail* well set on and level with the back.

Skin: The skin should be soft and mellow and handle well; not too thin. *Hair* fine, plentiful and silky.

Coat colour.—Bulls: Whole black or whole red (the two colours being of equal merit); a little white on the organs of generation not to disqualify an animal which answers all other essentials of this standard description. *Cows:* Black or red (the two colours being of equal merit). White on udder and the extension of white on udder slightly along inside of flank or underside of the belly, or white on tassel of tail may be allowed on an animal which answers all other essentials of this standard description.

Weight: Bulls should not exceed 900 lb. live weight when in breeding condition. *Cows* should not exceed 800 lb.

In view of that which is to follow, it is desired to call attention to these two characters of the ideal Dexter: brachycephaly and micromelia. Two undesirable characters are encountered occasionally—bad "tail-head," the tail not being terminal, but seeming to take origin farther forward along the back and arching upwards and backwards; and a combination of bent forelegs with inwardly turned hoofs. "Its toes turn in after a peculiar fashion, and it tends to walk over the outer digits, especially in the case of the hind feet."

The history of the Dexter is wrapped up in that of the Kerry and, like that of almost every breed, is befogged by anecdote and speculation. It is generally accepted that the Dexter is an off-shoot of the Kerry. Legend has it that the first Dexter had its origin in the chance mating of a recumbent Kerry cow and a seal from the sea. Be it as it may, it is certain that the breed has arisen out of the old-fashioned Kerry stock as the result of an outcross. The Dexter differs from the Kerry in that "the leg bones are shorter and more substantial, the neck thicker and shorter, the horns heavier, not so elevated and airy, and the head heavier and not so doe-like as in the case of the original Kerry."

Wilson has given an exhaustive survey of the literature which bears upon the origin of the Dexter breed and upon the manner in which it got its name. He concluded that the early records of the Dexter herd are unreliable and that a better idea of its ancestry can be derived from the genetical analysis of the modern Dexter.

A statistical study of the results of the mating of Dexter with Dexter show that four classes of calves are produced—black Dexter-type, red Dexter-type, black Kerry-type and red Kerry-type. By Dexter-type is meant a relatively massive head, stout body and limbs; by Kerry-type, a relatively slender head, slim body and long limbs. Of the four classes, the black Dexter-type is by far the most common, the red Kerry-type the least common, while the other two occur in more or less equal numbers. The appearance of four phenotypes in such proportion suggests at once that the Dexter is itself to be regarded as a Mendelian di-hybrid, and that its parental stocks differed one from the other in respect of at least two pairs of contrasted characters, coat colour and bodily conformation. The contrasted characters concerned were: black and red coat colours and Dexter-type and Kerry-type bodily conformations. The old type Kerry—black, slender and long-limbed—could have been one of the parental stocks. If this were the case then the other must have been a red, stout, short-limbed stock.

Wilson brings forward considerable evidence to show that such an animal was imported into Kerry during the formative period of the Dexter, and that it was crossed with the native Kerry, and sums up the evidence thus: "The probability, therefore, that Dexter cattle descended from black Kerries and red cattle of the Devon type is very high; and if further proof were wanted, it can be found by setting a red Dexter cow side by side with a red Devon." The modern red Dexter is a mottled red, showing delicate blackish traceries on the red ground; this same mottling is seen in the modern red Devon.

If it is assumed that the original parents of the Dexter were Kerry and Devon, two dominant factors will be introduced, B and S respectively, determining black colour of the coat and short-legged conformation of the body. It is easy to see that the result of back-crossing to either type of parent would give 50 per cent. black Dexter type, while interbreeding between the heterozygous black Dexters would give black Dexter offspring in the proportion of 9 to 16. These Dexters would be of four types, according as to whether they were duplex for both dominant factors (BBSS), simplex for both (BbSs), or duplex for one and simplex for the other (BSSs or BbSS). As soon as the picture of the ideal Dexter took shape in the breeder's mind, the production of a pure strain duplex for both factors followed automatically by the elimination of the red Kerry and the red Dexter off-types.

It is of importance to note that in 1919, the society which was formed in 1917 to promote the interests of Kerry and Dexter cattle breeding in Ireland,

decided to change its name to "The Kerry Cattle Society of Ireland." This alteration was considered advisable, for herds of pedigree Dexter cattle have practically ceased to exist in Ireland owing to the difficulty of breeding these cattle pure. It is the experience of Irish breeders that when Dexter cows are mated with a Dexter bull, a large proportion of the progeny are either stillborn or deformed. As a result of constant disappointments, owners have gradually given up the attempt at breeding pedigree Dexters, and, so far as Irish breeders are concerned, their whole attention is now directed to the development of the Kerry breed.

Concurrently with the ban upon Dexters in Ireland, a boom was started in England, and to supply the demand for Dexter cattle, the Irish breeders earnestly sought for methods by which Dexter-type calves could be produced and the monstrous calves avoided. The result of their experimentation has been that the Irish Dexter, the so-called "foundation stock" Dexter, is got not by a Dexter × Dexter mating but by using a Dexter bull and Kerry cows. This mating has never yielded a monstrous calf; it produces on the average equal numbers of good type Dexter and of "sneem" Kerries. The Kerry-type animal so produced is a diminutive Kerry; Sneam is the district of Kerry in which the foundation stock Dexter is raised for export to England, and "sneem" is used locally as a term of reproach, being applied to any undersized creature. The relative numbers of red and of black individuals varies considerably, for it is certain that many Kerries are heterozygous for their black coat colour character. In England the English Kerry and Dexter Cattle Society was founded in 1892, and published its first "Herdbook" in 1900. By the regulations laid down in it, "a cross between the Kerry and the Dexter is considered a half-bred, and cannot be entered." Very soon, as Wilson says, "what was formerly known to Kerry men now became known to other breeders who bred Dexters according to the rules of the 'Herdbook': that such a procedure invariably resulted in the production of a proportion of dead misshapen calves."

The Monstrous Calf of the Dexter.

The abnormalities which these stillborn calves exhibit are constant, and are so characteristic that the fœtus is known as a "bull-dog" calf. The cranium is bulging, the nose distinctly depressed, the lower jaw protruding; the upper lip is split, baring the tongue, while the swollen tongue, thrust far out, curls up over the nose. Owing to the disproportionate development of the buttocks, the tail seems to have its origin far up on the back: usually there is a gaping deficiency of the abdominal wall through which the intestines pass to form a large umbilical hernia. The skin hangs loosely in folds, and there is abundant subcutaneous fat. The limbs are ridiculously short and the digits unusually separated.

The period of gestation in the Dexter is approximately 284 days. In the great majority of cases it can be foretold that a pregnancy is to terminate in the production of a "bull-dog" fœtus, for in such cases the pregnant cow begins to increase in size very rapidly about the third or fourth month, and ultimately becomes very distended. The early obliteration of the hollow in the flank just in front of the hip is recognized as a sure sign of impending trouble. Then it is noticed that the cow is losing "water," which dribbles from the vulva, and that she is becoming less and less distended. After a time the loss of fluid ceases, but after a short interval the cow is as "big" as ever. Again there is the flow of fluid from the vulva and the decrease in size, and again the

cycle is repeated. Following one of these discharges of fluid from the vulva the fœtus is aborted. The fluid is described as being clear in the majority of cases; in a few it has been turbid. But, as Seligmann [22] has previously recorded, it is not invariable for a Dexter with a considerable degree of excess of amniotic or allantoic fluid to give birth to a deformed calf. Moreover, the pregnancy which results in the abortion of a dead monstrous calf is not invariably associated with such excess. Extremely rarely, so the breeders say, the first indication of anything abnormal is a premature labour. In such cases the calf is not delivered naturally, it must always be removed by operative procedure, and is dead when delivered. The puerperium following the delivery of the "bull-dog" calf differs from that following the birth of a normal one. The placenta comes away in small fragments, or has to be extracted manually, instead of being thrown off complete in a half to four hours. In fact, herdsmen will state that there is no after-birth in the case of the "bull-dog." The lochia last longer than is usual, the blood-stained discharge persisting in certain cases even as long as a fortnight instead of the usual one to four days. The abortion of a fœtus, other than a "bull-dog," is followed by an immediate cessation of mammary activity; the abortion of a monstrous calf, on the contrary, does not interfere with this, and the cow produces milk.

The normal Dexter calf is a small individual compared with calves of the same age, but of the larger breeds. No specimen of the normal Dexter fœtus has been available for comparison, but an "off-type" (a Kerry type) fœtus was obtained at the eighth month of pregnancy. The bull-dog fœtus of the same age is very much smaller.

Age of bulldog	Weight	Length	Diameter shoulders	Length of foreleg	Length of hindleg
7 months	12 lb. 15 oz.	28 cm.	44·1 cm.	5·1 cm.	5·7 cm.
7 ,,	10 ,, 3 ,,	30·5 ,,	42·6 ,,	7·3 ,,	7 ,,
7 ,,	11 ,, 11 ,,	30·2 ,,	39·9 ,,	6·1 ,,	5·7 ,,
4-5 ,,	6 ,, 11 ,,	17·8 ,,	30·2 ,,	6·1 ,,	6·4 ,,
3-4 ,,	3 ,, 8 ,,	20·5 ,,	17·0 ,,	3·0 ,,	6·0 ,,

An examination of many specimens has shown that there is never any suggestion of putrefaction or of mummification of the fœtus. The abortion quickly follows the death of the calf. The death of the fœtus is associated with severe fœtal anasarca (fœtal dropsy) in the case of the earlier abortions and of the majority of the later ones, and with profound dystocia in the cases which proceed until near term. In practically all cases in which there is hydramnios (or hydrallantois) fœtal anasarca is present; the fœtus is a fluid-logged shapeless mass and the almost complete subcutaneous covering of the abdominal wall is devoid of skin over a circular area based upon the umbilical cord. In the case of the older fœtus death results from dystocia. The prolonged and difficult labour is made inevitable by the shape and consistency of the fœtal head which cannot be accommodated by the maternal birth passages. The bull-dog fœtus has a head of unusual shape and consistence. The size of the fœtus and the shape of the head are to be regarded as significant features.

As regards the incidence of the monstrous calf, many entries of "premature" or "dead" are to be found in the herdbooks in the column which shows the births during the year. It cannot be expected that such a record is critical. English Dexter breeders, anxious to find a solution of the problem, supplied the following absolutely trustworthy figures:—

Total births	Normal calves	"Bull-dog" calves	Proportion
646	530	116	1 in 5·5

At first sight there appears to be no consistency in the occurrence of the monstrous calf. Some herds are singularly free from them, others yield so many that the breeder is forced to get rid of his stock. A certain cow may produce a series of most excellent Dexter calves and then yield a typical monster to the same sire. Or else, a cow will produce a series of "bull-dogs" to the service of several different bulls, and the next season will produce a prize-winner. There is no animal out of a Dexter × Dexter mating that is not related more or less closely to a monstrous calf; the "bull-dog" appears in all herds in numbers ranging from 5 per cent. to 30 per cent. of the total births.

As regards the coat colour of the "bull-dog" calf, it is more often black than red, for the same reason that black is the more common colour of the Dexter. Of the twenty-seven cases examined, twenty were black and seven red, as close an approximation to a 3 : 1 ratio as can be.

Of the twenty-seven cases examined, twenty-one were males and six females. For a long time none but males were received, which led to suspecting sex-linkage. Later, however, six female "bull-dog" were obtained; at first these were regarded as possible cases of abnormal differentiation of the sex-organization in a male. But the demonstration of definite ovarian tissue in the gonads had made it certain that they were female, and the condition, therefore, is not sex-linked. In the older fœtuses the scrotum is well defined as is also the vulval cleft, but in the earlier specimens the sodden skin and the great rent in the abdominal wall make the identification of the sexual apparatus peculiarly difficult. Moreover, since the specimens cannot be examined until at least eighteen to twenty-four hours after delivery, the histological evidence is rather weakened. But the evidence is such that it shows quite definitely that the "bull-dog" may be either male or female, though, if the series of cases examined can be regarded as a representative sample, the majority of monstrous calves are male.

The Pathology of the Monstrous Calf.

Fœtal anasarca in the great majority of cases is present. This condition is not uncommon in cattle and is almost invariably associated with the death and abortion of the fœtus at the sixth to seventh month. According to Williams [27], fœtal anasarca has been recorded only in ruminants among the domesticated mammals. The cause is not known; it has been ascribed by some writers to a congenital absence of the thoracic duct. In the case of the "bull-dog" it is invariably associated with hydrops amnii and it is reasonable therefore in this case to suspect that both conditions are due to one and the same cause, although in ruminants generally there is no doubt that the two conditions can occur quite independently.

Hydramnios.—The normal amount of fluid present in the amnion of the cow is about five to six litres, the amount in the allantois about six to fifteen litres. Any material excess is considered abnormal. The amniotic fluid is clear and the allantoic turbid in later pregnancy; the fluid evacuated in the case of the "bull-dog" calf is clear; the condition present in the case of the monstrous calf is probably hydramnios and not hydrallantois. When examined, no definite cell elements can be recognized in either stained or unstained preparations. The amount of fluid evacuated in one case was approximately 160 litres. Hydramnios is not uncommon in cattle and is the rule in such species crosses as *Bos americanus* × *Bos taurus*, or *Bos americanus* × *Bos indicus*. Williams [27] records that in one case (not in the Dexter) after

trocarization of the fœtal membranes and the withdrawal of twelve to fifteen gallons of the fluid, the original volume was restored within forty-eight hours. The cow showed signs of great thirst, and the author remarks that "apparently the water consumed went almost immediately to the fœtus and was deposited into the fœtal membranes, while the cow, drinking greedily, was suffering from water-starvation."

The exact cause of hydramnios is not known, nor can it be until the origin of the liquor amnii itself has been demonstrated. The prognosis in cases of severe hydramnios in cattle generally is very unfavourable both for the cow and calf, for, as a result of the pronounced uterine dilatation and uterine paresis, the cervix is not dilated naturally, and the abdominal muscles, losing tone for the same reason, do not aid in the expulsion of the fœtus. Delivery is always by operative procedure as is also the removal of the placenta. In the Dexter, however, the prognosis is entirely satisfactory as far as the cow is concerned; normal labour pains come on, the cervix is dilated, and the fœtus is expelled without any artificial interference unless it is an older fœtus, for then the head cannot pass the pelvic brim and operative interference is imperative. The placenta must be removed manually in most cases. The difference in the prognosis in the case of the Dexter and other breeds rests on a difference in the general musculature. The Dexter is a peculiarly muscular individual and can withstand much better the stretching which hydramnios involves.

Hydrocephalus was in one case associated with hydramnios and fœtal anasarca. It was greatly pronounced, there being literally no brain.

The Skeleton.—The shape of the skull of the monstrous calf is characteristic. Brachycephaly is marked, the cephalic index being about 100 per cent. and the skull being diminished in length and enlarged in width. The head is large with bulging forehead and parietal eminences; the nasal region is much depressed. The bones of the vault are thicker than is usual and the most striking feature of all is the extreme shortness of the base. The supra-, ex-, and basi-occipital, the basi-, ali-, and pre-sphenoids are fused into one mass of bone and no suture of any sort can be identified. The orbito-sphenoid is distinctly delimited. In a normal Dexter fœtus of seven months the basi-occipital measured 3·1 cm. and the basi-sphenoid 2·5 cm., giving a total antero-posterior length of 5·6 cm., when measured on the exterior of the skull, and one of 5 cm. when measured on the interior aspect. These combined bones in a "bull-dog" fœtus of the same age measured 1·2 cm. and 1·3 cm. respectively. In the normal, the foramen measured 2·6 × 2·8 cm., in the bull-dog it was 1·8 c.m. long and was waisted, measuring 0·9, 0·8, 0·9 cm. in width. No distinct condylar faces were discernible. Internally the ethmoid in the normal covered the orbito-sphenoid, in the bull-dog it did not.

The vomer in the monstrous calf is extremely robust and has a very broad articulation laterally with the palatines. There is no hard palate and the nasal portion of the skull is completely exposed ventrally in consequence of the entire absence of any inward growth of the maxillæ and palatines. The posterior palatine processes of the premaxillæ are present, being fused with the anterior end of the vomer. The maxillæ are greatly foreshortened as are also the nasals. The foreshortening of the maxillæ is illustrated by the unusual prominence of the lachrymals and by the fact that while the general contour and proportions of the post-palatine processes of the maxillæ are normal, the premaxillæ, normal in design, practically come back to the level of the maxillo-palatine suture. Viewed ventrally, the premaxillæ lie much deeper than the inner margin of the

maxillæ and palatine, the contour of the latter bones being such that there is no suggestion of any effort to make a palate. In this respect the skull is distinctly reptilian. The abnormalities of the skull are such as affect its posterior fossa and except for various degrees of asymmetry, and for the greater thickness of the vault, the remaining peculiarities are such as might follow from the abnormality in the ossification of the base. The skull of the "bull-dog" fœtus resembles that of the bull-dog in one respect only: both show foreshortening of the facial region due to an arrest of development of the nasals and maxillæ.

In the case of the limbs, the epiphyses of the long bones do not differ materially from those of the normal, but there is a profound difference in the length of the shaft. The shortening affects the proximal bones of the limb more than the distal. The digits are separated more widely than in the normal calf of the same age. Sections of the epiphyseal line of the long bones of the limbs show certain characteristic features. Normally, bone formation proceeds by (1) endochondral ossification at the junction of epiphysis and diaphysis, determining growth in length; and (2) periosteal ossification determining growth in thickness. In endochondral ossification three zones can be distinguished in the area between the undifferentiated cartilage of the epiphysis to the diaphysis: (1) the zone of proliferating cartilage cells in which the multiplying cartilage cells lie in a clear matrix: (2) the zone of parallel columns of cartilage cells in which the hyaline matrix is the seat of calcification, and (3) the line of ossification in which each column of zone (2) extends as far as the bone marrow, from which vascular loops extend to erode the cartilage columns in a regular manner. The septa separating the columns become eroded and denuded of their cells and become encrusted with calcareous salts, and on their surfaces osteoblasts are deposited by the vascular loops. These osteoblasts lay down successive layers of true bone whilst the calcareous material of zone (2) is removed by osteoblasts.

In the case of the monstrous calf, zone (1) is unaltered, but on the surface of this there is a vascular fibrous area enclosing small islets of cartilage. There is no zone (2), the columnar arrangement of the cartilage cells being entirely absent, and the cells, few in number, are scattered irregularly. The matrix is not hyaline but shows fibrillation and the deposition of calcareous salts is irregular. Zone (3) is thin and irregular. Endochondral ossification occurs but the absorption of bone trabeculæ is defective so that reconstruction of the cancellous tissue fails. The trabeculæ are excessively thick, with comparatively small rounded areolæ between them. The condition is one of abnormal endochondral ossification in that there has been defective absorption at the stage of secondary areolar formation. Periosteal ossification in these cases is normal and the abnormality is restricted to the epiphyseal line where the cartilage cells are entirely passive, where there is no division, no column formation and no vacuolation and hypertrophy.

The vertebral column is also involved, in some cases being less than half the length of that of a normal fœtus of the same age, and along with this abnormality there are others affecting the size and shape of the thorax and of the pelvis. The conditions found in the case of the "bull-dog" calf are such as are found in the clinical and pathological entity known as achondroplasia in the human. It would seem that the exhibition type Dexter is itself a low-grade achondroplast and that the "bull-dog" calf produced by the mating of two such individuals is a high-grade achondroplast, exhibiting the classical features of the condition in a most pronounced form. If the exhibition Dexters are simplex for the factor corresponding to the achondroplasia-like condition (Ss), then 25 per cent. of their calves should be high-grade achondroplast (SS).

The Ætiology of Achondroplasia.

In the case of the human, many and varied causes have been suggested. Bohn and Schwab as early as 1868 suspected a disturbance or an insufficiency of the placenta. Parrot (1876) considered that a congenital nutritive disturbance of the cartilage cells was responsible. Klebs (1889) suggested that a compression of the fœtus by the umbilical vesicles was the cause. Von Franqué (1893) and Rindfleisch (1889) also suspected mechanical pressure. Dor (1893) suggested that an auto-intoxication was the cause. Poncet and Leriche considered that the achondroplasts constituted a distinct race, while Buck and Mayer (1900) held that the condition was a hereditary process and that the most severe cases were the last of a degenerate race. Porak and Durante (1905) inclined to the opinion that the condition was sclerosis of cartilage resulting from an auto-intoxication. Cestan and Regnault described the condition as a form of intra-uterine rickets. Marie (1900) suspected abnormality of some gland of internal secretion, while Lugano and Devay agreed with Leblanc regarding a mal-functioning of the thyroid as the cause. Collman described a case in which the thyroid was much enlarged as did also Virchow and Neumann. Bowlby, on the other hand, records a case in which the thyroid was absent. Regnault, however, concludes that in the majority of cases this gland is normal. Vargas held that the thymus was responsible while Parhon, Shunda, and Zalplachta (1905) went further by suggesting that the condition was due to a combination of hypofunctioning of the pituitary, thyroid and thymus, together with a hyperfunctioning of the sexual glands. Murk Jansen [14] advanced an ingenious hypothesis to explain the conditions found in these cases. He argues that the responsible cause is a compression of the fœtus by a too small amnion or by a hydramnios. Keith [15] disagrees emphatically with Jansen's conclusions, pointing out that a comparison of such contrasted conditions as the frontocephaly of achondroplasia and opisthocephaly and simoprosopia leads directly to the conclusion that such differences in end-results can be explained in terms of different action on the part of the agencies regulating growth. This growth-regulating mechanism is in all probability the endocrine system, and Keith argues that in the light of recent advances in endocrinology it is indeed probable that the stimulus which brings about the preparatory pre-ossification changes in bone formation is an internal secretion "such as we may expect to arise either in the pituitary, thyroid, suprarenal or genital glands, or by an interaction of secretions from all of these," and that the cause of achondroplasia is a malfunctioning of one or more of these glands of internal secretion.

The present study has not furnished a complete answer as to which, if any, of the glands of internal secretion is to be regarded as the responsible causal agent in the production of achondroplasia, assuming, that is, that this condition in the bull-dog calf is achondroplasia. But this much is certain: there is profound abnormality in some of the ductless glands. If it is desired to investigate the cause of achondroplasia in the human, the most satisfactory way will be to maintain a herd of Dexters and to produce abortion of fœtuses during the first three months of pregnancy.

The Pituitary.

In the case of the human achondroplast smallness of the pituitary has been commonly reported and this fact has been considered to be of some significance as it has been conclusively established that this gland is concerned in the

normal growth processes. But no definite and constant histological abnormality of the pituitary has been found. In the case of the "bull-dog" fœtus, the pituitary is definitely smaller and is more compressed than in the normal. Histologically it presents the usual structure, except that in many cases there are areas of oxyphil cells in the pars intermedia. The cells of the anterior lobe appear to be more closely packed than is usual, and the vascularity of the gland to be less than in the normal. It is not profitable to discuss the possible significance of such vague impressions as these. But advantage was taken of the melanophore test devised by Hogben and Winton in order to examine the functioning of the posterior lobe of the pituitary in the monstrous calf. These workers have shown that following an injection of a minute quantity of posterior lobe extract into a frog previously kept under those conditions which conduce to skin-pallor, there is a very characteristic and rapid darkening due to a marked expansion of the melanophores. In the case of the normal cattle, pituitary is active, as estimated by this test, at the beginning of the third month of fœtal life. The pituitary is ground up in a mortar with sand, with distilled water, and the extract injected intra-abdominally. In the case of the "bull-dog" fœtus, the pituitary of a six months' specimen gives a very doubtful reaction. A four months' pituitary gives a still more doubtful reaction. It is granted that this test is one for posterior lobe activity and that a mal-functioning of the pituitary which would constitute a possible cause of achondroplasia would be one involving the anterior lobe. Nevertheless, it would appear to be not without significance that in the "bull-dog" fœtus the posterior lobe is, physiologically, relatively inactive at the fourth and sixth months; it is not unreasonable to think that the measure of the activity of the posterior lobe can be regarded as an indication of the functioning of the gland as a whole.

If this is so, then indeed there is reason to suspect that a mal-functioning of the pituitary during the earlier months of fœtal life is responsible for the abnormalities in ossification and in growth. If, for example, the pituitary does not function properly at the time when the normal process of ossification begins, and if the guiding stimulus of the pituitary is necessary for the normal development of bone, then a retardation of pituitary activity, or an insufficiency of its secretion, could lead to abnormality in bone formation and the degree of imperfection in the end-results will vary with the degree of retardation of the pituitary functioning and with the difference in the time at which the different parts of the skeleton become ossified.

The suspicion that a mal-functioning of the pituitary may be involved in the causation of the conditions found in the monstrous calf is strengthened by the work of Krogh [16], who has shown that there is good reason to believe that posterior pituitary substance is concerned in the production and maintenance of capillary contractile tonus. Insufficiency results in capillary dilatation and œdema. In this way pituitary mal-functioning can lead to anasarca, hydrocephalus and hydramnios. Moreover, it was found by Smith—and this finding has been confirmed by Hogben—that general œdema commonly followed injection of pituitary extract into larval amphibia.

In the case of the "bull-dog" fœtus, Seligmann (1904) [22] found that the thyroid was abnormal and concluded that the condition was one of fœtal cretinism. In seven cases the thyroid was œdematous and purple: the isthmus was absent or irregular in shape. Histologically the gland consisted of masses of more or less cubical or spheroidal cells and the capillary network was extremely dense. Very few vesicles and sometimes only the faintest trace

of a vesicular arrangement could be detected. There was complete or almost complete absence of colloid, and the lumina of the vesicles were packed with cells. In 1911 Sheather [23] described the thyroid as being normal in size, shape and histological structure, except that there was a slight excess of interstitial tissue in some parts of the gland. The vesicles were perfectly formed and filled with colloid. Crew and Glass (1922) [5] described the thyroids of five fœtuses and demonstrated that in these cases the thyroid did not show the histological features of a hypofunctioning but rather that of a hyperfunctioning gland. In the large series of cases which have been examined the thyroid has varied considerably. In some, mostly the younger specimens, it has not been remarkable; in others it was enlarged and the histological picture was that of a thyroid from a case of hyperthyroidism, no colloid being present, the small irregular vesicles containing papillary in-growths of epithelium and desquamated material, and the section consisted of masses of solid cellular hyperplasia. In the older specimens the thyroid showed the typical signs of involution, the vesicles were enlarged, irregular and full of colloid, the epithelium low and the previously hyperplasic intervening tissue undergoing retrogression and transformation into fibrous tissue.

The sequence of events as suggested by the different histological appearances seen in different cases would seem to be: first, a developing thyroid, then a hyperplasic hyperfunctioning thyroid, and finally an involuting gland with fibrous atrophy and progressive hypofunctioning. Such a scheme would accommodate the different descriptions which have been given by previous writers and would explain the diagnosis of fœtal cretinism. This seriation of events is typical of cretinism, but it is also typical of the course of events following removal of the anterior lobe of the pituitary in mammals, as has been shown by Cushing [6] and more recently by Dott [7]. The lesions found in the thyroid do not necessarily indicate that the condition is that of fœtal cretinism. The mother of a monstrous calf is not herself goitrous. Shattock, in Seligmann's paper [22], suggests that the maternal thyroid is probably sufficient for the mother's needs only, but the experiments of Halstead and of Edmunds on the dog suggest that, were this so, the thyroid of the fœtus would be greatly enlarged. The conditions found in the thyroid of the "bull-dog" fœtus can be regarded as secondary to a mal-functioning of the pituitary.

Since Gudernatsch [10] demonstrated the efficiency of thyroid feeding to accelerate Anuran metamorphosis, the value and specificity of this test at least as an indicator of the iodine content of the gland, as demonstrated by Lenhart [18] and Swingle [25], has been universally recognized. Using the axolotl—the larval form of the Mexican salamander—there is the opportunity of demonstrating thyroid activity in a most spectacular way, for a single meal of fresh gland suffices to induce metamorphosis. This is a critical test for thyroid iodine: the transformation does not occur in aquaria without this stimulus and it cannot be induced by the oral administration of inorganic iodine. This axolotl test has been shown to be a specific test for thyroid activity by Laufberger [17], Jensen, L. Kaufmann, and by Huxley and Hogben [13]. An axolotl weighing 64 grm. was fed with 2 grm. of fresh thyroid substances taken from a seven months' "bull-dog" fœtus. Complete metamorphosis resulted in shedding of the larval skin in the usual twelve to fourteen days after the thyroid meal. A second axolotl of 23 grm. was fed with 1 grm. of thyroid from a four to five months' "bull-dog" fœtus. Metamorphosis occurred with shedding of the larval skin at about the same time as in the previous instance. This test cannot be applied successfully

before the fourth months of intra-uterine life in the case of the normal cattle fœtus. From the fourth month onwards the thyroid is active as estimated by this test, but before it is not. It will be noted that according to the tests used, the pituitary is functional before the thyroid. These observations give a clear demonstration of the fact that there is present physiologically active iodine in the thyroid of the monstrous calf of the Dexter at an early stage of fœtal life. They do not support the contention that the condition is that of cretinism.

The *adrenals* have in no case been perfectly normal. In some there was an undue amount of fibrous tissue in the cortex and in the medulla. In the majority there were areas of cartilage and, in many, areas of cartilage bone with Haversian canals and areas of calcified cartilage with osteoblastic bone on its surface. The different stages in this bone formation would appear to be: hyperplasia of fibrous tissue, formation of hyaline cartilage in these areas, fibrillation of the cartilage, calcification of the matrix, absorption of the calcified cartilage by osteoclasts, and deposition of osteoblastic bone on the surface. The exact significance of this cartilage bone formation in abnormal situations has not been established, but since similar areas have been found in other organs in which there is plentiful connective tissue, it is suggested that the condition is a general one and possibly is secondary to a mal-functioning of the pituitary.

No other abnormality of the endocrine system was encountered. It may be remarked, however, that, in contra-distinction to the general finding of sexual precocity in the living human achondroplast, the differentiation of the sex-organization in the case of the monstrous calf of the Dexter is not so advanced as in a normal cattle fœtus of the same age.

The tentative conclusions arrived at from this study are the following: Very possibly the condition results from a mal-functioning of the pituitary between the second and third month of intra-uterine life. Under these conditions the proper control of cartilage bone-formation is lacking. The thyroid undergoes hyperplasia and this is followed by involution. The severity of the condition may be determined by the degree of retardation in the functioning of the pituitary or by the delay in the production of an efficient internal secretion of this gland. It is not claimed that a definite answer has been given to the question as to the primary cause of the condition. It is felt, however, that the study of the monstrous calf of the Dexter has provided a strong argument in favour of using this material for a complete and thorough study of the conditions akin to achondroplasia. A small herd of Dexters would provide the finest experimental material for a demonstration of the bearing of genetics upon pathology and upon the science of animal breeding.

SUMMARY.

(1) A review of modern conceptions concerning the condition of achrondroplasia in the human is given.

(2) A description of the "bull-dog" calf is given and it is shown that the conditions met with in such are identical with those of achondroplasia.

(3) It is suggested that the incidence of these abnormal calves can be interpreted in genetical terms, and the inference is drawn that the achondroplasic condition is a simple Mendelian dominant character; that the exhibition type Dexter is heterozygous for this character, and therefore the mating of such individuals must yield about 75 per cent. of bull-dog calves.

(4) Reasons are given for suspecting a non-punctual functioning of the pituitary during fœtal life as the primary cause—based on the genotype—of this condition.

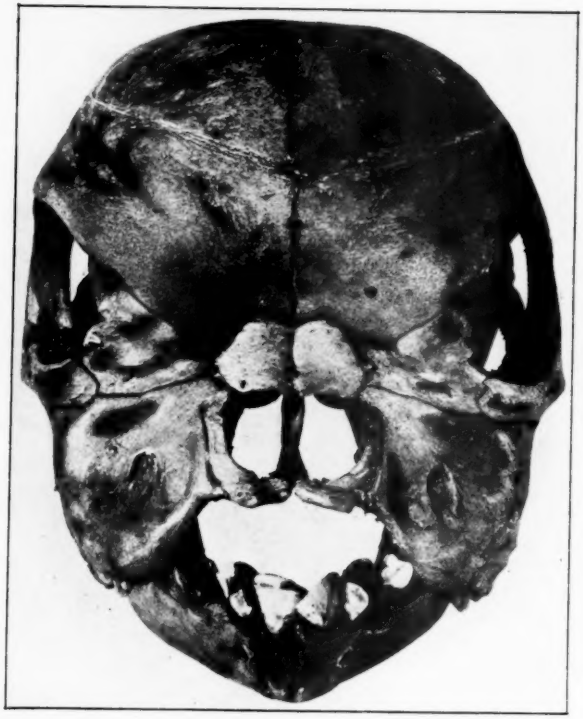
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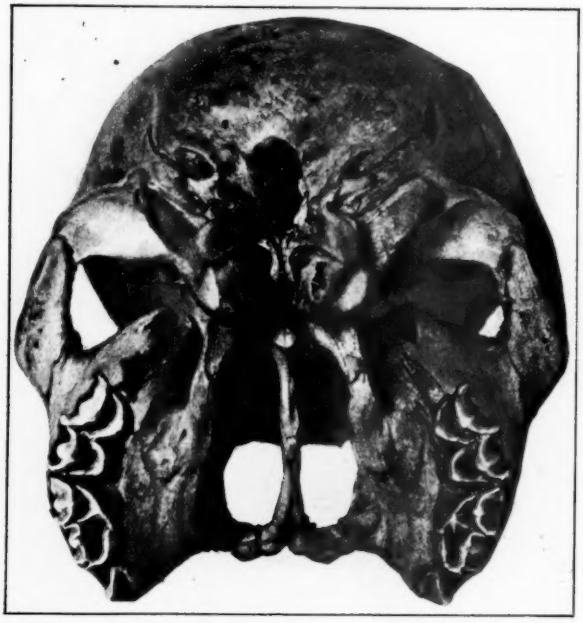
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Mr. L. P. Pugh said that Dr. Crew's paper opened up a wide field of thought and research. He agreed with his suggestion that the "bull-dog" calf was in all probability a non-viable form of achondroplasia. He wondered if Dr. Crew could give other examples of mutations that were additive in action. He thought the study of aborts intentionally produced in Dexter cattle likely to give very useful knowledge, and explained how such abortion could be produced. He expressed doubt as to whether the whole question of achondroplasia could be settled within the confines of a study of the Dexter breed, and said that he would like to see a general survey of all achondroplasic-like animals (of which there were many) in order to try to detect some common operative factor. That the head could be achondroplasic and not the limbs as in the case of the pug-dog, and the reverse as in the case of the dachshund was a fact that needed explanation. Mr. Pugh expressed the hope that Dr. Crew would be given the facilities for extended study of the Dexter breed and the aborts from them, and said that he felt sure that it would result in a great deal of useful knowledge both in genetics and in the study of the endocrine glands.



FIG. 1.—A "bull-dog" calf, 6 to 7 months, black ♂.





FIGS. 2, 3.—The skull of a "bull-dog" calf.

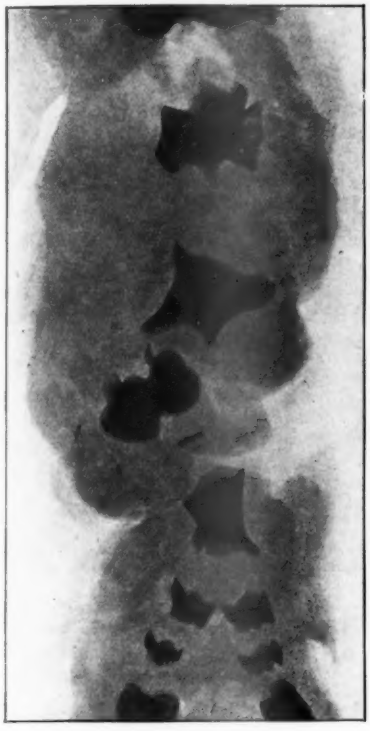


FIG. 4.—Radiograph of fore-limb.



FIG. 5.—Radiograph of hind-limb.

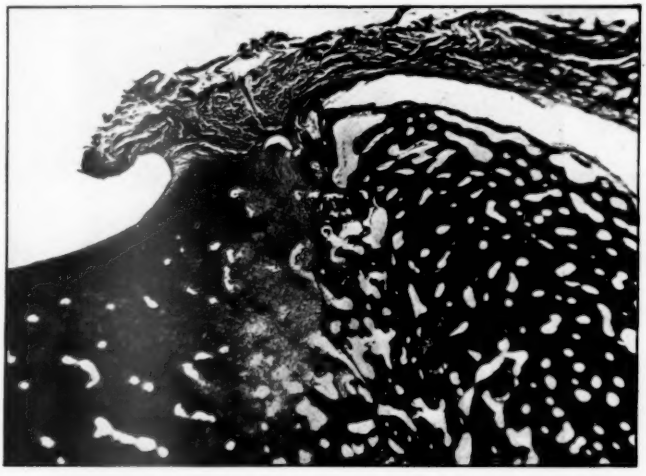


FIG. 6.—Photomicrograph of section through end of a long bone, showing complete failure of reconstruction of cancellous bone. × 22.



FIG. 7.

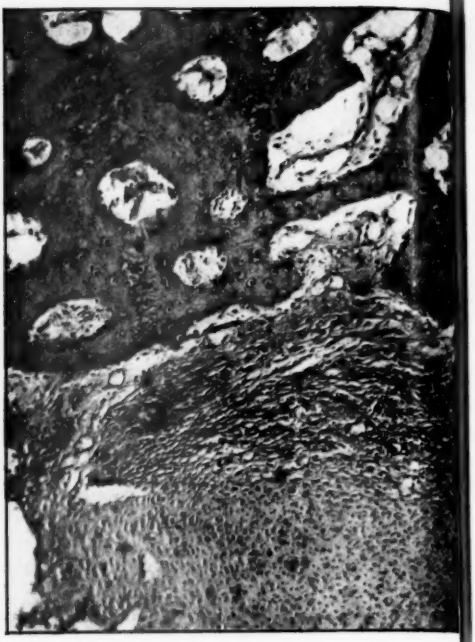


FIG. 8.

FIG. 7.—Photomicrograph of the epiphysial line showing attempt at column formation.

FIG. 8.—Photomicrograph of area of calcified cartilage, showing failure of absorption of same and absence of reconstruction of cancellous tissue. Osteoblasts are present, but fail to deposit osteoblastic bone. The epiphysis is normal, and there is no sign of any cell proliferation and none of any attempt to form the serrated zone. The ossifying junction is occupied by a thick mass of osteoid tissue, which separates the cartilage from the diaphysis, and this area is non-vascular. The condition is one of abnormal endochondral ossification. × 110.

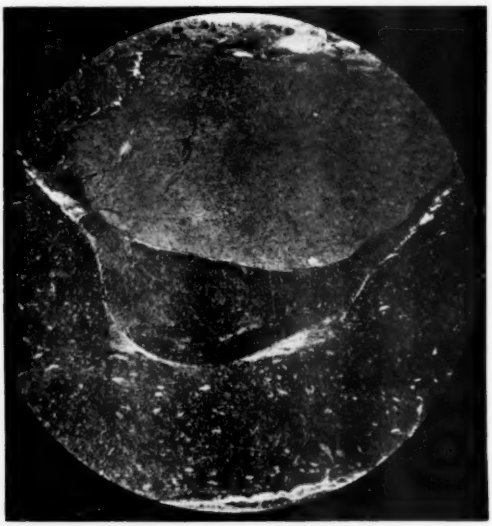


FIG. 9.—The pituitary of a "bull-dog" calf: normal structure. × 15.



FIG. 10.—The adrenal of a "bull-dog" calf, showing areas of cartilaginous bone-formation. × 85.



FIG. 11.—The thyroid of a "bull-dog" calf. Natural size.

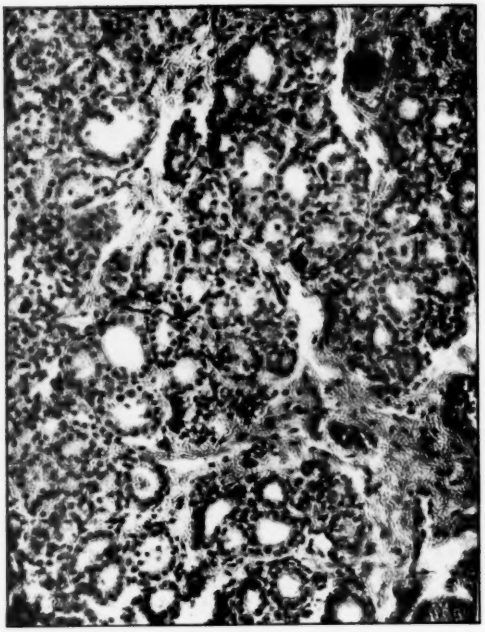


FIG. 12.

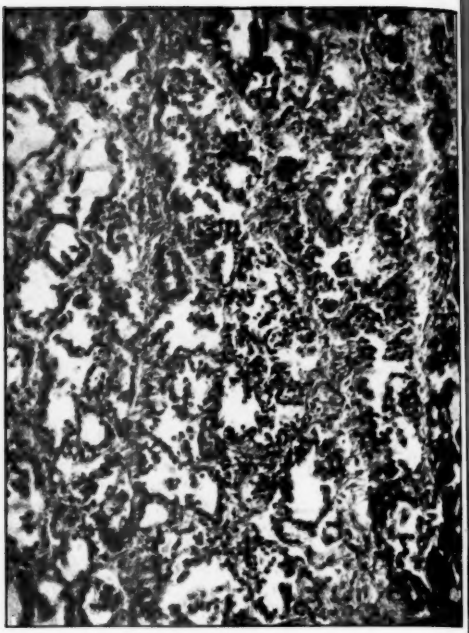


FIG. 13.

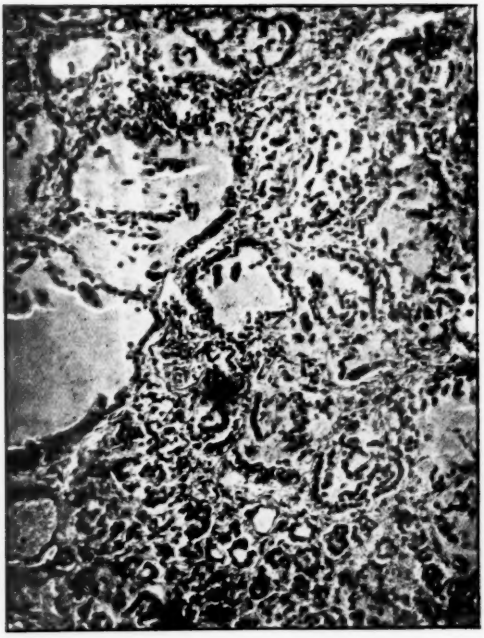


FIG. 14.

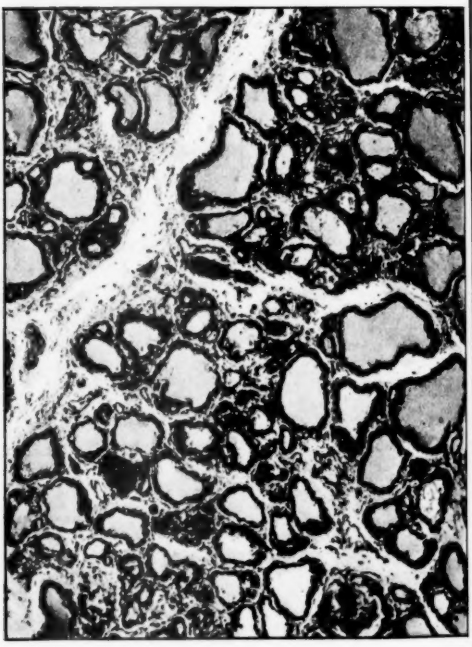


FIG. 15.

FIG. 12.—Thyroid of a "bull-dog" calf: normal structure. × 110.

FIG. 13.—Thyroid of a "bull-dog" calf: hyperplasia. × 110.

FIGS. 14 and 15.—Thyroid of a "bull-dog" calf: involution. × 100.

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Section of Dermatology.

President—Dr. E. G. Graham Little.

Benign Lymphogranulomata (Sarcoid).

By L. Savatard.

Patient, a male, aged 25. The skin lesions are bluish to brownish-red in colour, raised above the surface of the surrounding skin, with sharply defined outlines; fairly firm on palpation and infiltrating the true skin to some depth; unattached to the deeper tissues. In some atrophy has taken place and in the facial plaques central scarring, the result of previous treatment, is evident. The diascope reveals a brownish coloration but no true apple-jelly nodules. The lesions can justly be described as "lupoid" and hence the name of lupus pernio was formerly given to this condition (though all tuberculous tests have proved negative). The lesions, for the most part, are situated on the face and extremities though some few are located on the trunk. Those on the legs are more superficial and present some desquamation. Some of the fingers are deformed though the feet are clinically apparently normal.

In 1915 he was treated for bilateral lupus of the face by means of the Finsen lamp. After about two years' treatment an apparent cure resulted but early this year the facial lesions recurred, and the other lesions gradually supervened. The condition of the hands, however, was noted at the time of the earlier lesions and he suffered from recurrent attacks of swellings of the fingers preventing free movement. In May of this year (1923) the patient consulted Dr. John Wharton for acute iritis of the left eye and in September the right eye was affected with peripheral choroiditis.

When I saw the patient on September 10 I diagnosed the condition as one of benign lympho-granuloma and have requested his attendance to-day so that you may compare the case with the one I showed at the meeting of the British Association of Dermatology at Liverpool in July last.

The blood gives a negative Wassermann reaction, and the blood count shows an increase of white cells with 7 per cent. of large mononuclears.

A radiogram of the hands shows central destruction of millet seed to pea-sized areas in the distal ends of some of the phalanges, and the toes (which clinically are normal) present a similar, though less marked, condition.

A radiogram of the chest shows diffuse fibrosis of the lungs and a persistent thymus.

The liver and spleen are apparently normal.

The lymphatic glands—epitrochlear, axillary, inguinal, &c.—are enlarged and fibrosed.

The treatment, since the patient has been under my care, has consisted in small doses of Fowler's solution and under it the lesions are clearly resolving; but the iritis is not clearing up as rapidly as we could wish. The patient in my Liverpool case had suffered from iritis some years ago.

Case of Lupus Erythematosus with Hypodermic Nodules.

By H. MacCormac, C.B.E., M.D.

Patient, a female, aged 37, gave the following history. She had suffered from joint pains with swellings of the larger joints for eighteen or nineteen years; about fifteen years ago an eruption was observed on the fingers of both hands, which she thought to be of the nature of chilblains, but which persisted during the summer months; these lesions, still present, are typical examples of lupus erythematosus. About six years ago nodules appeared on the legs—well-marked Bazin's disease. The eruption on the head began some fifteen months ago as small "scurfy" areas, later becoming erythematous; there is a considerable loss of hair. From the scalp the eruption spread to the face, involving the forehead, chin and cheeks, as an erythema resembling more the acute than the chronic type of lupus erythematosus.

The following investigations were carried out: the Wassermann reaction was negative; a catheter specimen of the urine was sterile on culture, and no micro-organisms were detected in films made by the direct method. The nasopharynx was reported as normal, X-ray photographs of the teeth revealed no foci of infection, there was no clinical evidence of tuberculosis, and the complement-fixation test for tuberculosis was completely negative.

A vaccine was prepared from the intestinal flora, streptococci and *Bacillus coli*. This has been administered with benefit, so far as the joint condition and the eruption on the face and scalp are concerned, but without influencing the Bazin's disease or the eruption on the hands.

The special points of interest in this case are centred first, in the association of three conditions in one patient—lupus erythematosus, Bazin's disease, and a chronic arthritis. Many observers in this country, Barber in particular, have called attention to the co-existence of lupus erythematosus and arthritis, but the further addition to the complex of Bazin's disease must be extremely rare. The second point is concerned with the benefit resulting from the administration of the vaccine. There is no proof of any form of infection with the organisms used; their effect should therefore in this case be attributed to the injection of a non-specific protein.

Dr. Fordyce said that in his clinic two cases of subcutaneous nodules associated with lupus erythematosus of the face had been observed. Several nodules had been excised, but their histological examination did not reveal more than a chronic inflammation. Lupus erythematosus was one of the mysteries of dermatology and it was well worth while to study cases associated with Bazin's disease and the tuberculides. Dr. Cannon, his assistant, had been carrying on certain investigations in the Pathological Department of Columbia University, including tissue from cases of lupus erythematosus inoculated into guinea-pigs. Out of thirty cases inoculated, typical tuberculosis developed in five. Therefore one should not say that certain types of lupus erythematosus were not connected with tuberculous infection. At one time he thought lupus erythematosus in no way was related to tuberculosis but in later years he had become more conservative in his opinion.

Case of Mycosis Fungoides, with Microscopic Section.

By KENNETH WILLS, M.B.

THE patient, a male, in this case first came under my observation in September, 1920; Dr. Harvey, of Bristol, sent the case to me as a case of eczema. At that time I thought it might be pre-mycotic mycosis fungoides, or that it might be erythema multiforme, or possibly dermatitis herpetiformis. There were raised vesicles and pigmentation. During three years various methods of treatment have been adopted, and what has done most good is high rectal lavage associated with sunlight baths. But though the condition has abated it has recurred.

At the end of last year a tumour developed on the left side of his nose. I had a small piece cut out, and Dr. Hadfield had these photographs made; they show the characteristic picture of mycosis fungoides. With the development of these tumours the glands began to enlarge, and all the glands in the body are affected. The spleen was just palpable below the costal margin. That rather disposes of the statement made in books, that in mycosis fungoides the glands are only enlarged as a result of septic absorption from tumours and ulcerations. With the development of the tumours, the pre-mycotic stage has almost disappeared, and the only place where it can be seen is on the hands. The tumour on his nose disappeared with a three-quarter pastille dose, and another, which seemed about to fungate, has also disappeared after X-ray treatment.

Dr. J. H. SEQUEIRA said he referred to glandular enlargement in his survey of the subject in 1914.[1] He had seen enlarged glands in mycosis fungoides but he would not have associated such enlargement with sepsis. Septic conditions in this disease occurred very late and were now rare. Since X-rays had been found to control the disease, the horrible fungating conditions seen in former days had fortunately disappeared. He thought there might be some confusion with leukæmic conditions in which the glands were enlarged. This case was of interest as at one stage there had been an eruption which was suggestive of dermatitis herpetiformis, a point to be remembered in the future.

Case of Prurigo Nodularis.

By H. W. BARBER, M.B.

M. W., FEMALE, aged 31, first consulted me in June, 1920, on the advice of Dr. Milton, of Eltham, for an intensely itchy, papular and nodular eruption on the arms, backs of the hands, and legs. The eruption first appeared at the age of 21, some ten years ago, after exposure to the sun, it being at that time "similar to nettle-rash" and very irritating. It gradually disappeared, but after a while returned on the arms and legs, forming "hard, red spots, sometimes discharging." It was always more intense on the legs. Since its reappearance in this form she has never been free from it, fresh papules and nodules occurring from time to time, particularly in hot weather. At the age of 16 the patient was under treatment for cough and fatigue; she was told she had weak

1 "Discussion on Mycosis Fungoides," *Proceedings*, 1914, vii (Sect. Derm.), pp. 190-204, 234-36; *Brit. Journ. Derm.*, 1914, xxvi, pp. 213 *et seq.*

lungs and was recommended for sanatorium treatment. In August, 1919, she was treated at a tuberculosis dispensary. One of her brothers died of tuberculosis.

When I first saw her in 1920 I thought the lesions were tuberculides, particularly as on the legs there were deep necrotic nodules resembling erythema induratum. Moreover there were signs suggesting active tuberculosis of the lungs, and the patient had evening pyrexia. She was treated with small doses of novarsenobillon, given intravenously, and later with tuberculin, which appeared to provoke some focal reaction in the skin lesions.

I later realized that the eruption was not a tuberculide, but prurigo nodularis. The patient was for a time an in-patient at Guys Hospital, for investigation. She was examined by Dr. Marshall, who thought that although she had certainly had tuberculosis of the lungs, there was little evidence of active disease at that time. X-ray examination of the alimentary tract revealed considerable delay in the lower ileum and colon, with general visceroptosis. The urine was normal, except for excess of indican. The Wassermann reaction was negative.

Apart from treatment with tuberculin and arsenic the eruption has been treated with fairly large doses of X-rays with some benefit. Many of the nodules have become flattened after exposure to the rays, and the itching is much less than hitherto. Her general health, too, has much improved, but she suffers from periodical attacks of severe indigestion, which rather suggest a chronically inflamed appendix, as there is marked tenderness in the right iliac fossa. She is now being treated by injections of enesol.

The eruption is situated chiefly on the forearms, extensor surfaces of the upper arms, backs of the hands, and on the legs below the knees. The lesions consist of papules and nodules. A considerable number of the nodules have had verrucose summits, and at times on the legs there has been vesiculation with secondary infection giving rise to much pain and swelling. In my opinion prurigo nodularis is not the same condition as lichen obtusus corneus, nor is it a form of ordinary lichen planus.

DISCUSSION.

Dr. FORDYCE said he was sure Dr. Barber was right in his diagnosis. He believed the first case reported in America was one by the late Dr. James C. Johnston. He had studied the case very carefully with Dr. Johnston and in its clinical features it conformed very accurately with the one shown by Dr. Barber. The sequence of development seemed to be an urticarial lesion attended with itching resulting in a persistent nodule. The individual reaction was probably a factor in the condition.

Dr. A. WHITFIELD asked if this were not a case of what was known in France as "Prurigo à papules grosses"? He believed it began either as an urticarial papule, or a localized itching that caused scratching and the secondary development of an urticarial papule, the patient later digging out pieces. He agreed it was not lichen obtusus corneus.

Dr. A. M. H. GRAY said he was still inclined to think the condition now shown was closely related to lichen planus. The curious case he had shown before the Section two years ago was extremely suggestive of the relationship of this disease to lichen planus.[1] That patient had lesions of several types; on one side the lesions were exactly of the type which were seen in this patient. On the other side there were lesions of twenty-five years' duration, which all present admitted was verrucose lichen planus. Microscopically, the early lesions on the right side did not show any more typical signs of

[1] *Proceedings*, 1921-22, xv (Sect. Derm.), p. 13.

lichen planus than the cases which had just been described and he still thought one could not base an absolute and final diagnosis or opinion on a microscopical examination.

Dr. L. SAVATARD said that a short time ago he had a case of what he considered to be prurigo nodularis in a man, and the patient responded very well to X-ray treatment. He had no lichen planus lesions in evidence. While he was in hospital, fresh lesions appeared on the back of the hands as a result of scratching. He (Dr. Savatard) had a blood-sugar estimation carried out, and the result showed great excess; there was no glycosuria.

Case for Diagnosis.

By J. M. H. MACLEOD, M.D., and J. E. M. WIGLEY, M.B.

PATIENT, a female, aged 49, employed as a cook. She was well nourished and generally healthy. There was no suggestion of tuberculosis in herself or history of it in her family, nor were there any signs of focal septic trouble.

The eruption consisted of dusky red macules and patches and was distributed irregularly over the whole cutaneous surface, with the exception of the palms, soles and scalp. The lesions varied in size from that of a split pea to irregular patches several inches in their long axis, and in shape from circular at first, to assume irregular shapes later, partly from irregular peripheral extension and partly from coalescence with neighbouring lesions. At first they were slightly raised, soft to the touch, and on pressure with the diascope left a definite brown stain. Later they became flat, possibly slightly atrophic, and the staining on diascopy became less definite. Here and there on the surface the lesions presented telangiectasis, but this was not a constant feature. The original lesion appeared in 1915 on the left arm and face, and since then lesions had gradually developed until the eruption had assumed its present extent about a year ago. Once a lesion appeared it showed no tendency to spontaneous involution. Patient has been under the observation of the exhibitors at irregular intervals since 1915.

About 1916, when the lesions were coming out actively, they showed a tendency to grouping on the back, suggesting a corymbose syphilide, and so close was this resemblance that the original condition was originally diagnosed as a late syphilide.

A biopsy was performed, a recent lesion on the back being excised. This showed no definite changes in the epidermis, but in the corium there were peculiar patches of partially degenerated fibro-cellular stroma. These were situated chiefly about the sweat apparatus and the hair follicles, but were also present in the subcapillary layer of the corium. The blood-vessels in the subcapillary layer were dilated, but there was no inflammatory infiltration surrounding them. The degenerated patches consisted of an irregular network of attenuated collagen fibres with numerous small irregular degenerated connective tissue cells. There were no plasma cells nor definite giant cells, but the cells were so changed by the degenerative process that it was difficult to identify them.

Wassermann reactions were done on two occasions with negative results. Patient was also given three courses of salvarsan. This seemed to cause slight fading of the lesions, but there was no very marked change. Clinically the condition strongly suggested widely distributed sarcoid.

Dr. A. WHITFIELD said that what the section now showed was a degenerative infiltration, as if the main attack of the inflammation was over, and resolution was in

progress. He said that because there was a stellate vacuolation between the cells, as if they were liquefying. Although not now characteristic, he thought the histology was by no means incompatible with that of sarcoid. In this case the clinical diagnosis of sarcoid was tempting, and the lesions certainly appeared to be sinking down and resolving, with resulting atrophy.

Case of Anetodermia Maculosa.

By H. W. Barber, M.B.

Patient, a male, aged 49, came to me in August of this year on account of an eruption on the back. He had first noticed it about eighteen months previously. There were present on his back, particularly over the shoulders and on the outer surfaces of the upper arm, numerous patches of anetodermia, some showing the typical ballooned, herniated appearance. One was also present on the left leg.

The patient cannot remember having had any eruption preceding the appearance of the atrophic patches. There is no history of syphilis, and the Wassermann reaction was completely negative.

The case, clinically, would appear to correspond to that described by Jadassohn; and recently Pardo-Castello has published a case in a Cuban, in which he ascribed the atrophy to a previous syphilitic infection.

DISCUSSION.

Dr. A. Whitfield remarked that the patient volunteered the statement that the condition began with small boils, but he (Dr. Whitfield) questioned the accuracy of that. If it were true, the case would be one type of acne. They were all familiar with this type of atrophy left in some cases of acne, but he had been unable to ascertain whether it was due to a peculiarity of the patient's skin or to an unusual infection.

Dr. Barber (in reply) said that what the patient described as boils were really little balloonings. He did not think the patient had ever had acne.

Dr. E. G. Graham Little (President) said he had shown a like case with a similar distribution in a patient with extensive lupus erythematosus and without any history of syphilis. He believed that Dr. Adamson had also shown a case in which syphilis was excluded.

Two Cases of Exfoliative Dermatitis with Intense Pigmentation.

By H. W. Barber, M.B.

I have had under observation at St. James's Infirmary two cases of generalized exfoliative dermatitis (pityriasis rubra) accompanied by intense pigmentation of the Addisonian type.

Case I.—Male, aged 71. Admitted June 2, 1922. Died November 24, 1922. The condition had been coming on for three months. He had been under treatment at the London Hospital.

On admission to the Infirmary, exfoliative dermatitis was present, but on July 7, 1922, the skin had become noticeably darker; and from that date the pigmentation became steadily more marked.

August 21, 1922. *Blood count:* Red corpuscles, 3,380,000. No nucleated reds. No alteration in size. Hæmoglobin, 74 per cent.

White corpuscles, 8,200; polymorphonuclear neutrophils, 80 per cent.; eosinophils, 7 per cent.; basophils, 0·5 per cent.; lymphocytes, 10 per cent.; transitionals, 2·5 per cent.

October 1, 1922. *Blood count:* Red corpuscles, 4,100,000. No erythroblasts. No alteration in size.

White corpuscles, 14,600; polymorphonuclear neutrophils, 71·5 per cent.; eosinophils, 4·5 per cent.; basophils, 2·0 per cent.; lymphocytes, 20·5 per cent.; hyalines, 0·5 per cent.; transitionals, 1 per cent.

Post-mortem notes.—Suprarenals healthy. Some enlarged glands lower end of abdominal aorta. No malignant disease. Granular kidneys.

Case II (present case).—Male, aged 52. Admitted September 25, 1923. Sent from St. John's Hospital for Diseases of the Skin as a case of Addison's disease. Suffered from eczema in early childhood, and then again when about 12 or 14 years old. At that time his friends commented on his fine complexion.

His present illness commenced four months ago; it was called eczema. The skin peeled off three times. His blood pressure is 110. He has lately become mentally clouded. There are two spots of pigmentation inside the right cheek.

DISCUSSION.

Dr. G. PERNET, referring to the second case, asked if the pigmentation followed the exfoliative condition. Years ago he had a case of pityriasis rubra, in which pigmentation all over the body followed recovery.

Dr. E. G. GRAHAM LITTLE (President) asked whether the skin remained pigmented when exfoliation had taken place.

Dr. BARBER (in reply) said that the patient was not clear as to whether the pigmentation followed the dermatitis. After exfoliation had taken place, the skin beneath was equally pigmented.

Case of Possible Lichen Planus Obtusus Corneus.

By E. G. GRAHAM LITTLE, M.D. (President).

THE patient is a man about 45, with some patches of true lichen planus upon his limbs, and very numerous raised patches with a flat surface, about a quarter to half an inch in diameter, very horny in substance and recalling the description given by Brocq in the "Pratique dermatologique" under this title. These raised disc-like patches are freely present on the legs and thighs, but not above the pelvis. On the left leg there is a large patch of hypertrophic lichen planus about three inches by two, which is, however, quite unlike the sharply margined discs described above.

Case of Aleukæmic Lymphomata.

By E. G. GRAHAM LITTLE, M.D. (President).

THIS patient promised to come up from Folkestone but has not been able to do so. She is a woman of about 50 who during the last few months has developed a large number of flat blue circumscribed indurated patches, varying in size from that of a sixpence to that of a florin, and distributed chiefly on the

abdomen and back. The blood count is normal. Sections from one of the patches show typical lymphatic tissue very like that found in the case shown by the late Dr. Lee Dickinson and myself many years ago, but the colour of the nodules in that earlier case was bright yellow,[1] whereas the colour in the present case much more resembles the appearance seen in a case shown by Fox and Rolleston and fully described in the *British Journal of Dermatology*, 1909, xxi, pp. 259-61; 377-85.

Case of Trade Argyria.

By H. G. Adamson, M.D.

This is an example of a condition which, I think, is not now often seen. Formerly it was more common as the result of the administration of silver pills in the treatment of epilepsy. In those cases the coloration was blood-carried and universal but most pronounced on those parts exposed to light. The present case is, I think, an example of local argyria, the result of absorption by the skin, or of what has been called trade argyria, which occurs sometimes in those who work with powdered silver. This patient has for many years been occupied in making nitrate of silver. The pigmentation seems to be confined to the parts about the mouth and is particularly marked in the naso-labial furrows. Similar discolorations of the conjunctivæ are, I believe, known to ophthalmologists as the result of local applications of silver nitrate. It appears that the silver is deposited in the sub-epithelial margin and not in the epithelium, which explains the fact that it is never exfoliated but remains permanent. In trade argyria the silver particles are said to be deposited directly in the skin and to form small masses sometimes as large as a hemp seed. In this case the infiltration is more uniform and I am not prepared to say exactly in what way it has reached its destination. The patient states that the skin of the hands and face becomes splashed with silver nitrate and darkened (by exposure to light). This wears off, but leaves the skin around the mouth pigmented, but not, as we see, the skin of the hands.

Case of Boeck's Sarcoid.

By A. M. H. Gray, C.B.E., M.D.

The patient is a young man of 32. A swelling appeared two years ago on the centre of the forehead and has now attained the size of a cherry. There are several smaller nodules scattered about the face and extremities, but there are none on the trunk. The lesions first appear in the deeper layers of the dermis and slowly increase in size. They are first of pale brownish colour, not raised above the surface and easily palpable, but later they become elevated and of a much darker brown tint. In the large nodule on the forehead you can see yellowish nodules, and if you test them with a sharpened match you find they do not yield like lupus nodules, but are intensely hard. There is a section of one of the lesions under a microscope, and it shows the characteristic appearance of deep-seated collections of epithelioid and giant

[1] *Brit. Journ. Derm.*, 902, xiv, p. 219.

cells, running through the deeper layers of the dermis. The Wassermann and von Pirquet reactions are negative; blood count normal. A nodule was inoculated into a guinea-pig six weeks ago and the animal is still alive and shows no evidence of tuberculosis. During these past six weeks the patient has been having small doses of neosalvarsan, and there is a slight tendency to improvement.

I take it that this is a typical example of the nodular type of Boeck's sarcoid and differs clinically from the disseminated type, two examples of which have been shown to-day, and also from the miliary type, which in my opinion is the same as Crocker's "acne agminata."

Case of Cutaneous Calcinosis.

By A. M. H. Gray, C.B.E., M.D.

The patient, a girl, aged 18, has been developing swellings on the skin for two years. The first appeared on the right wrist, and since then one has developed on each elbow and recently two have appeared on the fingers. They seem to be of inflammatory origin, and contain white nodules, which tend to come to the surface. When opened, they are found not to be solid, but to contain a milky fluid; under the microscope this is found to be full of amorphous particles. I cannot yet tell you the chemical composition, but urates are absent. The nodules are quite opaque to X-rays. The blood cholesterol is in excess of the normal by 10 per cent., but the patient has no hyperglycæmia. There is no other illness, but apparently she has slight Raynaud's symptoms. I hope to report on the case when further investigations have been carried out.

Case of Arsenical Dermatitis with Granulomatous Nodules.

By A. M. H. Gray, C.B.E., M.D.

The patient is a man, aged 71, whom I first saw six weeks ago. He then had very extensive erythrodermia, covering all his body except portions of the back, neck and scalp. He also had intense pigmentation in the areas affected, and marked keratosis of the palms and soles, with small keratomata. He also had curious nodules, some as large as a bean, scattered over the whole affected area. They were infiltrated, and had appearances suggesting a granulomatous nature. He states that in 1912 the eruption began with patches on the leg, which spread, and involved both legs and both arms. About 1920 he was put on to 6 minims of liquor arsenicalis three times a day; he took this more or less continuously for two years, and the bulk of the eruption has developed since then. The main eruption is undoubtedly arsenical dermatitis with intense pigmentation, and arsenical keratosis. The question however arises as to whether these granulomatous nodules are part of the arsenical condition, or a definite granulomatous process which has been masked by arsenic. I have a section of a nodule under the microscope, but it does not suggest anything very definite. I thought it might possibly be mycosis fungoides, but, as the itching is so slight, that disease is rather contra-indicated.

DISCUSSION.

Dr. A. WHITFIELD said he had a case sent to him, years ago, in whom he first made the diagnosis of erythema multiforme. There were patches on the knees and elbows the size of a florin, and others the size of a sixpence, slightly desquamating, much infiltrated, and of a livid pink. As the condition was found to have lasted two years, he had to reconsider his first diagnosis of erythema multiforme. It was then found to be arsenical. It was soon after the Manchester arsenical epidemic, and the patient had the peculiar type of pigmentation described by Brooke which could be scraped off with the horny layer. The present patches he regarded as arsenical; they had the same target-like appearance and were depressed in the centre, as if they were resolving lesions of erythema multiforme.

Dr. E. G. GRAHAM LITTLE (President) recalled a case which he had shown, of a dermatitis of the hands, which in the view of several Members of the Section was a lupus erythematosus, but which Dr. Whitfield considered to be an arsenical eruption; and this was proved to be the case, as the man had been working with arsenical weed-killer. The termination of this case was extremely interesting, for he developed very septic vegetative growths in the groin and axilla suggesting pemphigus vegetans, and this condition proved fatal.

Case of Keratodermia Blenorrhagica.

By J. H. SEQUEIRA, M.D.

PATIENT, a male aged 38, was admitted to the London Hospital on September 9, 1923. His general health had been good except for an attack of "rheumatism" in 1913. On inquiry at St. Bartholomew's Hospital it was found that this joint affection was a unilateral synovitis of the right knee. This condition entirely cleared up under treatment and he was able to follow his occupation. The patient denies having had either gonorrhœa or syphilis.

The present illness began in January, 1923, when he noticed a difficulty in walking, the knee-joints being stiff. In June, 1923, his right hand became painful and the wrist difficult to move. In July a lump as large as a small nut appeared on the back of the right hand, and shortly afterwards lumps appeared on the legs from the knee to the dorsum of the foot. Gradually, at the same time the joints became stiffer and he could not use his arms and legs, and he had been confined to his bed for two months before his admission to the ward.

The patient, on admission, lay in bed with his neck fixed, the head bent forward, the thighs flexed on the abdomen and the knees flexed beyond a right angle. The elbows were bent and fixed and the wrists and fingers were greatly deformed, chiefly in a position of over-extension. The jaw was also stiff. The patient could not feed himself or lift himself.

At the heart's apex there was a systolic murmur but the second sound was quite clear. The lungs showed no abnormality. Both pupils were irregular in shape but reacted to light.

The skin eruption consisted of rounded dome-shaped nodules on the deformed toes, of a dark brownish colour. There were also numerous nodules varying from the size of a cob-nut to that of a pea on the dorsum of both feet and the front of the legs up to the knees, and on the tibia a limpet-shell-like mass. The soles were quite free. Smaller lesions with crusting were on the fingers and backs of the hands and wrists. There were small papulo-scaly lesions on the penis. The Wassermann reaction was negative.

Mr. Goulden examined the eyes and found there were extensive posterior synechiæ on both sides. Iris bombé, tension normal.

The examination of the urine showed a trace of albumin, and a few granular casts and uric acid crystals. No gonococci were found in the urine or in the prostatic fluid. Cultures yielded *Staphylococcus albus* only. Blood cultures were sterile. The blood count showed no abnormality.

Owing to the immobility of the patient cystoscopy was difficult, but Mr. Clarkson, in Mr. Hugh Lett's clinic, was able to make a complete examination and found swelling of the right vesicle which was also tender. The prostate yielded a small quantity of fluid (examination for gonococcus again negative). Urethroscopic examination revealed a somewhat congested mucous membrane, an incomplete degree of dilatability particularly in the bulbus urethræ, fibrous thickening on the roof of the bulbous portion of the urethra which will readily develop into a stricture.

A complement-fixation test was not done as the exhibitor understands its value is doubtful.

The result of Mr. Clarkson's examination proves the case to be gonorrhœal in spite of the patient's denial. Further, the polyarthritis, iritis and the peculiar type of cutaneous lesions form a characteristic symptom-complex. The patient is being treated by gonococcus vaccine, and by local measures for the removal of cutaneous lesions. The joint deformity is, however, so great that recovery appears to be extremely doubtful.

There has been no pyrexia while the patient has been in hospital.

Dr. E. G. GRAHAM LITTLE (President) said he had collected a large number of these cases some years ago, in one of which there had been an interval of thirteen years between the last attack of urethritis and the development of the keratodermia. It was remarkable in how many of the cases long intervals had elapsed between the onset of active symptoms of urethritis and the appearance of the keratodermia.

Case of Xanthoma Diabeticorum.

By J. H. SEQUEIRA, M.D.

PATIENT, a police constable, aged 36. He is of plethoric habit and he has had no illness until ten weeks ago when he developed an eruption, xanthoma diabeticorum, on the extremities. The urine contains a large quantity of sugar; there is hypoglycæmia and there is also cholesterinæmia. There is no acidosis. We intend to treat him with insulin, which is being used in another case, in which the xanthoma is already abating.

Case of "Multiple Symmetrical Cutaneous Cysts" or "Steatocystomata."

By HAROLD ORR, M.B.

PATIENT, a male, aged 25; of Jewish extraction. He suffered from acne vulgaris at 15, but was otherwise healthy until two years ago when the "lumps" for which he now seeks relief began to appear. They have been increasing in number ever since and none have disappeared.

There are several hundred tumours symmetrically grouped over the chest and upper abdomen, in the axillæ and on the margin of the chest in front of

the axillæ, in front of the elbows, behind the knees, between the buttocks; there are half-a-dozen scattered over the back and several in the supraclavicular fossæ. They do not irritate, are painless to the touch, freely movable over the subcutaneous tissue and they feel like gelatin capsules full of fluid. They vary in size from being just perceptible to a quarter of an inch in diameter. The contents on gross and microscopical examination have been found to consist entirely of fat droplets; cells, cellular débris and micro-organisms could not be demonstrated. The appearance of the contents varies from a likeness to clear olive oil in the largest cysts to that of a whitish cheesy material in the small ones. Six of the cysts suppurated and have left cicatrices. Blood examination showed a leucocytosis of 11,280, the differential count being normal; and the Wassermann reaction negative.

Four sections are shown of a cyst removed from the right forearm. In one section there is a complete cyst the wall of which consists of Malpighian epidermal cells. Upon the upper extremity of the cyst a portion of a sebaceous gland is situated; the lower extremity is continuous with a tortuous portion of hair follicle in which there is a section of the shaft of a hair. The cyst is evidently formed by sebum dilating the deep part of a hair follicle.

I believe this is the first case of its kind that has been shown before the Section. The late Dr. Pringle,[1] however, described a case which was precisely similar clinically. He and the late Sir James Galloway thought that the cysts were due to the hypertrophy of the sebaceous glands, and the liquefaction of its contents. Bosellini's[2] findings are in agreement with our own.

Serial sections are being made by Professor Turnbull, and we hope later to publish a full description of the case with drawings.

About 150 cysts on the chest wall were evacuated three weeks ago and they showed no evidence of re-forming.

Dr. H. W. WILSON said he saw Dr. Savatard's case. Serial sections were cut, and showed a dipping-in from the surface, and the cysts contained degenerated superficial epithelial cells, thus differing from Dr. Orr's case. He saw a similar case in Edinburgh, and the cut sections of that were exactly similar; they gave the appearance of yellow loose bodies under the skin. When the section in the older lesions was being made, the cyst underneath separated from the skin, and the neck of the inclusion cyst broke across when removed.

Case of ? Lichen Planus Hypertrophicus in a Man aged 54.

By S. E. DORE, M.D.

THIS patient's eruption is chiefly on the thighs, the arms, the legs and the ankles. He has had it three or four years, and when he was first seen a month ago he had a definitely scaly, inflammatory, nodular eruption of a brownish colour. I do not think the eruption is Darier's disease, though that diagnosis has been made by several members. I made the diagnosis of lichen planus because the lesions are superficial whereas the follicular plugs of Darier's disease are absent, and because the eruption has changed so rapidly. Since last week it has to a large extent disappeared; it has become flatter and more pigmented, and the clinical picture has completely altered. Darier's disease does not undergo involution in this manner.

[1] *Brit. Journ. Derm.*, 1899, xi, p. 381.
[2] *Archiv. f. Derm. u. Syph.*, 1898, xlv, p. 81.

Case for Diagnosis.

By WILFRID FOX, M.D.

PATIENT, a male, aged 47, came to the out-patient department with the nails affected as now shown. I had no doubt it was a case of ringworm of the nails, but failed to find the fungus in spite of repeated search. Dr. Hunt, our bacteriologist, was also unable either to find the fungus or to obtain a culture. However so convinced was I that it was a case of tinea that I removed the nails, scraped the base, painted them with iodine, and later applied dressings of dilute mercury ointment. They all re-grew exactly as before. Have others had the same difficulty in confirming the diagnosis, and if so, is it generally agreed that it is a case of tinea, and can any alternative treatment be suggested? The patient has done a certain amount of photography, but some of the toe-nails were also affected, so it is difficult to see how the condition could be due to chemical irritants.

DISCUSSION.

Dr. H. C. SEMON thought the fact that the disease had recurred in the same form in all the nails, after avulsion, and in spite of the careful after-treatment, supported the negative microscopic and cultural findings—in other words, this was not a case of tinea unguium. In a case which he had demonstrated to the Section, operated on, and eventually published (*Brit. Journ. Derm.*, vol. xxxiv, December, 1922), the disease also recurred, but not in all the nails. The microscopic finding in this case left no doubt as to the mycelial ætiology. Dr. Gray operated at a later date and succeeded in eradicating the infection in all but one or two fingers.

Dr. A. WHITFIELD said that in view of what Dr. Fox had said, this was probably not ringworm. The patient had done a great deal of photography, and it might be metal poisoning. The difficulty was that the nails of the toes were also affected. He rather thought however, that if the matrix of the finger-nails were intoxicated, the changes might also occur in the feet.

Section of Dermatology.

President—Dr. E. G. Graham Little.

Multiple Superficial Basal Cell Carcinoma.

By J. M. H. MacLeod, M.D., and J. E. M. Wigley, M.B.

THE patient, a woman aged 49, had three superficial basal cell carcinomata, one behind the right ear, and two on the back, the largest, which appeared as a slightly raised, circumscribed patch, about 1½ in. in diameter, was situated in the interscapular region. The lesion behind the ear was treated with radium and completely disappeared, with very little scarring. The large lesion on the back was treated experimentally with X-rays, to which it did not respond so satisfactorily as the post-auricular lesion had done to treatment with radium, and it was now proposed to treat both the lesions on the back with radium.

A microscopical examination of the smaller lesion on the back showed it to be an early basal cell carcinoma.

Dr. Graham Little (President) said he believed that the case was a type which, in a paper read at a meeting of the British Dermatological Association in Liverpool, he had endeavoured to name "erythematoid benign epithelioma." The curious benignity of the cases seen up to that date had been a notable feature. Since the Liverpool meeting, however, he (the speaker) had had a case of the same condition in a man aged 38, who had had two patches, both of them of several years' duration. Within the last four months a very sudden change had taken place in one of these patches in the form of rapid fungating ulceration with enlargement of corresponding glands. This patch had been widely excised and had been found to have undergone a transformation to a malignant type of squamous-celled epithelioma. The development of malignant types of epithelioma upon injured skin—for example, lupus vulgaris—was well recognized, and this transformation from a completely non-malignant type might be explained in the same way.

Lichen Atrophicus.

By J. L. Bunch, M.D.

THIS girl, aged 21, has a rather extensive eruption, chiefly on the trunk. The disease first began on the arm. First there were one or two ill-defined papules on the wrist, which seemed to resemble lichen planus; but now they have more or less involuted. The lesions on the trunk are of the erythematous group, and in many places are definitely atrophic. The lichen planus lesions were never well defined. She has not complained of irritation or itching, and she has had no lesions in the mouth, except a little ill-defined erythema of the

tonsils. There is an indefinite history of headaches. There are one or two enlarged post-sternomastoid glands, but none in the groin. I have examined her with a speculum, and there is no trace of a primary lesion. However, I am now having a Wassermann test made. I do not think there is much evidence that the eruption is a tuberculide, except the grouping of the lesions.

DISCUSSION.

Dr. G. PERNET said he regarded the eruption as a small follicular-grouped syphilide. It was well known that sometimes syphilides became atrophic. In this case it was an interesting point that the disease had been going on unchecked for three or four months, and apparently the patient had had no treatment up to the time of seeing Dr. Bunch.

Dr. J. H. SEQUEIRA agreed with Dr. Pernet's diagnosis.

Case of Sarcoma Simulating Mycosis Fungoides.

By W. KNOWSLEY SIBLEY, M.D.

THE patient, A. S., an electrical engineer, aged 64, was sent to me by Dr. D. L. Morgan on October 15, 1923.

About five years ago he developed a rather irritable condition of the skin in the dorsal region, and some papules, like small grapes, appeared and then disappeared, and, during the past twelve months, an irregular papular growth had slowly developed in this region. There had never been any trouble on any other part of the body.

He presented a large, very irregular, nodular growth, distinctly purplish in colour and measuring about 6 in. by 5 in. One of the nodules had recently broken down and was discharging, and had bled considerably on two occasions. A photograph taken at the time shows the condition.

I administered a full dose of X-rays, filtered through 6 mm. aluminium and felt, to the bulk of the area of disease, on October 16.

A marked improvement in the local condition has since occurred, a considerable part of the raised tumour formation has disappeared, and the area of ulceration has partly healed.

The case is of special interest from the fact that there have been no premycotic symptoms on any other part of the body, a local tumour formation being the only external physical sign of this disease, and the general health has not been affected.

Dr. Arthur Young, who examined the specimen microscopically, says: "The histological picture suggests that of a lesion of mycosis fungoides in the proliferative stage, when degeneration and ulceration are only just commencing."

DISCUSSION.

Dr. WHITFIELD said he thought, judging from the clinical and microscopical examinations in this case, that Dr. Sibley's original diagnosis was correct and that of the pathologist wrong. There seemed to be a good deal of evidence to support this view though none of it was quite conclusive, because sarcoma and mycosis fungoides were so closely allied.

Clinically, the tumour was much harder than was often the case in mycosis fungoides; in that disease, though the early and slight infiltrations might be rather resistant to the touch, as soon as a well defined tumour was formed it was almost diffluent in consistency. Secondly, if this was a case of mycosis fungoides it was an advanced case; and yet there was only a single lesion. This would be a very rare, if not indeed a unique occurrence.

Turning to the histological side he (Dr. Whitfield) would like to emphasize the fact that this was not one of the very early cases where scarcely any change was noticeable. There was plenty of change present but it resembled that of sarcoma, not of mycosis fungoides. In this case there was plentiful new growth in the depth of the pars reticularis, and the coarse strands of fibrous tissue were separated by an embroidery-like network of infiltration. The pars papillaris on the other hand was almost unaltered and showed scarcely more than the normal number of cells. In his (Dr. Whitfield's) experience, and according to other writers who had studied mycosis fungoides, very heavy infiltration of the papillary body was among the earliest changes and persisted up to the full development of the tumours.

The reaction to X-ray treatment was unfortunately of no assistance, since both diseases reacted favourably to it.

Dr. J. H. SEQUEIRA said that he recalled two cases which he had demonstrated to the Section.[1] In one of these a man had multiple mycosis fungoides tumours and generalized erythrodermia; the other was almost identical with Dr. Sibley's case, the patient having had a very characteristic horseshoe-shaped tumour on the back. He (the speaker) had demonstrated these cases as representing two very different types of mycosis fungoides. He had treated them both with X-rays, and in the second case, that of the limited tumour, the patient was entirely cured by repeated doses of X-rays; there had been no recurrence. In the other case the tumours disappeared entirely under treatment, but recurred, again and again, and the patient ultimately died. He (Dr. Sequeira) was, therefore, inclined to revise his diagnosis and to conclude that the single tumour was probably sarcoma. He had seen other cases of undoubted sarcoma cured by X-ray treatment.

Dr. H. MACCORMAC said that this question could be solved by the application of a test originally devised in the Cancer Research Laboratories of the Middlesex Hospital—the staining of the tissue for Altmann's granules. Dr. Beckton had demonstrated that Altmann's granules were absent in malignant tissues, such as carcinoma or sarcoma, but were present in the cells in inflammatory lesions. In this way one could arrive at a distinction between malignant and non-malignant conditions. Some years ago Dr. Beckton had stained sections from cases of mycosis fungoides for him (the speaker), and it had been found that the cells contained Altmann's granules in abundance. He (Dr. MacCormac) had mentioned this at a meeting of the Section held before the war, in connexion with a case shown, he thought, by the late Dr. Pringle.[2] The technique was somewhat difficult, and must be carried out by someone thoroughly familiar with the process.

A Case of Sclerodermia.

By Sir PERCY BASSETT-SMITH, K.C.B., C.M.G., R.N.

PATIENT, a married woman, aged 33.

Family history: No syphilis. No tubercle.

Previous history: Appendicitis and peritonitis two years ago.

Duration of disease: Two years. Treatment at St. John's Hospital, Lewisham, began September 4, 1923.

Condition: Diffuse sclerodermia both arms, excluding hands, shoulders, upper part of chest and back, neck, extending over lower part of face, also slight brawniness of thighs. Wassermann negative.

Treatment: Thyroid extract from ½ gr. t.d.s. to 2 gr. t.d.s. (present dose), with massage and oil inunctions.

The patient says that her condition is improving.

[1] *Brit. Journ. Derm.*, xxii, pp. 23-26.

[2] *Proceedings*, 1914, vii (Sect. Derm.), p. 225.

Two Cases of Hydroa Æstivale with Notes of Three Other Cases.

By H. W. Barber, M.B.

Case I: Boy, aged 7 (patient now shown). No family history of the complaint. The lesion first appeared in the summer of 1922 as a papulo-vesicular eruption on the knees and the face, and later on the backs of the hands. Extremely irritable. Subsided in the winter, but did not disappear completely, and became worse again during the summer of 1923.

Urine: Indican (only slight). Spectrum of hæmatoporphyrin present after urine has been standing.

Fæces: Bacteriological flora not strikingly abnormal. On a full diet: Guiac positive. Hæmatoporphyrin well marked on three examinations. On a meat-free diet: report not yet to hand.

Test meal: Acid secretion normal.

X-ray examination: Showed no evidence of stasis or other abnormality.

Case II: Boy, aged 14 (patient now shown). No family history of "summer eruption." Began after an attack of "influenza" with sore throat when patient was aged 12½. Eruption appeared on the face, backs of hands and knees. Became far worse in the summer, but the following Christmas he improved whilst in bed for an attack of tonsillitis. When seen had typical "summer eruption" on face, ears, backs of hands, and on the knees between the areas covered by the knickerbockers and stockings.

Urine: Indican + +. No hæmatoporphyrin found.

Fæces: Bacteriological flora apparently normal. On a full diet: Guiac test weakly positive. Hæmatoporphyrin present in considerable amount. On a meat-free diet: Hæmatoporphyrin still present in considerable amount.

Test meal: Acid secretion rather higher than normal; rapid emptying.

X-ray examination showed dilated cæcum and ascending colon.

Case III: Female, aged 34. No family history. Confinement two years ago. Rash appeared afterwards on the face, backs of hands, V-shaped area left by the cut of the blouse, and on the arms uncovered by the sleeves. Rash provoked by exposure to sun and wind.

Urine: Indican + +. No hæmatoporphyrin.

Fæces: Bacteriologically normal. On a full diet: Guiac positive. Hæmatoporphyrin well marked. On a meat-free diet: Guiac negative. No hæmatoporphyrin.

Test meal: Complete achlorhydria.

X-rays: Marked ptosis and hypotonus of stomach and moderate ptosis of the cæcum.

Case IV: Female, aged 18. No family history. Eruption began apparently when patient was aged 5. Always appeared in the summer, but disappeared in winter.

Urine: Indican (considerable amount).

Fæces: Bacteriologically normal. On a full diet: Guiac positive. Hæmatoporphyrin present. On a meat-free diet: Guiac negative. Hæmatoporphyrin absent.

Test meal: Complete achlorhydria.

X-rays: Stasis in transverse and descending colon.

Case V: Female, aged 31. No family history. Indigestion for many years. Eruption appeared seven years ago on face, neck, ears, backs of hands and arms.

Urine: Nothing abnormal.

Fæces: Bacteriologically—*Bacillus coli*, *Streptococcus longus* and *fæcalis*. On a full diet: Guiac positive. Hæmatoporphyrin present. On a meat-free diet: Guiac negative. Hæmatoporphyrin absent.

Test meal: Almost complete achlorhydria.

X-rays: No stasis or other abnormality.

In conjunction with Dr. J. H. Ryffel, Dr. W. Payne and Dr. F. D. Howitt, I have investigated a series of cases in which there was marked sensitiveness to light. (Hydroa æstivale, summer eruption, &c.). As far as our researches show up to the present these cases may be divided into two groups:—

Group 1.—*The juvenile type* in which the eruption is more severe, as a rule, than in Group 2, and in which hæmatoporphyrin may be formed in such quantity as to cause hæmatoporphyrinuria. In this type the gastric hydrochloric acid is not diminished, and the hæmatoporphyrin is present in considerable quantity in the fæces, *both on a full diet and on a meat-free diet.*

Group 2.—*The adult type* (in one of my cases the eruption apparently appeared first in childhood, but always disappeared entirely in winter), in which the eruption tends to be less severe than in Group 1, and in which hæmatoporphyrin has not yet been detected in the urine. In this type the gastric hydrochloric acid is either completely absent or extremely low (complete achlorhydria has been found in all our cases of this type), and the hæmatoporphyrin is present in marked quantity in the fæces on a full diet, but *completely absent on a meat-free diet.*

It would therefore appear from the cases so far investigated that in the juvenile type there is probably an inborn error of metabolism, whereby hæmatoporphyrin is formed in excess, independently of diet, although the sensitiveness to light may not appear until several years after birth. In a case published by Sir Archibald Garrod, the first urine passed by the child was "red," and at the age of three months a bullous eruption appeared on the face and hands, which disappeared before the end of the first year and left no scars; the teeth were pink and the urine contained hæmatoporphyrin. Later, the typical eruption of hydroa æstivale appeared and left scars.

In the adult type, on the other hand, the hæmatoporphyrin is only present on a meat-containing diet, which would suggest that the hæmatoporphyrin is formed from the hæmoglobin contained in meat. It is possible that the absence of hydrochloric acid from the gastric juice, which has been constant in our cases of this type, is connected with this. Professor Gowland Hopkins is now investigating the possibility of hæmatoporphyrin being formed from hæmatin by the action of intestinal bacteria.

DISCUSSION.

Dr. GRAHAM LITTLE (President) said that Dr. Barber had made an important contribution to a difficult subject. He (the speaker) asked whether members had had much personal experience of finding hæmatoporphyrin in cases of hydroa. He had tried in a number of instances at the East London Hospital for Children but had never been able to associate hæmatoporphyrinuria with this condition. It was an addition to knowledge that hæmatoporphyrin might be present in the fæces and not present in the urine, and so might produce sensitization.

Dr. G. PERNET said that he had a case of hydroa æstivale in a child last summer at the West London Hospital, and as regards hæmatoporphyrinuria the result was negative.

Dr. MACLEOD asked whether Dr. Barber could explain the number of cases of light sensitization in which no hæmatoporphyrin could be found either in the urine or in the blood serum. A relative of his (the speaker's) had an extraordinary sensitiveness to light; she had suffered from this for ten years, and was completely unable to face the open daylight. Every kind of treatment had been tried for it without success, and dietary changes made no difference. The condition had not developed until the patient was in her twentieth year.

Dr. HAROLD ORR said that in the London Hospital there was a patient who had marked cheilitis in the lower lip, with spring catarrh, the latter being well known to ophthalmologists. It was believed to be due to light sensitization. The eye condition of that patient was doing fairly well on radium. Ayres had reported a series of cases in Southern California in which cheilitis was an associated condition. In the case now mentioned there was no hæmatoporphyrin in the urine, but after hearing Dr. Barber's remarks, he (the speaker) would have the fæces examined for it.

Dr. A. M. H. GRAY asked if Dr. Barber had been able to show in the case of adults that when these patients were put upon a meat-free diet they became less sensitive to light, and if he had also found that the hæmatoporphyrin disappeared from the fæces when hydrochloric acid was given by the mouth?

Dr. J. H. SEQUEIRA said that these cases of light sensitiveness could be tested at all periods of the year by exposing them to carbon arc light or to a mercury vapour lamp.

Dr. S. E. DORE said there were all grades of these cases; some were of the prurigo or eczematous type, and others of the type now shown. The cases were not only light-sensitive, they could be provoked by cold winds in winter and possibly by an internal factor such as focal sepsis. He (Dr. Dore) had shown to the Section a child who had suffered from an eruption every summer for five years, and in that case exposures to the ultra-violet rays of a mercury-vapour lamp had not aggravated the eruption but on the contrary had resulted in considerable benefit.

Dr. BARBER (in reply to Dr. Gray) said that he had put his patients of the adult type on a meat-free diet and an hydrochloric acid after meals, but that he could not yet say whether this treatment had any beneficial effect as in most of these cases the sensitiveness to light was slight or absent in the winter months.

Chronic Pigmentary Dermatitis of the Legs.

By H. W. BARBER, M.B.

H. I., MALE, aged 23. The condition of the legs began when the patient was a child, and has gradually progressed. The general health is good.

Appearance: There are some varicose veins on both legs. From the bases of the toes to half-way up the thighs there is a condition which partly resembles ephelis ab igne and partly Schamberg's progressive pigmentary dermatitis. There are angeiomatous vessels and a reticular network of what is, I think, obviously blood pigment.

The case bears some points of resemblance to one shown by Dr. Pringle on May 15, 1919,[1] as a probably early case of the miscalled multiple idiopathic hæmorrhagic sarcoma of Kaposi, but in his case there were raised pad-like, purplish vascular growths, which are not present in my case. I have not yet been able to do a biopsy. Personally I am inclined to consider this a case of Schamberg's disease, and in my opinion the case shown by Dr. Pringle was one of Schamberg's disease also.

[1] *Proceedings*, 1919, xii (Sect. Derm.), p. 48.

Two Cases of Actinomycosis.

By J. H. SEQUEIRA, M.D.

(I) ACTINOMYCOSIS OF FACE.

G. W., AGED 61, a farm foreman, of Kettering, noticed two years ago a discharge from the *left* nostril. Two months later a swelling appeared on the *right* cheek; this increased in size, soon broke down and discharged pus, in which the ray fungus was found. Surgical treatment had been offered and refused. The patient was admitted under my care at the London Hospital on October 30, 1923. The right side of his face is deformed by large rounded swellings outside the right eye, firm to the touch and not tender. The outer half of the right orbit is excavated and thin pus runs from its depths. The right eye is occluded by swollen lids; there is a free discharge of pus from ulcers on the right side of the nose and from sinuses on the right cheek. The pus is full of streptococci.

His teeth are bad on the whole, there is excessive salivation, and the jaw movements are limited. His chest is unaffected; the abdominal viscera show no change.

Blood: Red blood cells, 4,890,000; hæmoglobin, 60 per cent.: leucocytes, 11,200. A differential blood count on October 30, 1923, showed: Polynuclear neutrophils, 80 per cent,; polynuclear eosinophils, 4·5 per cent.; small lymphocytes, 6·5 per cent.; large lymphocytes, 1·5 per cent.: large hyaline cells, 7 per cent.; coarsely granular basophilic cells, 0·5 per cent.; Wassermann, negative.

The patient has improved with treatment by X-rays and 120-grain doses of potassium iodide daily.

(II) ACTINOMYCOSIS OF ABDOMINAL WALL.

J. V., a BOY, age 13, admitted to the London Hospital on September 3, 1923. Three weeks previously, in jumping across a ditch, he had fallen and had hurt his abdominal wall. A swelling developed and gradually increased in size. The patient had had pneumonia three years previously and an abscess of the neck in the same year. On examination a large hard swelling was found in the left lower abdominal quadrant and a smaller swelling in the right lower quadrant. An incision was made into a soft area in the larger swelling; this opened into a large abscess cavity below the aponeurosis of the external crural oblique muscle. A smaller abscess was discovered on making an incision over McBurney's point. The actinomyces were found on September 14, 1923. The sites of incision are represented by indolent red margined sinuses; a swelling can be palpated in the right iliac fossa; there is no abnormality per rectum, and the liver and spleen are not enlarged. There is a nocturnal temperature of about 100° F.

I am able to show this case by the kindness of my surgical colleague, Mr. Driberg.

Severe Lichen Planus of the Mouth.

By A. M. H. GRAY, C.B.E., M.D.

THE patient is a man, aged 53. For five years he has had a chronic stomatitis, which has caused him very considerable discomfort. It appears to be entirely due to lichen planus. I have never seen such an extensive case of the disease in the mouth. The whole tongue is whitish

and swollen, and there are deep depressions along it. There are also large irregular milky patches inside both cheeks, and also inside the lips, with a tendency to slight excoriation. The vermilion border is also similarly attacked. There is a little warty growth on the right side of the tongue. A surgeon to whom I showed it does not think it is more than simple papilloma. The patient has also typical patches of lichen verrucosus on the shins, and papules of lichen planus on the front of each wrist. The Wassermann test has been done twice: once before giving salvarsan, once after, and it was negative both times. Two injections of salvarsan have had no effect on the symptoms.

There is also a curious atrophic condition of the nails. He has completely lost the nails of the first and second toes of each foot, while an atrophic condition exists in the three outer fingers of the left hand, the nail fold being adherent to the atrophic nail plate on these fingers.

Dr. GRAHAM LITTLE (President) said that, in his experience, the occurrence of lichen planus in the mouth without lesions on the skin was rare. Dubreuilh had mentioned in the *Annales de Dermatologie* that affection of the mucous membranes without skin lesions was more common than affection of the skin without mucous membrane lesions. This statement was so contrary to general experience that he (the speaker) had called attention to it, and Dubreuilh had written a private letter in answer saying that after revising his statistics he had concluded that his first statement had been an erroneous one, and had withdrawn it.

Case of (?) Darier's Disease.

By H. MACCORMAC, C.B.E., M.D.

PATIENT, a female, aged 18. She believes that the eruption first appeared about the age of 7 years—possibly earlier—as a distinctly "scurfy" condition of the scalp; it seems to have spread downwards from this situation over the face, neck and trunk.

Present condition: The head is distinctly "scurfy" but is without the characteristic lesions of Darier's disease. On the temples the eruption has become confluent, forming a rough sheet; the naso-labial folds are also considerably involved; the cheeks are only slightly affected. Passing downwards, the eruption appeared on the neck in the form of many discrete lesions, especially in the supra-clavicular regions. The upper chest is relatively free; the eruption becomes prominent again over the abdomen. The arms, legs and back are almost unaffected. Thus it will be seen that as regards its distribution the condition conforms to Darier's disease. Darier has described the characteristic eruptive element as a papule the size of a pin-head, capped with a greyish crust inserted into a depression, generally, but not invariably, corresponding to the pilo-sebaceous orifice. This description of the lesion is applicable to the case exhibited. The diagnosis is confirmed by the microscopic appearances as shown in the slide exhibited. The horny layer is thickened and fills the pilo-sebaceous orifice as a sort of plug; the Malpighian layer is folded in an abnormal fashion, and here and there are certain prickle cells which have undergone the special degenerative change resulting in the "round bodies" of Darier, characteristic of this disease. There are certain features somewhat unusual in this case. The disease is stated to be more common in the male sex; the exhibitor has only met with three examples, all in females, two of them have been shown at the Section. Although treatment is usually of little effect, the patient has clearly improved, as regards the face, by applications of salicylic oil. No other members of the family are similarly affected.

Section of Dermatology.

President—Dr. E. G. Graham Little.

Results of Treatment in Syphilis of the Nervous System.

By John A. Fordyce, M.D.

(*Professor of Dermatology and Syphilology, College of Physicians and Surgeons, Columbia University, New York.*)

Possibly an apology is due to our neurological colleagues for bringing the subject of syphilis of the nervous system for discussion before the Section of Dermatology since this phase of the "great infection" has hitherto been chiefly regarded as a part of neurology.

The symptomatology of the two most important clinical entities of this group, namely tabes and paresis, has been largely observed and developed in the late or terminal stages, when patients present themselves to the neurologist. For this reason the statement is often made by teachers and text-books that they begin years after the primary infection. The study of neurosyphilis at this time is of much academic interest and it gives the observer a certain satisfaction accurately to locate a focal lesion or a tract degeneration. He may use his diagnostic skill but has little opportunity to apply his therapeutic art. It is true that medical literature contains scattered observations showing early infection of the nervous system, but it is only within the past ten years that more careful clinical and laboratory methods have shown the relative frequency with which the spirochætæ enter the nervous system during the time of their dissemination by the blood and lymph streams. It has been shown by many investigators that fully 25 per cent. of all syphilitics present clinical or laboratory evidence of involvement of the nervous system in the first year of the infection, at a time when they are under the care of the dermatologist or specialist in syphilis and are most readily influenced by specific remedies.

The systematic examination of all early cases to detect the presence or absence of neurosyphilis, the determination of its type if present, and the adoption of proper therapeutic measures are of equal importance to the practitioner and to the patient because of the prophylactic value of these procedures in limiting the late degenerations. If a patient can be assured, after adequate treatment and after laboratory tests have shown the nervous system to be free from infection, that he will not develop tabes or paresis, the mental relief is well worth the inconvenience and possible disturbance incident to a lumbar puncture. The old and perhaps the still prevailing view that the nervous system can be infected at any time during the persistence of the disease has little to support it except vague clinical impressions. It could only be determined by spinal fluid examination showing absence of early infection and by similar examination at a late period proving its existence. No such evidence has been produced. On the contrary, many clinical observations are on record where the histories of patients with late syphilis of the nervous system show

the occurrence of ocular palsies, bladder and rectal disturbances, paræsthesias, changes in the deep reflexes and other symptoms or signs of early implication. These symptoms had regressed under treatment but had usually recurred in a more pronounced manner until a well-defined type was established. I have, fortunately, been able to follow the life history of several patients from the early months of their infection, with positive evidence of cord and brain syphilis, until the development, twenty years later, of typical tabes and paresis. One of these patients, who eventually died of paresis, filled several business positions requiring great executive ability. In the last three years of his life he developed marked changes in personality, extravagance in his manner of living, and expansive ideas. His spinal fluid showed the paretic formula. His nervous system had harboured spirochætæ from the early period of his infection until definite stigmata of degeneration appeared. In similar cases, in which, however, modern therapeutic methods have been employed and controlled by spinal-fluid examinations, cures without relapses have been achieved.

We have, in addition to clinical evidence, which is becoming more and more convincing, observations showing the persistence of visceral and cardiovascular lesions for years, with slight tissue reactions, the recurrence of skin and mucous membrane changes at or near the sites of earlier ones, and the recurrence of attacks of keratitis in congenital syphilis.

The experimental work of Nichols and Reasoner, together with the clinical observations of Mott and others as to the existence of familial types of neurosyphilis, supports the theory of a strain of organisms with neurotropic affinities or rapid invasive power. I have reported from my own experience twenty family groups comprising about sixty individuals showing the incidence of neurosyphilis in those closely related.

The number of "positive" spinal fluids in the early months is probably greater than the total number of cases of late neurosyphilis. Some are cured by routine treatment, so that we may say in a general way that the percentage of early and late cases is fairly equal. We are obliged to conclude, therefore, either that early infection of the nervous system is cured and a later invasion takes place, or that the later types are rapidly or slowly evolved from the earlier ones in spite of the therapeutic measures employed.

We have been able, in our private and public work, to control by clinical observation and laboratory tests so-called "cures" in many patients treated in a routine manner during the early infection and are convinced that the large majority whose nervous systems were implicated at that time retain the infection. Symptoms may temporarily disappear, the blood Wassermann reaction may become negative, and the patient for a time feels secure, only to realize later his true condition. The assumption, therefore, that secondary neurosyphilis is cured, and that a nervous system potentially vulnerable to spirochætal invasion persists, is extremely improbable, and has little support either in clinical observation or definite laboratory control.

It is not necessary to emphasize the statement that all early lesions of syphilis readily yield to specific remedies, when contact can be established. The persistence of the infection in the central nervous system, after the disappearance of all visible and accessible lesions, is due to the cessation of treatment adequate to control the systemic infection and to change the complement fixation reaction in the blood. Failure to control the infection in the central nervous system by systematic spinal-fluid tests leads to the persistence there of residuals which later become activated.

Longer use of the salvarsan group intravenously, if the effects are controlled

by fluid examinations from time to time, cures certain types of early and late neurosyphilis. On the other hand we have cases in all stages of the infection which fail to respond to drugs by the ordinary channels but which yield in a rapid or gradual manner to combined intravenous and intraspinal injections or to intraspinal treatment alone.

The data which I shall present show in a striking manner that the early lesions in the nervous system respond more rapidly to properly directed therapeutic attacks than do the later ones. I shall also endeavour to prove that the cure of early neurosyphilis in all probability prevents later degenerations, and furthermore that freedom of the nervous system after adequate treatment has never, in my experience, up to the present, been followed by reinfection.

It is perhaps needless to say that the physician who treats syphilis should have sufficient neurological training to enable him to recognize the signs or symptoms of nervous system involvement, the ability to interpret the findings of the laboratory, and the necessary skill to carry out efficient therapeutic procedures. Every well-appointed clinic for treating these cases should have a certain number of beds available so that lumbar punctures can be made and the patient permitted to rest for at least twenty-four hours afterwards. Students of medicine should be properly instructed as to the bearing of early neurosyphilis on the late degenerations and taught to visualize all the possibilities of the disease. After a time a correct conception of the pathology of syphilis will be more widely diffused among the members of the medical profession and will awaken them to the possibility of preventing its most dreaded sequelæ. The tragedy of neurosyphilis to the individual and his family and the expense and economic loss to the State are too well known to be touched upon here. Any prophylactic measure which may prevent or lessen its incidence should receive serious consideration. The routine administration of so many injections of our modern arsenic preparations, combined with mercury, even when controlled by the Wassermann reaction in the blood, is inadequate for protecting the patient's future. The signs and symptoms of nervous-system involvement in the early months are sometimes absent or are so indefinite as to be easily overlooked. If lumbar puncture is limited to the cases with obtrusive signs, irreparable damage may result.

In an analysis of over one thousand cases of syphilis in which lumbar punctures were made in my clinic by Dr. Rosen and myself, we found that 26 per cent. showed positive findings in the first two years. The fluid findings in one group of cases showed a relatively low cell count, a slight excess of globulin, a positive Wassermann reaction with the maximum amounts of fluid and a luetic gold sol curve. The minor cases were found to be amenable to routine treatment: they showed no progress and only slight neurologic signs. On re-puncture several months later they yielded a negative fluid.

In other groups of cases the following symptoms were most frequently met with: headache, vertigo, insomnia, nervousness, pains in the extremities referred to as rheumatic, phobias, and inability to concentrate. On careful physical examination, involvement of the optic and auditory nerves was occasionally met with, together with pupillary irregularities and alterations in the deep reflexes. The cell count in the latter group ranged from 25 to 300; the globulin reaction was strongly positive and the Wassermann reaction strongly positive in the higher dilutions, 0·4 to 0·1 c.c. The gold sol curve was not infrequently of the paretic type.

We encountered several malignant cases giving the picture of an acute meningitis and sometimes terminating fatally. A cell count as high as 2,000

with all the other phases strongly positive was exceptionally found. Experience has taught us in these malignant cases to begin treatment with relatively small doses of our remedies and to relieve the increased pressure by spinal drainage before employing energetic measures, otherwise a reaction of the Herxheimer type may easily supervene with an intensification of all symptoms.

In the charts which I have prepared, the detailed findings and methods of treatment, together with the results, will be shown in a number of cases re-examined from eighteen months to ten years after their last treatment after all the findings in the blood and fluid became and remained negative.

A full realization exists in the minds of my colleagues and myself as to the fallibility of all laboratory tests and the possibility that the nervous system may continue to harbour spirochætæ with all the tests negative. Certain types of neurosyphilis, including the purely vascular, the gumma unaccompanied by a meningitis, some unusual psychoses, and epilepsy of luetic origin, are often found with little or no change in the fluid. These exceptional conditions in no way alter the general rule that the spinal fluid reflects the pathological processes in the brain and cord, and enables us often to determine its type and to differentiate it from certain non-syphilitic affections which cause a similar clinical picture.

I have emphasized the incidence of early neurosyphilis, its relative amenability to treatment and its probable relation to the late forms. Unfortunately it is too often overlooked at the opportune time and causes gradually or abruptly menacing signs or symptoms which may be correctly interpreted or wrongly diagnosed. The nervous system, like other organs, may be the habitat for years of the spirochætæ with little or no tissue reaction until a traumatism or other exciting factor stimulates their activity or until sufficient time has elapsed to permit centres or tracts to be compromised which give rise to objective signs.

A low-grade parenchymatous encephalitis with slight or no meningeal involvement may exist for years with indefinite symptoms impossible to classify or interpret without the aid of spinal fluid tests.

A slight character-defect, loss of ambition, forgetfulness, irascibility, mental depression, so-called neurasthenic symptoms terminating in a nervous breakdown, often precede for years the final stages of paresis, and their origin is unrecognized until, during medical examination undertaken because the patient's family has become alarmed by a change in his personality, or because his business associates have become disturbed by financial losses caused by gross errors in his judgment, pupillary changes are revealed. Loss of consciousness after unusual mental effort, or without assignable cause, may lead to an investigation revealing a long standing brain syphilis.

It can be conservatively stated that the number of cases of syphilis of the nervous system is much greater than statistics show, as these are based on the classical clinical syndrome and fail to take cognizance of the atypical or minor types.

The symptomatology of cord syphilis and early tabes may be equally vague and long unrecognized. Patients with gastric crises mistaken for appendicitis, gastric ulcer, floating kidney or gall bladder involvement, are not infrequently taken to the operating table. Rectal and bladder crises are mistaken for local conditions. Leg-pains called sciatica or rheumatism may precede for years definite disturbance in gait. An autopsy on a patient who died at the City Hospital, New York, from a ruptured abdominal aneurysm revealed a focal degeneration in the posterior roots of several of the nerves in the lower cord.

A diagnosis of sciatica had been made before a lumbar puncture showed fluid changes.

Apart from such obtrusive symptoms as double vision, severe headache, convulsions, aphasia, paralyses, and typical tabetic pains, weakness of the bladder with lost or impaired sexual power, may first cause the patient to seek advice. Reference to symptoms is merely suggestive and intended to emphasize the value of spinal-fluid examination in a class of patients who have come under my care, because of an antecedent infection. In many of them a diagnosis of syphilis of the nervous system has not been made.

The longer these investigations are continued the more strongly are we impressed with the importance of recognizing the progressive or stationary character of the pathological condition which may be present, and the transitional types between the early meningeal lesion, and the complete text-book descriptions of tabes and general paralysis.

Judging from our experience a comprehensive re-study of the symptomatology in correlation with the findings of the laboratory is necessary before a rational prognosis can be made and a logical method of treatment devised and carried out. It is needless to say that the conclusions reached at one time as to the prognostic value of a definite formula in the fluid have been modified in the light of additional experience. The absence of the paretic gold-sol curve in the great majority of cases definitely excludes paresis. We have had only one case in which the curve changed from luetic to paretic. Personality-changes had been noted in this patient for a year and there seemed little doubt as to the slow development of paresis when he died of an intercurrent typhoid.

Cerebral meningo-vascular syphilis and general paralysis may give identical findings in the fluid and present very similar clinical pictures. A differential diagnosis is often only possible after prolonged treatment. In the former there is coincident clinical improvement and changes in the fluid reactions, while in the latter, the Wassermann reaction remains persistently positive in the higher dilutions and the gold-sol curve is unchanged. The cell count in paresis may be changed to normal by treatment, thus relieving the menacing symptoms due to a meningitis. Remission following intensive use of the arsenicals is often marked and may continue for months or years, leading the patient's family and friends to believe a cure to be probable. A close observation of the fluid formula should always cause a guarded prognosis, as relapses follow the remissions and death eventually occurs. Remissions in cases of general paralysis are due largely to the control of the existing meningitis. The deeper parenchymatous lesions, not being reached by the drugs, continue to progress, eventually leading to the usual degenerative stigmata.

The persistence of a strongly positive Wassermann reaction in the high dilutions, with a paretic gold-sol curve after intensive and prolonged treatment, is in our experience the only practical way of differentiating these closely related clinical conditions. We have not infrequently been obliged to change our original diagnosis of paresis to meningo-vascular syphilis after observing the results of treatment on the symptoms and the fluid reactions. On the other hand an original diagnosis of meningo-vascular syphilis has as frequently been changed to paresis for the reasons given.

We are convinced, after prolonged observation, that histological changes, identical with those met with in paresis in the usual locations, may exist for years in parts of the brain not concerned with the intellectual functions. In one of our cases, to be referred to later, there was an entire absence of mental symptoms, speech disturbance, tremor, or any of the usual signs of paresis.

He was under treatment at intervals for nine years, during which time he carried on his occupation as a stationary engineer and never lost a day except for treatment. He was given a large number of intravenous as well as intraspinal injections with practically no effect on his fluid reactions. A diagnosis of probable or potential paresis was frequently made because of the persistence of his reactions after so much treatment. In March, 1922, he became nervous and apprehensive about his condition; toward the end of the month he developed convulsions, after which he was disoriented, excitable and so unmanageable that he was placed in an institution where he remained until his death on July 10, 1922. During the nine years he was under our treatment a diagnosis of paresis from his symptoms or objective signs could not have been made until the last year of his life. This condition has been aptly designated by the late Dr. Southard of Boston as "paresis sine parese" and illustrates the long latent or asymptomatic period which may exist in brain syphilis.

An academic attempt to define exactly what is meant by paresis or tabes, or when neurosyphilis becomes one or the other of these so-called entities, is not of great importance from a practical view-point. We are endeavouring to treat a condition and not a name, and to base our therapeutics on the activity and accessibility of the spirochætæ. Pessimistic views based on the failure to influence a focus of degeneration following obliteration of a terminal blood-vessel or to restore tract degeneration secondary to neuronal destruction, are often misleading and fail to take cognizance of the problem as a whole. The creation of a pseudo-tabes and a true tabes, according to their theoretical mode of origin, is largely artificial and based on few necropsy findings in the early stage of the process. A primary neuronal degeneration may be caused by direct invasion of the posterior roots. In such cases the meningeal reaction may be slight, and secondary to the neuritis.

The progress under these conditions is less favourable than where the cell count indicates a more definite meningeal response. Advanced posterior-column degeneration is of course irremediable but many associated symptoms such as pain and crises when due to an associated meningitis can be relieved by treatment. Furthermore, tabes progresses because the cause persists; new segments of the cord are involved by fresh invasions of the organisms. A reversal of the positive phases in the fluid by combined intravenous and intraspinal treatment has frequently in our experience been followed by a complete and permanent arrest of further extension of the degeneration and by a gradual subsidence of the accompanying symptoms.

All physicians with a wide experience in syphilis of the nervous system occasionally meet with a patient presenting typical Argyll-Robertson pupils, absent knee-jerks with minor sensory disturbance, and little or no disturbance in gait. In cases of this kind which we have examined, the blood and all the findings in the spinal fluid were negative, and have remained so over a period of years. The possibility of the arrest of tabes by the ordinary routine treatment or by the defensive forces of the body should encourage us in our therapeutic endeavours.

In a clinical and serological analysis of 442 cases of neurosyphilis in my private work during the past ten years, 156 patients with tabes were investigated, of whom 50 were in an early stage with incomplete findings of posterior-column involvement, while 106 were sufficiently advanced to give classical symptoms. In 70 per cent. the Wassermann reaction was positive in the blood, and with one exception always positive in the fluid when larger quantities were used in the test. Twenty-five per cent. had a paretic

gold-sol curve and 75 per cent. a luetic one. The cell count varied from normal to 350.

Of this number of cases thirty have been re-examined and have remained asymptomatic clinically and serologically for from one to nine years. Thirty-seven cases were not re-examined. Sixty-eight cases in addition to those re-examined showed marked clinical improvement. Because of advanced degeneration in the lower cord without evidence of an active meningitis, twenty cases were treated only intravenously, together with mercury intramuscularly and by rubbings.

The larger number of cases of neurosyphilis are referred to me by physicians who have treated them more or less intensively in a routine manner with salvarsan and mercury, without an antecedent fluid examination. It is not, therefore, always possible to determine the extent to which the fluid findings are influenced by intravenous treatment alone. As a rule in these cases all the phases are positive; the cell count is sometimes low and has probably been affected. In patients who have become intolerant to salvarsan intravenously, we are obliged to employ intraspinal treatment with serum obtained from another treated patient. In this way we are able to obtain definite proof of its spirochæticidal action uncomplicated by other factors.

Dr. Swift, in a personal communication, has recently given me the history of a case of typical tabes treated by Dr. Ellis and himself in 1913, with serum intraspinally, and no intravenous injections. He was examined ten years after his last treatment, when his fluid-findings were still negative with no progress of his tabes.

Swift and Ellis, to whom we are indebted for the introduction of salvarsanized serum, proved by their experimental work in 1914 that it has a definite action on the organisms of relapsing fever, and that this action was increased by heating the serum for half an hour at a temperature of 56° C.

The definite response of certain types of neurosyphilis to intraspinal treatment after failure of prolonged use of the intravenous route has been confirmed by long clinical experience and by careful laboratory control. The advocates of the method have endeavoured to recognize its limitations, and to use it when other methods have failed, and when the fluid-formula offers clear indications for its employment.

In optic atrophy the result of a basilar meningitis, the use of methods demonstrated to be futile or of little value is wasted effort and results in the loss of valuable time. Persistent intraspinal treatment when indicated by the fluid findings can arrest its progress and often preserve a useful amount of vision. Our statistics show that thirty-three cases were under observation and treatment, nine of which were of the simple optic atrophy type, seventeen of the tabetic type and seven of the paretic type. Twenty-seven patients had positive Wassermann reaction in the blood and fluid, with cell counts from normal to 180. Eight had almost complete atrophy of both eyes before treatment was undertaken. No amelioration was brought about. Seven had complete atrophy in one eye and partial in the other. Two of these progressed and five became stationary. Seventeen had incomplete atrophy of both eyes. In twelve improvement took place and the process was arrested. In five a further reduction of the visual fields was noted before arrest of the process. The majority of these patients were controlled by well-known eye-men and the fields of vision taken before, during, and after intraspinal treatment. In five of the patients arrest of the atrophy has been confirmed from two to five years after the finding of negative blood and fluid.

The importance of the condition in question, the failure of routine treatment to relieve it and the fear which still exists as to the possible production of neuritis by the arsenical preparations have led me to emphasize the value of intraspinal treatment.

For comparative study we have grouped our 442 cases under the following tentative diagnoses based on the clinical and laboratory findings; secondary neurosyphilis; vascular and meningo-vascular neurosyphilis; pre-tabes or tabes imperfecta; tabes well developed; optic atrophy and general paralysis.

Three hundred and sixty-five of them have been re-examined from one to ten years after treatment was discontinued. In some cases we considered the treatment adequate while in others we failed to secure full co-operation with the patient or for other reasons it was discontinued too soon.

Nineteen cases of secondary neurosyphilis were treated intraspinally because of the persistence of their symptoms and fluid-findings after from one to three courses of salvarsan intravenously and mercury intramuscularly. The Wassermann reaction was positive in all cases either in the blood or fluid and in the large majority in both. The cell count was from normal to 940. Of these nineteen early cases five received more than one course of combined intravenous and intraspinal treatment, eight more than two courses and five more than three courses. One patient sensitized to salvarsan intravenously was treated intraspinally only and received sixty-seven injections before a cure was achieved. This case illustrates the tolerance of some patients to this treatment and the necessity of persisting in it. In all nineteen patients the clinical symptoms such as headache, insomnia, vertigo, aphasia and facial palsy disappeared. Where marked tendon and pupillary changes were present these remained unaltered. In every case the reaction became negative in the blood, and in only two cases remained weakly positive in the fluid, probably because of failure to carry out the treatment systematically. The seventeen patients who were serologically negative have remained so from eighteen months to six years. We have yet to detect a recurrence in an early case discharged with negative findings after adequate treatment, or to observe a late infection of the fluid in one shown to be normal in the first two years of the disease.

Under the diagnosis of vascular and neuro-vascular neurosyphilis we have included a group of 139 cases which came under observation later than two years after the primary infection. The majority had both subjective symptoms and objective signs, but not sufficiently definite to be placed with the tabetic or paretic group. In twenty-five the Wassermann reaction was negative in the blood. The diagnosis might easily have been missed if a complete examination had not been made. Twenty-eight had a strongly positive Wassermann reaction in the fluid with a paretic curve. Eighty-six a similar reaction with the luetic curve. Sixteen had a negative reaction. In the latter group the diagnosis was based on the history and the presence of symptoms pointing to blood-vessel involvement without a concomitant meningitis. The cell count in the entire number analysed ranged from normal to 2,900; the high count in one case only. Intraspinal treatment was only employed when the fluid findings were positive. Forty-five clinical and serological (blood and fluid) cures were obtained, which have persisted for from one to ten years. Sixty-five cases were improved clinically with a reduction in the strength of their reactions.

In the tables it will be noted that the number of cases we considered cured becomes progressively less with the age and type of the infection. In 116 cases of paresis which were treated intensively thirty-nine have died, thirty

have passed from observation, and forty-seven are known to be living. We have no cures to report, although remissions were induced in forty-six patients which lasted from a few months to several years. These remissions occurred in patients with a high cell count when the meningeal picture was in the foreground, and were coincident with a reduction in the number of cells. A number of these patients became amenable to home care after previous institutional confinement. Thirty-three patients received from two to five or more courses of combined treatment, and were under observation from ten to eleven years. Apart from the induction of remissions, which have been confirmed by several of my neurological colleagues, we have little that is hopeful to offer.

I have attempted to emphasize some of the general principles of diagnosis and treatment in certain types of syphilis of the nervous system, and to show that results may be obtained by intraspinal therapy when the usual methods have failed. Ten years' experience with it has enabled me to formulate rather definitely its indications and contra-indications. It was never intended to supplant routine treatment for the general infection, but rather to supplement it in the most inaccessible locality. Further experience has modified some of my earlier optimistic views, but has confirmed my belief in the method as a useful adjuvant, and in certain cases the only hope of relief that can be offered this unfortunate class of patients. Attempts to modify the original technique of Swift and Ellis to increase the efficiency of the serum have met with little success. We were for a time favourably impressed with the method of Dr. Ogilvie, i.e., the addition of from one-tenth to half a milligram of salvarsan to the serum before heating it, but further experience has taught us that even these small quantities added to 25 or 30 c.c. of serum at times causes irritation of the lower cord. It is exceptional for any irritation to follow the injection of properly prepared serum if sufficiently diluted with spinal fluid before it is re-injected.

A preliminary drainage is not necessary and only a quantity of fluid required for the tests is removed. Into the receptacle attached to the spinal puncture needle 30 to 40 c.c. of fluid are permitted to accumulate and to this is added the inactivated serum. The mixture is then allowed slowly to re-enter the subarachnoid space by gravity. In this manner about one-third of the entire quantity of the spinal fluid is mixed with salvarsanized serum before its re-injection. We have found that half an hour after the preliminary intravenous injection is the best time for removing the blood. This is permitted to clot over night in the ice-box. The next morning it is centrifugalized and the serum pipetted into a sterile tube. After a repetition of this procedure, to ensure the removal of all red cells the clear serum is pipetted into another tube and heated for one half hour at a temperature of 56 degrees C.

The number of injections and the intervals between depend chiefly on the duration of the infection. The intervals are never less than two weeks, and are gradually lengthened until six or eight treatments are given; then a rest period of six weeks to two months before a second series of injections. In the early cases one series may be sufficient permanently to reverse all the reactions in the fluid. In old cases many repetitions over several years may be required. Numbness in the buttocks and genital organs, together with difficulty in urinating, are definite warnings to discontinue the treatment or to lengthen the intervals.

The contention that the method in question is a universal "cure all" in neurosyphilis has never been upheld. It is a valuable method in selected cases

in the hands of one trained in its proper use. It is enduring the test of time with clinicians who are trained in interpreting the findings of the laboratory. Criticisms of it are mainly from a theoretical view-point and individual prejudice rather than from deductions based on a long-continued and proper

TABLE I.—SUMMARY OF THE AMOUNT AND RESULTS OF TREATMENT OF PATIENTS WITH THE VARIOUS TYPES OF NEUROSYPHILIS (EXCLUSIVE OF PARESIS).

	Secondary neuro-syphilis	Vascular and meningo-vascular neurosyphilis	Pre-tabes (tabes im-perfecta)	Tabes (well-developed)	Optic atrophy
AMOUNT OF TREATMENT.					
Less than 1 course salvarsan intravenously and Hg intramuscularly	—	2	—	11	—
More than 1 course salvarsan intravenously and Hg intramuscularly	—	15	4	3	—
Less than 2 courses salvarsan intravenously and Hg intramuscularly	—	18	—	1	5
Less than 3 courses salvarsan intravenously and Hg intramuscularly	—	3	—	—	—
Less than 1 course salvarsan intravenously combined with intraspinal	—	15	16	23	6
More than 1 course salvarsan intravenously combined with intraspinal	5	17	7	26	6
Less than 2 courses salvarsan intravenously combined with intraspinal	8	31	14	21	16
3 courses or more salvarsan intravenously combined with intraspinal	5	25	5	9	—
Intraspinal alone	1[1]	1	—	—	—
RESULTS.					
Symptom-free and Wassermann reaction negative in blood and spinal fluid ...	17	45[3]	13	17	7
Marked clinical improvement	all[2]	65	—	44	12
Slight clinical improvement	—	8	4	19	—
Stationary	—	—	24	—	10
Not influenced	—	2	2	11	8[6]
Symptoms increased	—	5	—	4[5]	2
Died	—	5	2[4]	4	—
Disappeared from observation	—	14	5	14	—
DURATION OF CURE (Clinical and Serological).					
1 year	1	16	3	3	1
2 years	5	7	3	2	2
3 ,, ...	7	9	1	3	2
4 ,, ...	3	7	3	4	1
5 ,, ...	—	4	1	1	—
6 ,, ...	1	3	1	—	1
7 ,, ...	—	—	1	1	—
8 ,, ...	—	1	—	2	—
9 ,, ...	—	—	—	1	—
10 ,, ...	—	10	—	—	—

[1] This patient received sixty-seven intraspinal injections before a cure was obtained.

[2] Two patients would not follow systematic treatment. Wassermann reaction still ± in cerebro-spinal fluid.

[3] Eleven patients (among whom only two had four + Wassermann reactions in cerebro-spinal fluid) were treated only with salvarsan intravenously and Hg intramuscularly.

[4] While under treatment one patient developed a stronger Wassermann reaction in cerebro-spinal fluid, and gold curve changed from luetic to paretic type; patient died with typhoid fever

[5] Three developed Charcot joints while taking desultory treatment. One who had improved became much worse two years after discontinuing treatment.

[6] These eight patients had almost complete double optic atrophy before treatment was started.

TABLE II.—SUMMARY OF EFFECT OF TREATMENT ON THE WASSERMANN REACTION IN THE BLOOD AND CEREBRO-SPINAL FLUID OF PATIENTS SUFFERING FROM THE VARIOUS TYPES OF NEUROSYPHILIS.

Type	Secondary neuro-syphilis	Vascular and meningo-vascular	Pre-tabes (tabes imperfecta)	Tabes (well developed)	Optic atrophy	General paresis
WASSERMANN REACTION IN THE BLOOD.						Surviving cases
Number re-examined	19	127	41	81	33	64
	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.
Negative remained negative... ...	16	20	24	26	19	19
Positive became negative	84	43	37	27	21	17
Positive became weaker	—	19	17	9	12	11
Remained strongly positive	0	18	22	38	48	53
WASSERMANN REACTION IN THE CEREBRO-SPINAL FLUID.						
Number re-examined	19	99	40	77	33	64
	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.
Positive became negative	89	43	32	22	21	0
Positive became weaker	11	53	58	53	24	—
Remained strongly positive	0	4	10	25	55	100

TABLE III.—SUMMARY OF THE AMOUNT AND RESULTS OF TREATMENT OF PATIENTS WITH PARESIS.

	Died			Living
		39		78
AMOUNT OF TREATMENT.				
None	—	—	—	13
Less than 1 course intravenously and intraspinally	20	—	—	19
More ,, 1 ,, ,, ,, ...	—	—	—	13
2 to 5 courses ,, ,, ...	—	—	19	33
RESULTS.				
Remissions	—	23	—	46
Slight improvement	—	5	—	—
No ,, ...	—	11	—	19
Institutionalized	—	—	—	20
Amenable to home care	—	—	—	44
Epileptiform seizures during treatment	—	—	—	11
TIME DEATH OCCURRED AFTER BEGINNING OF TREATMENT.				
2—4 months	3	—	—	—
1 year	5	—	—	—
1½ years	5	—	2	—
2 years	3	—	7	—
3 ,, ...	2	—	3	—
4 ,, ...	1	—	6	—
5 ,, ...	1	—	1	—

use of it. It was furthermore discredited by the unfortunate results which followed the suggestion of Ravaut, i.e., the direct addition of neosalvarsan to the spinal fluid. The occurrence of traumatic myelitis and resulting paraplegia after the latter procedure led many to condemn all attempts to influence neurosyphilis by this route.

Conclusions.

(1) Invasion of the central nervous system occurs in the majority of cases during the period of general dissemination of the virus.

(2) It is often checked but seldom cured by the usual therapeutic methods. Relapses are due to residua after subcurative treatment.

(3) Late cases of neurosyphilis including paresis and tabes are not reinfections from foci outside the nervous system, but are due to long persistence of the infecting organisms *in loco*.

(4) If the nervous system is not involved in the early period of the infection it is extremely improbable that it will be in a later period. Vascular lesions and gummata may be excepted.

(5) In seeking for an explanation of a persistent positive Wassermann reaction in the so-called latent period the possibility of a nervous system lesion should be kept in mind.

(6) No patient should be discharged from treatment without the information afforded from a lumbar puncture.

(7) Certain types of syphilis of the nervous system including early meningitis, late meningo-vasculitis, tabes with positive phases in the fluid and with not too advanced degeneration in the lower cord, early optic atrophy with the findings of a meningitis, are more rapidly controlled by combined intravenous and intraspinal treatment and not infrequently only by this method.

(8) In general paralysis remissions of long duration are achieved which are more striking in patients with a pronounced meningeal reaction. Cures cannot be attained by any existing treatment.

(9) As a result of the arsenic investigations we have been pursuing for the past two years we have been able to detect arsenic in the spinal fluid of every individual receiving intravenous treatment. An explanation of the cures after intraspinal injections is not therefore wholly due to the arsenic content of the serum, but possibly to the development of the antibodies *in vivo*.

Final judgment of a therapeutic procedure should be based on results rather than on theoretical reasoning.

Discussion.

Dr. James Collier said that Dr. Fordyce seemed to have had the advantage over London specialists in that he saw cases at so much earlier a stage, and treated them before they showed any neurological symptoms.

He (Dr. Collier) agreed that invasion of the nervous system occurred in very early days of the disease; and it was remarkable how long the infection remained latent. One patient whom he knew had been infected at the age of 18, but developed his first nerve symptoms at 61. On the other hand there were examples in which the most virulent and progressive degeneration of the nervous system of the tabetic form occurred merely some months after the primary infection. Those cases were rare, but very important.

He had not had a large experience of the use of intraspinal injection in the form of salvarsanized serum. When he used the drug regularly he believed that the chief effect came from intravenous injection. Dr. Fordyce had however attained

wonderful results, better than he (Dr. Collier) would have thought possible. That was probably because the treatment was carried out thoroughly, and was completely under the control of serological methods.

Dr. Fordyce had mentioned cure of "optic atrophy;" he (Dr. Collier) felt that the term required explanation. There was the optic atrophy which arose from a gross lesion of one optic nerve, such as that caused by a gumma; that was cured sometimes and arrested often. Did Dr. Fordyce refer to the slow progressive diminution, with restriction of fields on both sides, which came in from the periphery? This form was believed here to be primary neuronic degeneration, and he had never seen a case of the kind cured. All the cases of the kind he had seen went on to complete blindness, except one, and that was a case of congenital syphilis. It would be interesting to hear whether Dr. Fordyce had seen bilateral symmetrical optic atrophy, without a sign of retro-bulbar neuritis, cured.

Professor F. R. FRASER said he also thought that the results obtained in New York were, to a large extent, due to the very thorough treatment carried out there. There were many workers who maintained that the results of lumbar puncture, combined with intravenous injections, were sufficient, without intraspinous salvarsanized serum as well. Comparison with the results in series treated in that way and by intravenous injections alone would be of very great interest. Lacking that, one could not help being greatly struck by the series now shown. The lesson was one which ought to be taken very much to heart, because in many hospitals in London the workers did not quite appreciate the possibilities of treatment that Dr. Fordyce had insisted upon, and how very frequent early involvement of the nervous system was in cases which were not showing, symptomologically, definite signs of the known entities of syphilis. If for no other reason, members must be grateful to Dr. Fordyce for having expounded and insisted on these points.

Dr. ARTHUR ELLIS said that the results recorded in this paper were a satisfactory vindication of the value of prolonged, intensive and intelligent treatment in the care of neurosyphilis. One often heard opinions glibly expressed as to the uselessness of treating syphilis of the central nervous system. The expression of such opinions was usually an accurate measure of the amount of endeavour the individual in question gave to the care of these cases. Unfavourable opinions both as to treatment as a whole and as to the value of intraspinal treatment were too often based on an entirely inadequate use of the remedies. There was much too prevalent a tendency to aim merely at the relief of symptoms and not at eradication of the signs of active disease.

Practically everything that Dr. Fordyce had just told them was in keeping with his (the speaker's) more limited experience. The examination of the spinal fluid by various observers in cases of early syphilis showed a striking uniformity—evidence of infection of the central nervous system in about 30 per cent. of all cases examined. It was true that Ravaut and later Dreyfus had advocated an even higher percentage of infections, but in the speaker's opinion a more critical analysis of their results led to the same conclusion as the figures given by Dr. Fordyce, namely 25 per cent. to 30 per cent. It was an interesting coincidence that this was also the figure given by Fournier for the percentage of cases of syphilis developing manifest lesions of the central nervous system. One could not help being impressed with the probability that this dissemination took place during the period of septicæmia, that is, during the secondary period. He (Dr. Ellis) agreed with Professor Fraser as to the lack of general recognition of this essential fact.

He also thoroughly agreed with Dr. Fordyce as to the benefit of prolonged treatment, both for early involvement of the central nervous system and in its later stages. In his own experience, if treatment was continued until all evidence of infection had disappeared, clinical progress of the case ceased and there seemed to be a complete arrest of the process. His experience with general paresis was small, but like Dr. Fordyce he had never seen more than temporary benefit. Dr. Swift had informed him that a review of the original series of cases treated with intraspinal injections in 1912-1914 showed that every one of the patients who had proved resistant to the treatment had subsequently died of general paresis.

The difficulties in connexion with the treatment of neurosyphilis in England, and especially in connexion with the use of intraspinal injections, were largely related to the question of man-power. It was almost impossible for any individual, either in his private or in his hospital work, to carry out treatment on a large series of cases on the scale presented by Dr. Fordyce. In his private work Dr. Fordyce had the services of at least two full-time assistants, here the consultant worked alone. In hospitals in America there were one or two house physicians for from twenty to thirty beds, in this country only one for from sixty to eighty beds. Under such circumstances adequate intravenous therapy was sufficiently difficult, and the much more time-consuming intraspinal treatment had never received an adequate trial.

Dr. G. RIDDOCH said that Dr. Fordyce's important contribution was perhaps unique in this respect, that it presented the results, in a very large series of cases of neuro-syphilis, of treatment which had been persisted in for years and adequately checked by both clinical and serological tests.

Dr. Fordyce had re-emphasized the importance of examination of the cerebro-spinal fluid not only as an aid to diagnosis but also at intervals throughout the treatment as a help in gauging its efficacy. This was essential in cases where alteration in the spinal fluid had been found ; for the Wassermann reaction in the blood might become negative and the clinical state stationary at a time when the specific tests in the fluid remained positive. It had been said that clinical improvement and persistent abolition of those serological tests, which were looked upon as definite manifestations of active syphilis in the nervous system, meant that the disease was arrested. In general, this was probably true ; but now and again one encountered a case in which all the tests were negative, and yet the patient continued to go downhill. This small but important group of cases required serious investigation.

Dr. J. H. SEQUEIRA said that he would not venture to discuss the intricate details of neurosyphilis, but on behalf of the dermatologists and those in charge of syphilis clinics in London, he said that the clearly-expressed theme of Dr. Fordyce's paper would be carefully studied, and full attention would be given to the statistical tables, which were remarkably full and complete. Dr. Fordyce had focussed on the very important point, insisted on some time ago by Head and Fearnsides, that the brain and spinal cord were affected early in the generalized stage of syphilis, and this showed how very important it was that treatment should be started with the least delay. The lesson of this paper for those who were practising dermatology and those in charge of syphilis clinics, was to concentrate on early and thorough treatment, and to do every-thing possible to induce patients to come for treatment before the disease became generalized. The diminution and the ultimate obliteration of neurosyphilis could only be hoped for in proportion as that great need was recognized and cases came to the doctor at the earliest stage.

Dr. H. MACCORMAC said that, being a dermatologist and not a neurologist, he was not competent to discuss the technical questions involved in the purely neurological aspect of the problems so completely dealt with by Dr. Fordyce. The neurologist might be said to see the failures of the dermatologist, although this generalization, like many others, was only partly true. This, however, formed a common link between the two branches of medicine.

He (Dr. MacCormac) always thought that syphilis could be broadly divided into two groups, the early and generally curable stage, and the late and generally incurable stage ; the neurologist usually saw examples of the latter type. But it should also be remembered in a discussion of this kind that even in the early, the " dermatological " stage of syphilis, there was an involvement of the central nervous system. This did not give rise to signs and symptoms except in rare cases, and could only be detected by examination of the cerebro-spinal fluid. Such examinations had been made by many competent observers, and it might be accepted as a fact that in a considerable pro-portion of early cases such evidence of involvement could be found, although opinion might differ as to the exact percentage—some put it as high as 60 or even 80 per cent. In most cases in which the disease was treated thoroughly in the early stage, on

examination at the termination of such treatment, the cerebro-spinal fluid was found to be normal. This did not necessarily mean that such cases might not later develop central nervous system syphilis, but it brought the changes in the cerebro-spinal fluid into relationship with those in the blood, and showed that even where there were definite modifications from the normal, indicating involvement of the nervous system, cure was possible, because the disease was still in the early stages. In the later stages the signs and symptoms might be considerably modified, and the disease arrested by treatment, but the chance of complete cure was small, for it was usually found that even when prolonged and intensive treatment was undertaken the Wassermann reaction remained positive, or finally became so.

There was another group of cases midway between the early and late varieties where a cure had not been achieved, and where a routine examination of the cerebro-spinal fluid might reveal disease. In these cases, however, prolonged, continuous treatment was justified, because the outlook, although not as good as in very early stages of disease, was much more favourable than in late syphilis. It was interesting to observe that this type of case often appeared in the groups of neurosyphilis treated by the arsenical compounds. He thought a clearer impression of the effects of treatment on disease would be obtained by considering separately these three stages of infection. Those who had had the advantage of hearing Dr. Fordyce's paper would, he (the speaker) thought, be encouraged to take a more favourable view of what might be hoped for from "salvarsan" treatment of specific nervous disease.

Dr. WILFRID FOX said that Professor Fordyce was extraordinarily fortunate in being an expert in two branches of medicine. Such a combination was rare in this country and although most of those present had at one time or another attended clinics at Queen Square, most dermatologists in after-life were dependent on their neurological colleagues.

One thing the lecturer had done was finally to crush the heresy that neurosyphilis was increased in this country by more active treatment. This suggestion was made repeatedly at each stage of the advance of therapeutic methods. When the Aachen treatment came into vogue, when intramuscular injections of mercury were employed, and finally at the commencement of the salvarsan treatment, it was asserted that although the dermatological lesions were removed, more patients suffered from tabes and general paralysis. The recent work of Colonel Harrison in this country, together with that of Professor Fordyce, had shown that this was not true.

Dr. A. M. H. GRAY said that Dr. Fordyce gave a figure of 30 per cent. to 35 per cent. as the average number of secondary cases which bore evidences of neurosyphilis, remarking that he thought that figure was high because he had a large number of special cases sent to his clinic. He (Dr. Gray) would like to know the percentage Dr. Fordyce got in those cases which he had treated in his own clinic from the start; was the figure as high when he had control of them himself in the early stages? Dr. Ellis had suggested that that was the average figure, and pointed out that it corresponded closely with the known figures of tabes and paresis. If that was so, it was a terrible thing to contemplate, because those were the figures attained in pre-salvarsan days, and it would mean the situation was not improved by the use of the newer remedies. He did not himself think the figures for treated cases could be so high; it probably meant that a number of untreated or incompletely treated cases had been included.

Dr. FORDYCE (in reply) said that their New York statistics, in a large number of cases, showed that about 25 per cent. had early nervous system involvement. Ravaut's figures, in his (Dr. Fordyce's) opinion were too high and were based on minor changes such as slight increase in pressure or slight globulin reaction with other phases negative. In his reference to the report of Dr. Rosen and himself he (Dr. Fordyce) stated that the higher percentage was due to the inclusion of the results of the examination of a number of known neurosyphilitics sent to the clinic for investigation.

He (Dr. Fordyce) desired again to emphasize the statement that all early cases with positive findings did not require intraspinal treatment. He employed the method only in cases which were refractory to routine treatment.

Replying to Dr. Collier's inquiry as the early occurrence of tabes Dr. Fordyce said the answer depended largely on our definition of that condition. He had seen ataxia occur within the first two years, but as a rule the complete clinical syndrome did not develop until years later. Infection of the nervous system, which he believed always took place in the early months, was often checked but not cured by the treatment employed. Failure to control the results of treatment by spinal fluid examination was largely responsible for the persistence of the infection. An examination of the fluid before the final discharge of a patient from observation was in his (the speaker's) opinion imperative. With the possible exception of gummata and pure vascular lesions he had seen no cases of late neurosyphilis develop in patients with early normal fluid. He had also seen the type mentioned by Dr. Riddoch, of posterior root degeneration in which symptoms persisted in spite of negative fluid. These were exceptional and did not invalidate the general rule that tabes progressed because the nervous system continued to harbour spirochætæ.

Section of Dermatology.

President—Dr. E. G. Graham Little.

Case of Lichen Nitidus (Pinkus) or Tuberculide Lichéniforme et Nitida (Chatellier).

By H. W. Barber, M.B.

This patient has a typical eruption of lichen nitidus on the flexor surfaces of the forearms and on the legs. She was referred to Dr. Marshall for investigation for active tubercle. He reports: "I can find no abnormal signs in the lungs. She is apyrexial and is putting on weight. There are enlarged glands on the right side of the neck, probably old tuberculous infection."

Histologically the characteristic cellular infiltration in the papillary layer is seen on either side of a follicle. The epidermis is thinned over the papule. No giant-cells are seen.

A Rare Lingual Condition.

By H. W. Barber, M.B.

This is a very rare lingual condition on the dorsum of the tongue. The patient was referred to me by Mr. Fagge, who said he had never seen such a condition before. In the posterior half of the tongue there is a U-shaped area, covered by reddish nodules with a white sclerotic base, and there are a few small nodules on the hard palate, which have developed since I first saw the patient. The condition has been noticed only four months. The Wassermann reaction is negative. When I first saw the case I did not know what its nature was, but a week later I saw the current number of the American *Archives*[1] and found that in it Dr. Fordyce and Dr. Cannon had described and illustrated two cases as "a hitherto undescribed condition of the tongue" (illustrations shown). Both patients had had syphilis, but were apparently cured and the Wassermann reactions were negative, and in one case twelve injections of arsphenamin had no effect on the tongue lesion. In one of the cases there were small wart-like lesions on the hard palate.

Fordyce remarks that an analogous condition was reported by him in February, 1897, under the title of "hypertrophic lichen planus," in which grouped nodules were present on the under surface of the penis and contiguous surface of the scrotum and on the lower extremity, giving rise to a clinical picture not unlike the tongue lesions referred to. The histology was also similar, there being epithelial hyperplasia associated with connective-tissue sclerosis.

[1] *Arch. Derm. and Syph.*, Dec., 1923, vol. viii, p. 749.

DISCUSSION.

Dr. H. SEMON asked whether Dr. Fordyce had suggested any ætiology of the condition.

Dr. BARBER replied that Dr. Fordyce regarded the condition as inflammatory, though the nodules were evidences of a secondary epithelial hyperplasia. Fordyce did not say whether he considered that syphilis had anything to do with it.

Case for Diagnosis (? Parapsoriasis en Gouttes).

By H. W. BARBER, M.B.

THIS patient has an eruption on the arms, over the shoulders, on the hips and legs identical with that seen in cases recently shown by Dr. MacCormac.

History.—The eruption began on the arms and body about six months ago. Dr. MacCormac very kindly saw her with me and agreed that the case was similar to his own cases, and he suggests that the eruption belongs to the group described as parapsoriasis en gouttes, a conclusion with which I agree. Many of the lesions show well the adherent scale which gives the appearance, at first sight, of atrophy. Brocq figures a similar case in his "Précis-Atlas de Pratique Dermatologique." He points out that in some cases somewhat infiltrated papules are mixed with the more characteristic lesions and that histologically the structure is analogous to that of the tuberculides, according to Civatte. When I first saw my patient there were definite papular lesions which, clinically, were suggestive of tuberculides. The complement-fixation test to tubercle is positive, but my experience of this test has not convinced me of its value. (A section was shown.)

DISCUSSION.

Dr. GRAHAM LITTLE (President) said that the case he showed before the Section two meetings ago had been under treatment without any local applications; only streptococcus vaccines had been used. The patient had improved very much indeed. He thought Dr. Barber's case was a little unlike the instances of parapsoriasis en gouttes he had seen; the latter were usually very intractable, and the fact that the present patient was getting well seemed to emphasize the difference.

Dr. A. M. H. GRAY said he thought parapsoriasis en gouttes was very rare. The lesions were chiefly on the trunk and were very resistant to treatment. The striking feature about them was the underlying lichenoid appearance of the lesions. After removal of a scale, the surface was seen to be very flat and shiny. The lesions in Dr. Barber's case were not quite like that.

Dr. GRAHAM LITTLE (President) said he had had a case which could be described exactly in Dr. Gray's words. The patient was a legacy from Sir Malcolm Morris's clinic, and he (the President) had had it under observation ten years, and it proved absolutely intractable.

Case of Rheumatoid Arthritis, Old Endocarditis, Tubercular Peritonitis, and Papulo-Necrotic Tuberculides.

By H. W. BARBER, M.B.

PATIENT, a female, V. S., aged 32.

Previous History.—Her rheumatoid arthritis began at the age of 18, the finger-joints being affected first. Her teeth had been removed eighteen months before her first admission to Guy's Hospital.

She was admitted under Dr. Beddard, March 6, 1923, for pyrexia and signs of endocarditis. At that time there is a note in her report saying that there were scars on the outer sides of the legs, which had been present for about two years, and which had resulted from "small boils containing yellowish fluid." Dr. Beddard saw her on March 9, and diagnosed aortic endocarditis and regurgitation.

Two blood cultures were negative.

A *Streptococcus longus* was recovered from the fæces—type not stated—and from this a vaccine was made. Swabs from the tonsils yielded only *Micrococcus catarrhalis* and *Staphylococcus albus*. She developed bronchitis, and a *Streptococcus longus* was found to predominate in the sputum.

The vaccine injections gave little or no reaction either in the joints or generally.

On April 23 she vomited four times, and developed abdominal pain, which settled in the right iliac fossa, where there was tenderness and rigidity. Appendicitis was diagnosed, but the abdomen being opened the patient was found to be suffering from tubercular peritonitis. Large calcareous glands were found in the mesentery and in the right iliac fossa. The appendix appeared normal and was not removed. The temperature fell to normal on August 24, and she was discharged.

I first saw her early in November on account of an eruption on her legs, hands, forearms and elbows. The appearance of the lesions were those of a tuberculide of the papulo-necrotic type. She was readmitted to hospital under my care on November 14. Dr. Eyre was asked to see the patient with a view to inoculating a guinea-pig with material from one of the active lesions, but unfortunately by the time he saw her they had subsided.

Investigations.—Wassermann reaction negative.

The complement-fixation test for tubercle was done November 25, and was negative. It was repeated November 29, both new and old antigen being used, and in each case the result was completely negative.

The fæces yielded no growth of *Streptococcus longus*, but only one specimen has so far been examined, so that this observation is of no significance.

The tonsils were definitely septic and have been removed. Culture of excised tonsils gave a mixed growth of *Streptococcus longus* and *Micrococcus catarrhalis*.

X-ray report on joints of fingers and wrists: "The metacarpo-phalangeal and interphalangeal joints show a symmetrical arthritis. The several affected joints show extensive erosion of the articular cartilages and underlying bone. There is very little new bone formation and no loss of detail in bony structure. The inferior radio-ulnar joints show similar changes but less advanced. The wrists and carpus show considerable erosion of articular cartilage, but no erosion of bone. The appearances are typical of infective arthritis of rheumatoid type. No changes are seen in any way suggestive of a tubercular infection. Disease appears to be still active."

This case is of great interest in relation to two cases published by W. H. Guy in the *Archives of Dermatology and Syphilology*[1] of this month. One of these cases is apparently an exact counterpart of my case, as there was rheumatoid arthritis of the same type, and the same type of papulo-necrotic eruption on the arms and legs. No evidence of tuberculosis, however, could be discovered.

[1] *Arch. Derm. and Syph.*, Dec., 1923, vol. viii, p. 754.

The patient had, however, general visceroptosis with very marked intestinal stasis, and the fæces contained streptococci both of the viridans and hæmolytic type. Examination of the teeth, tonsils, and nasal sinuses gave only negative findings.

The other case was also negative as regards investigations for tuberculosis, there was no arthritis, but there were marked signs and symptoms of hyperthyroidism. The teeth and sinuses were negative, but the tonsillar crypts contained *Streptococcus viridans* in pure culture: this organism was also constantly recovered from the stools.

Guy injected pure cultures of *Streptococcus viridans* into the skin of these patients and produced indolent inflammatory lesions, leaving fine scars. Inoculation into thirty healthy persons did not produce lesions, but inoculation into two individuals with circulatory stasis produced lesions resembling papulo-necrotic tuberculide. He then injected attenuated colon bacilli. In addition to local lesions resembling tuberculides, there appeared an extensive crop of lesions on the arms and legs that closely resembled the local lesions and papulo-necrotic tuberculide. These lesions persisted for two months and left fine scars. He therefore concludes that "clinical" papulo-necrotic tuberculide in certain cases is the result of dissemination of a variety of feebly pathogenic organisms from different foci of infection, localization and lesion formation determined by circulatory stasis and lowered resistance."

In my case it is possible that the lesions on the arms and legs are true tuberculides, since the patient has had tubercular peritonitis; but the appearances described at the laparotomy were those of old healed tubercle, and the complement-fixation test, on the value of which I am not qualified to express an opinion, is completely negative. It is possible that in my case the papulo-necrotic lesions are also due to endogenous inoculation of the skin with organisms other than the tubercle bacillus.

In view of what is stated in Dr. Guy's article, during the last few days examinations of her intestines by X-rays have been made, and they show marked ileal stasis, and marked stasis in the colon.

DISCUSSION.

Dr. A. WHITFIELD said he had seen a very large number of cases of papulo-necrotic lesions of tubercular nature, and in a large proportion it was almost impossible to find the original source. In the old days he took them into hospital and tried their reaction with the old tuberculin, and he had not had one case of the kind which did not show an enormous constitutional reaction to the old tuberculin, which he still regarded as the most certain diagnostic test, though it was admittedly too risky to employ from the patient's standpoint.

Dr. GRAHAM LITTLE (President) said that he had shown three cases of papulo-necrotic tuberculide which came on in young children immediately after measles, and that the influence of measles in fostering tuberculosis was well known. His view coincided with that of Dr. Whitfield, that in the majority of these cases there was associated tubercle.

With regard to the complement-fixation test, at St. Mary's there was a special exponent of that test, and many tests were done with it. They believed in it so much there that if it proved negative in a case, there was hesitation in diagnosing the case as tubercle.

Hæmatoporphyria Congenita with Hydroa Æstivale.

By A. M. H. Gray, C.B.E., M.D.

This patient, a girl of 15, appears to be one of the very rare cases of hæmatoporphyria congenita, of which Sir Archibald Garrod has collected the records of eighteen cases. This child has had a skin eruption on the face, hands and forearms ever since her fifth year, and she is known to have had port-wine coloured urine since that time.

The lesions are said to come up as blisters, and the blisters never burst spontaneously but slowly dry up, and in three or four weeks they have almost disappeared. During three or four weeks in hospital she has had only one blister, on one side of a finger, and it contained blood-stained fluid. That has now dried up and disappeared. Over the whole affected area there are numerous pitted scars, shallow, and none very large, with the exception of one in the centre of her forehead. All the affected areas, except the backs of the hands, are covered thickly with downy hair, a condition which has been described in other cases and is probably protective. The urine contains enormous quantities of porphyrin, and the fæces are red and contain much of a similar substance.

The blood count shows practically nothing abnormal.

Dr. Price-Jones has examined the blood on two or three occasions, and has made some interesting observations. Garrod states in his book that there is no evidence of marked red corpuscle destruction, but Dr. Price-Jones, who has had a large experience of blood examinations, has concluded, from examining this girl's blood, that she is producing red blood corpuscles in excess, as evidenced by the presence of large numbers of immature (polychromatosic and stipple cells) and nucleated cells: the blood platelets were much increased in number over the normal. His view, therefore, is, that there is blood-cell destruction going on, but the bone-marrow is compensating for it by an enhanced manufacture, so that the red corpuscles are maintained at about their normal number.

I have tested her reaction to the Kromayer lamp, and also to the Finsen arc. She reacts similarly to the controls with both lamps, developing an erythema but no blisters.

From the dermatological point of view, the interest is, that although one has seen many cases of so-called prurigo æstivale, I do not think I have seen a case with lesions like those in this patient, which, apparently, is typical hydroa vacciniforme of Bazin, and I shall be glad to know if anyone among those present has previously seen this type of eruption. In prurigo æstivale there is an itchy papule, but there are none here. In this case the blisters arise from the normal skin. Crocker considered that the two conditions were distinct, but latterly there has been a tendency to group them as one disease. Most of us have seen cases of prurigo æstivale and tested for porphyrin without success.

DISCUSSION.

Dr. A. Whitfield asked whether the fragility of the blood corpuscles was tested.

Dr. H. W. Barber asked whether a tooth had been removed and examined. He said that one point of interest in reading about Sir Archibald Garrod's case was, that Dr. Douglas Heath was asked to see the child, and Dr. Heath said that if he had not known there was hæmatoporphyrin in the urine he would have regarded the case as

one of epidermolysis bullosa. That supported what Dr. Gray had just said, that true hydroa æstivale should not be considered as the same condition as prurigo æstivale. In his own case the urine was not dark in colour, but hæmatoporphyrin could be demonstrated in it.

Dr. H. SEMON said it was very interesting to hear that the reaction to the Kromayer lamp was negative. A similar experiment had been tried in a case of xerodermia pigmentosa, which he had recently transcribed from the *Archiv für Dermatologie und Syphilis.* In this case also there had been no violent reaction to the application of artificially produced ultra-violet rays, and such reaction as there was had been delayed. Further investigation might in the future modify our views on the effects of sunlight in these conditions.

Dr. GRAY (in reply) said that in this case there was no family history of the condition. He should have mentioned that milium-like bodies, such as were seen in epidermolysis bullosa, were present on the back of the hands. He fell into the same trap as Dr. Douglas Heath had done ; he first thought this case was one of epidermolysis bullosa, chiefly because he saw the child in a bad light, and he thought the face was free, and because of the milium bodies. She had not pink teeth, but they were distinctly yellow.

Striæ Atrophicæ.

By W. KNOWSLEY SIBLEY, M.D.

PATIENT, a stoutly built young married man, aged 22, with one child, was sent to me by Dr. Spong.

There was nothing to note either in his family or personal history. He had always been a fine sturdy boy, and had never had anything the matter beyond the usual diseases in childhood. He did not consider that he had recently either gained or lost weight.

Some three years ago, he noticed some pinkish lines appearing on his upper arms, over the deltoid region, and more recently these have appeared in other regions of his body, and for about a month, on the abdomen. The lines or striæ are parallel, symmetrically arranged and all longitudinal, none being transverse. There are some half-a-dozen pale thin bands down the outer part of both arms, over the deltoids, and one or two more recent pinkish ones on the outside of these. Another set of fine bands occurs on the anterior surface of the forearm, from just above the bend of the elbow nearly to the wrist.

Several broader and more pigmented bands occur over the iliac margins and in the groins. Over the abdomen are some half-a-dozen broad and rather pigmented bands typical of those described as lineæ gravidarum or albicantes in women.

Striæ are not present about the head, neck, shoulders or chest, and none below the groins.

I have also, at the hospital, another patient, a middle-aged man with some very marked linear striæ, only they are transverse, across the dorso-lumbar region. In the present case the lines are all parallel and vertical. Cases of such lines in the knee-joints after typhoid fever have been described, and the lineæ are always transverse. It cannot be seen why, in this case, the lines should be vertical and in the other cases horizontal. He is a fat, heavy man, and I wonder whether there is any endocrine element in the case.

Laboratory Report.—The specimen taken for section included both "stria" and normal skin on either side. Although macroscopically quite obvious (and

easily palpable) microscopically it was impossible exactly to determine the position of the "stria." There are some definite general changes in the whole skin. The epidermis is somewhat atrophic and in many places not more than three cells deep. It is also unusually corrugated, each "ruga" consisting of from one to three papillary areas. There is decreased cellularity of the corium generally, and considerable increase of collagen and elastin, especially in the deeper layers. The sweat and sebaceous apparatus appears normal. There is a small area where the more superficial layers of the corium are almost completely devoid of elastin, in sharp contrast to the areas on either side, which show much deposition of elastin. This is the only area which can be differentiated as the possible site of the "stria." The epithelium over this area is raised (in the sections) rather than depressed, but this may have resulted from fixing and hardening processes.

DISCUSSION.

Dr. F. PARKES WEBER said he thought that in these cases two factors were concerned in the development of the "striæ." One was a want of proportion between the cutis and the parts below the cutis; the second factor was a constitutional one of some kind—certainly of unknown nature when the striæ occurred in apparently healthy young adults, as they sometimes did. In the present case greatly excessive growth of the subcutaneous fat below the cutis had occurred, and the abnormal type of this obesity suggested the presence of a constitutional factor, perhaps of endocrine origin.

Dr. H. W. BARBER said that he had a case which he had watched for about two years, that of a girl now aged 16, who was abnormally fat. At first he thought it was the pituitary type of obesity, but sexually she was well developed. She had well marked striæ atrophicæ over the deltoid region and shoulders, and marked thyroid tolerance. She was taking a good deal of thyroid, but it was not making much difference to her pulse-rate or her weight; but the striæ were now less evident.

Dr. GRAHAM LITTLE (President) said he had reported a case of very remarkable striæ atrophicæ in a boy aged about 16, who had had a severe attack of mumps. Probably both the factors mentioned by Dr. Parkes Weber came into play in that case, because the boy had had a severe infection and had rapidly developing obesity, probably resulting from the way in which the case had been treated; he had been kept at home and fed up.

Case of Epithelioma Adenoides Cysticum.

By E. G. GRAHAM LITTLE, M.D. (President).

PATIENT is a man, aged 50, with numerous tumours on the face. I think this is a case which answers to all the assumed criteria of differentiation between epithelioma adenoides cysticum and rodent ulcer. It is of familial type: three of the children and the mother had suffered from the same condition, which began in this patient before his eighteenth year. Several of the tumours have lasted thirty years or more. In some of them there has been some ulceration, exactly resembling what we see in rodent ulcer. The section under the microscope shows the typical structure which Brooke described in epithelioma adenoides cysticum. Personally, I do not now think a distinction can be made between rodent ulceration and Brooke's disease; I believe them to be phases of the same condition. As Dr. MacCormac has pointed out, the one feature which is very distinctive consists of the familial histories

one gets in epithelioma adenoides cysticum and does not get in rodent. I may say that the pathologist at St. Mary's would not accept it as being anything but rodent ulcer, and he has a very extensive experience of tumours generally.

DISCUSSION.

Dr. A. WHITFIELD said he would join issue with the pathologist at St. Mary's. He (the speaker) had not seen many histological sections of epithelioma adenoides cysticum; but if one placed the sixth-inch objective in the centre of the tumour, so as not to be near the normal epithelium, one found that in this case the interepithelial fibres were unmistakably present and well developed. He regarded that as a feature excluding rodent ulcer. He had examined hundreds of rodent ulcers and, except in two rare cases in which sections were given him by Dr. MacCormac in which the rodent ulcer had become mixed up with squamous-celled carcinoma, he had not seen those interepithelial fibres. Many years ago Dr. Dubreuilh, of Bordeaux, wrote a very long paper on rodent ulcer, and pointed out that interepithelial fibres were found in certain cases; but it was obvious from the illustrations that they were little strands joining one cell to another, not like the regular and symmetrical fibrillæ. He could not say whether that was present in every case of epithelioma adenoides cysticum. There was here also at the edge a very poor palisade layer in comparison with rodent ulcer. The two points he had mentioned, taken together, on which he laid stress, excluded rodent ulcer. Anything might necrose, and it was only where the epidermis was most stretched and least nourished that the ulceration occurred; he thought the ulceration was caused mechanically by cutting off the blood supply.

Dr. H. MACCORMAC said he was inclined to agree with the President, and he thought the appearances presented in the section were those of a rodent ulcer. The question of intercellular fibres was an interesting one; at one time he had devoted a good deal of attention to this point. His experience had led him to believe that without the use of special staining methods it was very difficult to form an accurate opinion as to whether true fibrils were present or not. Unless the tissue was first placed in an isotonic solution the process of hardening was apt to produce pseudo-fibrillæ. In the section under the microscope the new growth was seen to be composed of the characteristic small cells of the rodent type. He doubted whether the condition could be differentiated from a rodent ulcer on the histological appearances alone, although the history of the case placed it in a different category.

Case of Lupus Pernio.

By E. G. GRAHAM LITTLE, M.D. (President).

PATIENT is a single woman, aged about 45. The illness began six years ago with an injury to the right thumb, which swelled and suppurated (and is still deformed). Patches of reddened bluish induration followed on the right cheek, the nose, both on the bridge and at the alæ nasi, the chin and neck, and forehead. There has never been any ulceration or suppuration in these. Her complement-fixation test to tubercle, at first reported negative, is now definitely positive. Schaumann, who has studied this type very particularly, at first regarded it as a non-tuberculous affection, but more recently attributes it to a bovine tuberculous infection.

Dr. A. M. H. GRAY asked whether the President thought lupus pernio was generally tuberculous. He believed that Schaumann did not think so at first when he classified lupus pernio and sarcoids, though more recently he had been inclined to attribute it to bovine tuberculosis. He (Dr. Gray) would have thought all the evidence was against lupus pernio and sarcoids being tuberculous.

Favus of Glabrous Skin.

By E. G. GRAHAM LITTLE, M.D. (President).

PATIENT is a young woman who works in her lodging, in her own room, in the East End. There are no aliens in the house and she does not mix with other people in it. The probable causal agents are the mice which overrun her room. A mouse was caught in the room and sent to me. It had a small nodular growth on the leg, but I have not found the fungus in it. I show a plate of a pure culture of *Achorion quinckeanum*, obtained on Sabouraud's media from scrapings of the skin affected, which establishes this case as a mouse-favus. When seen a few days ago the patient had two circinate patches on the elbow and on the upper part of the chest, in which typical scutula were seen and from which fungus was readily demonstrated. The patches have nearly disappeared at present.

Restricted Annular Lichen Planus.

By E. G. GRAHAM LITTLE, M.D. (President).

PATIENT is a man, aged 57. I first saw him in May, 1922, with an extensive weeping eczema of the hands, ears and groins, which cleared up under treatment. In July, 1923, he began to have bluish circinate patches and rings on his face, forehead, thighs and groin, which are clearly lichen planus. There is moderate itching in these areas. The mucosæ are quite free of disease. There is a patch of plane papular lesions, discrete so far and not ringed, on the nape.

Case for Diagnosis.

By H. MACCORMAC, C.B.E., M.D.

THIS patient is an example of a condition that has recently come under my notice. In all, four such cases have been seen. The history given in the case shown to-day indicates that an eruption first appeared at the age of 14 years, but as the original rash is stated to have been pustular and to have died out it would seem more accurate to assume that the present rash is no more than three years old. It has a definite distribution and course, and can be recognized by these features. Three types of element are seen; a flat macule, small, and presenting a slight degree of scaling; a slightly raised, urticated lesion, of a bright red colour; and the most characteristic lesion, a small macule completely covered by a squame, this squame bearing some resemblance to a thin dirty cover slip. This last lesion, if examined carelessly, conveys the impression of an atrophic spot, an impression heightened when the skin is picked up between the fingers when the stiff squame remains flat. This scale can be peeled off, leaving an underlying smooth red surface. There are few, if any, subjective sensations. The eruption occurs on the trunk mainly, slightly on the extremities. The patient exhibited to-day is the fourth seen at the Middlesex Hospital clinic. In the case of the first patient the eruption cleared up completely in three or four months. The next two patients coming

under observation were recently shown at the Section, when a variety of opinions were expressed as to their nature. The fourth case is shown to-day. Histologically no very striking appearances are seen, the general architecture conforming to what might be expected from the clinical appearances, viz., parakeratosis, œdema of the papillary layer, and marked perivascular collections of round cells.

Dr. Barber's case, shown to me by him some few days ago, would seem also to belong to this eruptive group. It might be suggested that all the cases were examples of Brocq's parapsoriasis en gouttes, but against this is the accepted observation that parapsoriasis tends to persist, whereas in the first case seen in the series the lesions have completely disappeared, and in the second and third cases they are becoming less marked. The eruption does not appear to conform to any of the definitely classified dermatoses.

Case of Lupus Erythematosus with Deep Scarring.

By M. G. Hannay, M.D.

Patient, a single girl, aged 24. Lesions of lupus erythematosus first appeared on the face and arms six years ago. The point of interest is the unusually deep scarring, and the thickened fibrous character of the scars, which are partially fixed to the deeper tissues. The girl is one of a family of nine, all healthy. She herself has never had any illness of importance. Examination has not revealed any defect, except that she suffers from Raynaud's syndrome. The Wassermann reaction was negative. Blood pressure 120. Blood film shows no abnormality. When first seen about three years ago, most of the lesions were distinctly erythematous, and slightly scaly. Various treatment tried has little or no effect, but after a somewhat prolonged course of mercury and potassium iodide, by the mouth, the erythematous and scaly character has almost entirely disappeared, leaving whitish depressed scars. Since discontinuance of this treatment there has been a reappearance, though to a less degree, of the redness and scaliness.

Case of Lupus of Face and Palate.

By H. C. Semon, M.D.

This young man, aged 20, was treated first six years ago, for a lesion thought to be a boil on the left cheek. It has never quite healed and the case was referred to me in January, 1923, by Mr. Barrington Ward, at the Royal Northern Hospital. In addition to the external lupoid patch which you see in process of involution, there is extensive involvement of the soft palate and nasopharynx, with secondary epiphora of the left eye, the tears from which are doubtless responsible for the infection on the cheek.

Local treatment, which included scraping, and later painting with acid nitrate of mercury, was unsuccessful. The patch has always broken down again. Since he has been having daily arc lamp baths, however, there has been a distinct improvement, and less tendency to secondary sepsis. The palatal patches, according to a report from the throat department, remain very much *in statu quo*. The patient himself is confident that there is less

epiphora. He eats and sleeps better, and has put on weight, so that I am very much encouraged to prolong the baths, other than which there is no very satisfactory means of dealing with this complicated type of case. He has had about forty baths, of an average duration of three hours each.

I hope to show the patient again on a future occasion.

Dr. W. J. O'DONOVAN said that the absence of pigmentation of the skin in this patient was very noteworthy. Repeated observations had shown that the greater the pigmentation the more certain was the clinical benefit demonstrable. At the London Hospital, under Dr. Sequeira, four hours daily was the usual amount of time given up by patients to arc-light treatment.

Case of Mycosis Fungoides with Serpiginous Eruption for Thirty Years.

By W. J. O'DONOVAN, M.D.

G. O., AGED 61, male, newsagent. Rheumatic fever when aged 21. No other illness. Married, four children alive out of ten. One born dead. Wife alive and well. Ringworm-like eruption on one arm thirty years ago, skin never clear since. Steady increase of eruption noticed since 1918. Itching very variable,



Mycosis fungoides with serpiginous eruption.

sometimes intense in limbs, trunk always free of symptoms and of rash. *Present condition*, a grey-haired, well-nourished man, with receding gums and foul teeth. No visceral abnormality. No ascites. On both arms and thighs is a bright pink eruption of a serpiginous involuted pattern, which he says never shifts its pattern. (See figure, p. 49.) It is raised a little and scales very slightly. It resembles the tattoo patterns found in savages tribes. On the bend of the left elbow is a dome-shaped red ulcerated tumour of the skin of twelve months' standing. Both legs are pink, and on the right is a shallow 1 in. circular ulcer. The Wassermann reaction is negative. He was admitted to the London Hospital under Dr. Sequeira on December 19, 1923.

Postscript.—Later, X-ray treatment has effected very great improvement.

HISTOLOGICAL REPORT ON A PORTION OF SKIN REMOVED FROM THE LEFT ELBOW. BY HUBERT M. TURNBULL, M.D.

S.D., 2631, 1923.—In the greater part of the section the dermis, with the exception of a narrow zone beneath the epidermis, is completely occupied by a massive cellular infiltration, by which the elastic fibres are almost completely destroyed. On one side of this the infiltration forms perivascular patches in the dermis. The epidermis is slightly thickened over the densely infiltrated portion of the dermis.

In the zone beneath the epidermis the dermis is rarefied by œdema, and is considerably infiltrated with mononuclear cells. Amongst these cells plasma-cells are the most numerous; mast cells are relatively few. The capillaries are dilated, but empty. Many capillaries are partially or completely filled with large lymphocytes, and lymphocytoid cells with deeply basophil cytoplasm.

In the patches of infiltration in the dermis to the side of the massive infiltration, plasma cells are even more numerous, there are many small lymphocytes, and mast cells are scanty.

In the massive infiltration plasma cells and mast cells are very rare. The cells are chiefly either spindle fibroblasts with large, pale oval and round nuclei, or round or polygonal cells with smaller, more deeply stained, oval or round nuclei and more basophil cytoplasm. Amongst the fibroblasts with pale nuclei are a few giant forms with two or, occasionally, three or four nuclei. When three or four nuclei are present they are superimposed. The second type of cell includes lymphocytes, young fibroblasts and cells with karyokinetic figures. The lymphocytes, especially small lymphocytes, are relatively scanty. There are, however, several rounded nodules which closely resemble lymphadenoid nodes; in these, closely packed lymphocytes surround a central group of large, pale, round or spindle cells. The vessels frequently contain many neutrophil leucocytes. Round such vessels there are a few neutrophil leucocytes in the infiltration. In one area neutrophil leucocytes are abundant. Only one eosinophil leucocyte was seen in the infiltration. Other capillaries and veins are completely filled with granulation tissue.

The condition is obviously a granuloma. The granulation tissue resembles that of mycosis fungoides, though in our specimens of undoubted mycosis fungoides lymphocytes are relatively more numerous, and eosinophil leucocytes are abundant in almost all the infiltrations.

Section of Dermatology.

President—Dr. E. G. Graham Little.

Œdema Perstans of Eyelids (Five Years).

By A. Winkelried Williams, M.B.

I have brought this case first because the condition exists on both sides of the face, and, secondly, because the cultivation from secretion from the little fissures which form at the corner of the eyelid gives a pure culture of *Staphylococcus aureus*. Streptococci were not found. The trouble began about five years ago. The man is a tram driver. At first there were recurrent attacks, followed by intervals during which the swelling went down. But for the past six months it has persisted in the condition you see now. All the kinds of treatment I tried failed to give a good result. Recently I had an autogenous vaccine made. So far, he has only had two doses of it, but apparently there has been considerable reduction on the left side, slight on the right.

With regard to the nose, there is no pus inside it, but on transillumination both frontal sinuses and both maxillary antra appear quite black. There is also a coloboma of the choroid. Wassermann reaction negative. Blood counts show no eosinophilia.

I shall be glad of any help in regard to treatment.

DISCUSSION.

Dr. A. Eddowes remarked that the conjunctivæ of the eyelids were of a very dark red colour, which suggested chronic infection. It might be advisable to take a swab and see if streptococci could be obtained from the region. In some respects the condition reminded him of *erythematous eczema*. The infection of the eyelids might account for the fact that the swelling was worse when the patient was warm in bed as stated by him, and that, conversely, any toxin produced there might be more slowly manufactured when the parts were colder while he was at work. If the condition was not improved by local application of sulphate of zinc drops, or by the yellow oxide of mercury ointment, he advised careful deep puncturing of the œdematous area to find fresh lymphatic tracts to carry off the œdema.

Dr. G. Pernet said he had seen one or two cases of the kind, but of lesser degree. It was more or less permanent. He considered it was due to blocking of lymphatic channels about the eyelids.

Dr. F. Parkes Weber asked whether any Member had seen lasting good result from making "new lymphatic channels" in similar conditions. He (the speaker) had never satisfied himself that the procedure was worth carrying out. In a case of chronic œdema (so-called "trophœdema") of one lower extremity in a woman, aged 23, shown by Dr. Weber at the Clinical Section of the Royal Society of Medicine in December, 1908,[1] lymphangioplasty, according to Sampson Handley's method, was tried without success.

Dr. A. M. H. Gray said that some years ago he put in some threads in a case similar to the one now being discussed, in the way advocated by Mitchell, of Belfast. The chronic œdema of the face in that patient was the result of old erysipelas which had been contracted during the South African War. The measure had no effect whatever, it did not even give temporary relief. A year later he removed the threads, as they had been worrying the patient.

[1] F. Parkes Weber, *Proc. Roy. Soc. Med.*, 1908-1909, ii (Clin. Sect.), p. 52.

Case of Granuloma Annulare.

By J. M. H. MacLeod, M.D.

PATIENT is a girl, aged 4½. The lesions consist of almost symmetrical rings on both palms. The rings are unusually regular in outline, and with a diameter of about 1½ in.

The interesting point with regard to the case is that the affection began six months ago, and came on after the child had a bad fall and threw out her hands to save herself, sustaining abrasions on both palms. When these healed, the mother noticed the rings. These seemed to have formed, not by peripheral extension from a central lesion, but, according to her statement, were their present size from the first.

It is unfortunate that we have to perpetuate a name which does not describe the histology of this condition, for it is not a granuloma, and sections show simply an infiltration of small connective tissue cells with a few mast cells.

DISCUSSION.

Dr. G. PERNET said he had made sections a long time ago of a case of Dr. Radcliffe-Crocker's and then suggested the name "Celluloma annulare."[1]

Dr. GRAHAM LITTLE (President) said his impression was that the onset of granuloma annulare was commonest in children. The position in the present case was very unusual; it was more commonly seen on the backs of the hands. With regard to the name, Radcliffe-Crocker's contribution was such an important one that he thought his name should be retained.

Dr. A. M. H. GRAY asked whether injury was a usual association. Usually the lesions came out on the backs of the knuckles, and on the buttocks, prominent parts particularly liable to injury.

Case of Lupus Erythematosus of Lower Lip.

By G. B. Dowling, M.D.

I SAW this patient for the first time at the V.D. department, Guy's Hospital. He told me that he thought he was suffering from syphilis and that he had had a sore on his penis two months previously. His Wassermann reaction was negative. I could not see that the condition on his lip could be anything but lupus erythematosus, although there is no evidence of lupus erythematosus on him elsewhere. The condition is entirely confined to the lower lip.

I shall be glad of suggestions in regard to treatment. I am giving him silver salvarsan, and he says it is improving him considerably. Silver salvarsan, I know, often does good in lupus erythematosus.

DISCUSSION.

Dr. H. W. BARBER said he thought this patient had some traces of lupus erythematosus along the helix of both ears, and there seemed to be some atrophy. In the previous year a German author had published eight cases of lupus erythematosus which he had treated with silver salvarsan, with very good results. He (the speaker) had seen cases improve with it, but never one get well.

[1] *Brit. Journ. Derm.*, 1908, xx, p. 329.

Dr. M. G. HANNAY said that he had been treating cases of lupus erythematosus of the fixed type with subcutaneous injections of sulfarsenol, and some had shown remarkable improvement. No case had yet been long enough under treatment for him to be able to say whether a cure would be effected.

Dr. G. PERNET said he had seen only one or two cases of lupus erythematosus of the lip. There was not the ragged superficial ulceration as in Dr. Dowling's case.

Parapsoriasis Guttata.

By G. B. DOWLING, M.D.

I SAW this man at the Ministry of Pensions. Dr. Semon had seen him the year before, and had diagnosed the case as one of parapsoriasis en gouttes. I shall be glad to hear what the ultimate prognosis of this condition is believed to be. I know these cases do not respond to any form of treatment. This patient has had the condition five years, and so far it has remained practically unaltered during the whole period.

Dr. A. M. H. GRAY reminded Members that at the last meeting a case had been shown under this heading,[1] and in his (the speaker's) comment on that case he had suggested that it did not fulfil certain diagnostic conditions. But the present case fulfilled these conditions absolutely: there was a history of chronicity for five years; the distribution was right, and there were the characteristic psoriasiform scaling and the lichenoid appearance of individual lesions.

Case of Lichen Hypertrophicus.

By W. KNOWSLEY SIBLEY, M.D.

THIS patient is a domestic servant, aged 41, who has had an eruption on her hands and feet for a year. At first sight the condition on the backs of the hands was so suggestive of lupus erythematosus that it was thought to be that disease. But on careful examination it was seen that there was considerable hypertrophy of the swollen areas, with typical lichen planus lesions about the ankles, and fairly typical lichen planus plaques on the buccal mucosa. At one time the lesions were very irritable, but recently the irritation has subsided.

I should like to have some opinions as to the treatment of hypertrophic lichen planus. We know that X-rays cause a quick disappearance of ordinary lichen planus papules, but I believe that they always make the hypertrophic variety worse.

Dr. GRAHAM LITTLE (President) said he had shown a similar case to this, but rather more severe. The patient—a publican—had had on all the fingers and the palm, medallion-like lesions of lichen planus hypertrophicus. He (the speaker) believed that freezing was the best means of reducing the lesions.

Papulo-necrotic Tuberculide treated by Arc-lamp Bath.

By H. C. SEMON, M.D.

A YEAR ago this boy, aged 15½, had tuberculous glands of the neck, which involuted satisfactorily under X-ray treatment. At about the same time he developed papulo-necrotic lesions on elbows, knees, back of the hands and

[1] See *Proceedings*, 1923, xvii, p. 40.

shins. These were very active about six weeks ago; and the patient should have appeared at the last meeting of the Section, but did not come. In his case the effect of the arc-lamp has been extraordinarily good, so good that I think it is almost specific. My experiences with injections of arsenic, &c., have not been very happy. This means is so simple, and so greatly improves the general health also, that it probably is the best treatment for this type of case.

Dr. O'DONOVAN said he would record an experience that did not run parallel with Dr. Semon's case: A young Jewess, admitted to hospital under Dr. Sequeira's care for multiple necrotic small tuberculides of the face and limbs, was treated by irradiation of the whole trunk by light. While under treatment her lesions increased in number and in severity. She became pallid, lethargic and pyrexial, and although treated daily she failed to pigment in the way other patients were pigmenting under similar treatment. Radiographic examination of her chest showed the presence of enlarged glands of the hilum of the lungs. It seemed probable that the prognosis of a tuberculide treated by arc-light must depend upon the activity or quiescence of internal foci of tuberculosis.

Intractable Psoriasis.

By H. C. SEMON, M.D.

I HAVE brought this case hoping that I may receive some hints as to treatment.

The disease is of the chronic, infiltrated and scaly variety, localized to the extensor aspects of the forearms and with a pronounced tendency to flare up and fissure when irritated by cold and winds, &c. The psoriasis followed an alleged injury to the palms by a running hawser a year previously and it first appeared on the forearms. Every form of treatment, including arsenic by the mouth and by injection, X-rays, and ointments, containing chrysarobin, tar, mercury, resorcin and phenol have failed to give any relief, and a recently prepared Danysz's vaccine was equally unsuccessful.

DISCUSSION.

Dr. A. M. H. GRAY said he could not be sure whether this was psoriasis or lichen planus. Some of these cases were extraordinarily difficult to diagnose. Still, it did not seem to be like psoriasis, and the intractability suggested the possibility of it being lichen planus. He asked whether the man had had arsenic in large doses.

Dr. O'DONOVAN said he agreed with Dr. Semon's diagnosis. The case was interesting and important from its ætiological aspect. The patient definitely attributed his skin eruption to an injury to his hands. This injury had consisted of friction which had produced blistering, and the patient had complained of thickness and stiffness of the skin of his palms ever since, but the eruption on the rest of the body had come on twelve months after the original injury. The text-books all agreed that in a psoriatic surface an outcrop might follow in the site of an injury to the skin; but he (the speaker) had been greatly impressed by two cases, unique in his experience, that had occurred at Dr. Sequeira's clinic at the London Hospital in one week recently.

The first case was that of a young man who for some years had worked daily in an aniline factory. His arms became stained with colours, and he cleaned them every night with a solution of lime provided for this purpose by the firm. Quite suddenly his arms became irritated and he appeared at the clinic with an eruption of one week's duration on both his forearms, which were encircled by confluent shades of silvery scale over a purplish background. Above this eruption—which was situated over an area typically affected by the ordinary eczematous reaction to observational chemical

irritants—there were on both his arms a few discrete psoriatic lesions up to 2 cm. in diameter.

The second case was that of a barmaid who had been attending the clinic for some months for lupus of the right eyebrow. Careful examination had excluded the possibility of pre-existing psoriasis of scalp or body, and there was no history of any skin affection in her family. She asked to be seen out of her turn owing to a new eruption. She had fallen a week previously and grazed her left elbow, which was now the site of a scaly psoriatic lesion 3 cm. in diameter; there were small lesions on both her legs, though the other elbow and both knees were free.

He (Dr. O'Donovan) felt greatly indebted to Dr. Semon for showing a case that brought the traumatic ætiology of psoriasis under discussion.

Dr. S. E. DORE said that it was a well known fact that psoriasis followed slight injuries to the skin,[1] the injury might be the starting point of the psoriasis—and this was probably the explanation of the cases following vaccination—or lesions might occur as the result of a scratch by a pin or the finger nail, in patients already suffering from the disease. A few applications of X-rays would generally clear up a patch of psoriasis and he suggested this as a diagnostic method in Dr. Semon's case.

Dr. DYSON said he thought all were agreed that local injury determined the locality of a psoriatic patch, but there was no reason to suppose that injury was a determinant or cause of psoriasis.

Dr. GRAHAM LITTLE (President) agreed that lichen planus was liable to appear on the site of an injury, almost as frequently as psoriasis, around scratch marks for example, and at the waist where friction occurred. Therefore he did not think this consideration could be regarded as a distinction between the two diseases. He supported Dr. Semon's diagnosis in this case. Psoriasis was so often an inherited disease that he (the speaker) asked whether Dr. Semon had obtained a history of previous psoriasis in this patient's family. Frequently a history of delayed psoriasis was obtained.

Dr. F. PARKES WEBER asked whether the fact of lichen planus lesions appearing in scratch-marks was not a point against the suggestion that injury might produce lichen planus or psoriasis in previously unaffected individuals. For probably all persons in whom scratches produced lichen planus were lichen subjects already.

Dr. SEMON (in reply) said that other people had given the patient almost enough arsenic to poison him. The psoriasis here had appeared after the alleged injury. The palmar thickening he regarded as the result of ichthyosis. X-rays had been applied at a hospital in Bath, but there was so much local flaring up that it was abandoned.

Case of (?) Actinomycosis.

By E. G. GRAHAM LITTLE, M.D., (President).

THE patient is a woman, aged about 45, who has had for six months the condition for which she is being shown. There is a circumscribed patch on the left cheek, consisting of granulomatous infiltration of a dusky red colour with very ill-defined borders, painless, and at the present time showing no tendency to ulceration. My own opinion of it is that it is an actinomycosis, but I have not had any opportunity of making any further investigation as yet, as the patient is not under my care.

[1] *See* Ziemssen's "Handbook of Diseases of the Skin," 1885, p. 246, for plate showing psoriasis affecting tattoo-marks.

Case of Erythema Circinatum.

By E. G. Graham Little, M.D., (President).

I show this case as probably of the same nature as one which I brought before the Section in 1912, reported on page 119 of the *British Journal of Dermatology* for that year. This patient is aged 40, and for four years has had a repetition of the patch which we see now present. Her history is that a red spot would come first and spread fairly rapidly to form a ring, the ring enclosing a patch of discoloured skin, and with a bluish-red interior. The ring would enlarge, always leaving a discoloured interior. It is that interior which was characteristic of the first case I had, and is characteristic of this also. The rings may even reach a size of 5 or 6 in., but always with the bluish-red discoloration. This patient has had several of these patches behaving in the same way. The patch now shown began to appear a week before Christmas, and it has reached its present size, 3 in. in diameter, in four weeks. The edge sometimes approaches a condition of vesication, sometimes it is œdematous and urticarial looking. It might possibly be the condition which Darier described three or four years ago under the name "annulaire centrifuge," but I do not think it is that condition because in those cases which Darier described the progress was more rapid—a matter of days rather than weeks—the ring was unchanged and there was no discoloration. I have had one case of that type, and one which was possibly of that type. I have still a third case which I thought was one of the same condition, but which proved to be a case of dermatitis herpetiformis.

The present patient has had exactly the same kind of lesion repeated at intervals every two years, and if this had been a case of dermatitis herpetiformis we should probably have seen some symptom of it before now.

I believe that the case which I brought forward in 1912 was the first of a series of the same type, one being shown by Dr. Gray almost immediately afterwards. Their nature has remained undecided. I think erythema circinatum is the best title for them.

Dr. A. M. H. Gray said that his own case was much more acute, but it was closely related to this type. The patient was a woman, 37 years of age, who was shown at the Section[1] and at the Seventeenth International Congress of Medicine. The lesions had been persisting for some time, and they used to come out in crops, symmetrically on the lower limbs, the forearms and the hands, and the distribution was much the same as in fairly acute erythema multiforme. The lesions always started as almost ivory-white papules. They increased in size, and in a short time hæmorrhage occurred in the centre. Then the centre flattened, and the lesions continued to spread as a raised white ring until they were of considerable size. Blisters appeared on some of them. No lesions were present on the mucous membranes or on the face. At the same Congress a case of very similar nature had been shown by Dr. Bunch, who had labelled it granuloma annulare; but he (Dr. Gray) did not agree with that opinion; he thought it was a case of the same type as his own, but less acute, without blistering or hæmorrhage.

[1] *Proc. Roy. Soc. Med.*, 1913, vi (Derm. Sect.), p. 133.

Case of Ulcerative Stomatitis ; ? Nature.

By A. M. H. Gray, C.B.E., M.D.

Patient, a single woman, aged 46. I saw her once last October, and I have seen her again to-day, and I have great difficulty in deciding as to the nature of the condition. The lesions have been going on for two and a half years and she has been under treatment by Dr. Nabarro for some time. She has recurrent ulcers in the mouth, chiefly on the tongue and inner side of the lips ; sometimes they are on the cheek, sometimes on the back of the pharynx. Apparently she has never been quite free from them since they first began, but individual lesions heal up, leaving scars. Last week she was worse than she has been previously. Dr. Nabarro has been investigating the pathological condition of the mouth, but I do not think he has found an organism which he regards as causal. However, he obtained a streptococcus from one of the lesions, and he has given two courses of vaccines, made from streptococci found. She improved considerably during the first series of injections but the second has had little or no effect. He has done a Wassermann test and the reaction was negative, and he has given her some injections of salvarsan, but without clinical effect. She reacted definitely to tuberculin, but tuberculin injections made the condition worse. She now has a deep ulcer on the inner aspect of the upper lip on the left side, and a deep linear ulcer on the right side of the tongue : there is a yellowish slough at the base of each ulcer and the edges are ragged and overhanging. The only thing I could suggest was that this might be a case of severe herpes iris of the mouth, with much secondary ulceration partly due to friction of the teeth ; the ulcers are most marked where there is pressure.

The patient has had no other lesions, except an ulcer on the vulva which involves chiefly the posterior end of the right labium majus and is of the same character as those in the mouth. There is a scar of an old lesion in the same position on the left labium majus.

DISCUSSION.

Dr. Knowsley Sibley said that in 1899 he had described these cases under the name of stomatitis neurotica chronica. About the same date some cases were published by Jacobi, of New York. All the patients had been women, and they were all of neurotic type. According to her friends the present patient worried over little things. It was noticed that when she had an extra worry, a fresh lesion developed in the mouth. One of the cases he (the speaker) had described was that of a well-known society woman, who for twenty or more years of her life was a martyr to the condition, and frequently had to remain in her bedroom for weeks together because of the enormous ulcers. Another patient was a neurotic woman who, for ten years, had recurrent attacks, but not so severe as in the first case. Dentists knew these cases very well, and sometimes removed the teeth, but that procedure had no effect on the disease. The only beneficial measure was to treat the general nervous system, and to paint locally with tincture of iodine. Apparently, however, nothing stopped the outbreaks. The lesions were very painful, often excruciatingly so.

Dr. F. Parkes Weber said he had seen a similar case to those described by Dr. Sibley (*British Medical Journal*, 1899, i, p. 900, and *Proceedings of Royal Society of Medicine*, Section of Dermatology, 1914, vii, p. 61). He thought the condition was acknowledged to be remarkably intractable to treatment.

Dr. MACLEOD said that it was possible that the condition was an exaggerated example of so-called geographical tongue, with recurrent ulcerations about the mouth. The patient had said that she suffered from indigestion and that it was aggravated by certain articles of food.

Dr. S. E. DORE said that these might possibly be cases of artefact. The remarks of the previous speakers rather pointed to that possibility although it was difficult to see how the condition could be self-produced. It might be worth while investigating them from that point of view.

Dr. W. KNOWSLEY SIBLEY said that these were genuine cases; there was no suggestion of an artefacta element in them.

Case of Cheilitis Glandularis Serpiginosa.

By GEORGE PERNET, M.D.

THE patient is a man, aged 81, attending the West London Hospital. Duration of disease, three to four years. The condition began about the left angle of the mouth and gradually spread towards the right side for about two-thirds of the lips. It was on the strength of the mode of development that I put him on iodide of potassium and added the qualification "serpiginosa." But the Bordet-Wassermann reaction is negative and there are no signs of syphilis. The buccal mucous membrane on the left side is also affected and this has improved somewhat from treatment by a 15 per cent. lactic acid solution. The superficial whitish-grey, slightly raised infiltration of the red portion of the lips and the plugged orifices of glands have improved to some extent. But this may have been due to the removal of four septic lower front teeth.

Section of Dermatology.

President—Dr. E. G. Graham Little.

Case of Macular Leprosy.

By E. G. Graham Little, M.D. (President).

THIS, I have little or no doubt, is a case of macular leprosy; it has features of considerable interest. The patient is a Swedish lady, aged 58, who has been resident in England since 1896 when she left Calcutta after ten years' residence there. The first lesions to make their appearance, according to the history, were noticed in 1917 in the form of "blisters," which she attributed to injury, on her left elbow. In this position at the present time there is a considerable area of atrophic skin. The eruption takes the form of slightly scaly, dusky, pink patches; in some places, as for example, on the fore-arms, the advancing edge of the patch is of a bluish colour, swollen and slightly indurated. Sensation seems to be exaggerated rather than diminished in these areas: stroking with the point of a pin is more keenly felt than on the normal skin. There has never been any neuritis, and subjective symptoms have been remarkably few. Both ulnar nerves are thickened at the elbow but are not painful upon pressure. There is nowhere any whitening (vitiligo). The distribution of the patches is as follows: there is a large quadrangular patch in the middle of the back between the scapulæ, about 6 in. × 5 in. This appears to be the oldest patch and is the least infiltrated. There is a long patch occupying the upper arms and the fore-arms on both sides from above the elbow to the wrist. At the wrist the margin is swollen and blue. Patches are found under both breasts, on the lower abdomen and on the shoulder. There are no lesions below the waist. She says that her face flushes and swells greatly from time to time, so that her eyes are embedded in the swelling. When it subsides there is always some persistent swelling left, but the features are not in any way leonine. Her health otherwise seems remarkably good. Attempts have been made to find bacilli in the discharge from the nose after provocative doses of potassium iodide, but without positive result. An extraordinary feature in this case is the long interval of time, twenty-eight years, which has elapsed since she left India, the country where she probably contracted the disease. This is not, however, an impossible incubation period. Crocker reported a case, which was seen by Hallopeau, in which there had been an interval of thirty-one years between the onset of the disease and the date when the patient had left Martinique after a residence there of fifteen months.

DISCUSSION.

Dr. J. TITMAS said that the patient came to him because of the condition of her face, which, she said, had been present only six months. There was no family history of the disease.

Dr. J. M. H. MACLEOD said he agreed with the diagnosis. He considered that the length of the incubation period was no guide in the diagnosis of leprosy, as it was uncertain. He did not think that the areas of disease in this patient had been long present, as they were in the early hyperæsthetic condition. He showed a picture of a patient—also a woman, and of about the same age—who had similar patches on the arms, and in whom typical anæsthetic patches had developed subsequently. He thought that it would be advisable to put the patient on chaulmoogra oil, or one of its derivatives. He did not consider that a cure for leprosy had yet been established—the most that could be said of leprosy was that in some cases it was eventually a self-curative disease—and that none of the remedies put forward could yet be regarded as specifics in the same way as salvarsan was regarded in relation to syphilis. The chaulmoogra oil seemed to him to act more in a nutritional way than as a specific. He had under his care recently about ten cases of leprosy, and these had been treated by injections of ethyl esters of unsaturated fatty vegetable acids, injections of gynocardate of soda and injections of sodium morrhuate, &c., all of which were remedies which had been strongly advocated. So far the results from them had been disappointing, in view of the statements which had been made in their favour both in the medical and the lay press.

Dr. G. PERNET said he also agreed with the diagnosis. The incubation period of leprosy was a long one; he had seen it manifested twenty years and more after the patient had left an endemic leprous area. He regarded macular anæsthetic cases, from the prognostic standpoint, as on a different footing from the nodular cases. Some of the former he considered were improved by intramuscular injections of soluble mercury. Recently, a man who presented an extensive nodular condition of the face was so much benefited after eighteen months' treatment with chaulmoogra oil that he could not be identified as the same person. He did not consider that the prognosis in the present case was bad if the patient persevered with treatment, kept up her general health, and did not return to Bengal.

Dr. H. SEMON alluded to a similar case which he had seen in Vienna, in which the diagnosis was confirmed by recovering the bacillus from the nasal mucous membrane by means of a platinum loop, and he suggested that this might be tried in the case of the present patient.

Dr. A. M. H. GRAY said that at the present time he had four cases of leprosy under treatment; they were of different types, and confirmed what Dr. Pernet had just said.

One case was that of a boy, whom the speaker had had under observation for four years; his was a typical nodular case. He responded distinctly to chaulmoogra oil, especially when given with 3 per cent. gynocardate of soda intravenously, but his lesions had also materially diminished under intramuscular injection of E.C.C.O. (Muir).

Another of the patients was a mixed case; and he had nodules and definite anæsthetic patches as well. Intravenous injections of gynocardate of soda with chaulmoogra oil by the mouth had caused all the nodules to disappear, and although his macular patches had not altogether disappeared, they had distinctly become less. He was put on intramuscular injection of sodium gynocardate, but for some reason he did not keep up his treatment very well, and his lesions recurred. During the past several months he had again been on intravenous injections, and now practically all his lesions had gone.

Another case was purely macular, and this patient had been on intramuscular injections of E.C.C.O. for about four months, and, so far, there had been but little change in the lesions.

The fourth case was a very advanced one; the patient, a woman of over 60, was bedridden and covered with nodules; she had corneal lepromata and irido-cyclitis and

was now blind. She had had treatment in various places for many years. He had her in hospital for some time, and gave her intravenous injections of sodium gynocardate, but with scarcely any benefit resulting.

Dr. A. WHITFIELD said that in no case of the disease he had seen was there complete absence of all paræsthesia, and he wished to know from those with a greater experience of the disease whether it was within their knowledge that very extensive erythematous patches could appear in a case without any paræsthesia at all. He had looked for that as a very early and characteristic sign, and the present patient denied she had had any burning or tingling sensation. That absence led him to hesitate in agreeing with the diagnosis.

Case of Darier's Disease.

By S. E. DORE, M.D.

PATIENT, a male, aged 67, a labourer, has suffered from an eruption for six months. He states that he had a similar eruption lasting a month, thirty years ago, after returning from India. There is a characteristic follicular keratosis in the suprascapular region, abdomen, flanks and lumbo-sacral region. He also has a lipoma of twelve years' duration above the left iliac crest. The skin shows the typical microscopical appearances of Darier's disease.

Case of ? Darier's Disease.

By S. E. DORE, M.D.

THE patient, a male, aged 54, is not present, but he was shown at the October meeting of the Section as a case of lichen planus on account of the superficial characters of the eruption, its rapid involution and the presence of what appeared to be lichen planus papules, in some parts. The eruption closely assimilates to that of Darier's disease, and this was the diagnosis made by several of the Members of the Section. A microscopical section is shown which, in my opinion, confirms the diagnosis of lichen planus.

Dr. A. WHITFIELD said the section was compatible with the diagnosis of lichen planus, and showed no evidence of Darier's disease. He regarded it as mild hypertrophic lichen planus.

Case of Mycosis Fungoides.

By S. E. DORE, M.D.

PATIENT, a man, aged 80, has suffered from a copious eruption on his trunk and limbs for one year. The eruption was preceded by a "carbuncle" on the back of the right shoulder, now represented by a large scar. The eruption is typical of the pre-mycotic stage of mycosis fungoides, and is accompanied by enlarged glands.

I suggest that the carbuncle was in reality a mycotic tumour preceding the eruption, as I have seen several cases in which tumour formation appeared to antedate or synchronize with the rash on the skin. The patient had syphilis sixty years ago, but this is not thought to have any bearing on the case.

DISCUSSION.

Dr. G. PERNET said that the white areas were normal skin, and emphasized the fact that the mycosis fungoides areas assumed a concave appearance at borders. He suggested the man should be given quinine, preferably in an effervescing mixture, as he considered it had done good in some cases he had had under his care.

Dr. WHITFIELD remarked on the age of the patient, and said that these cases usually occurred in families remarkable for their longevity. This patient's mother died at the age of 96.

Dr. G. H. LANCASHIRE said the patient was clear that the area of the affection on the right thigh was all in one sheet, and that the apparently normal areas of skin had developed from the abnormal. He (the speaker) thought the outline of the patches was rather suggestive of there having been a clearing-up, as the man said.

Dr. W. KNOWSLEY SIBLEY said that two years ago he showed a case in a woman, still alive, who for four years had been a case of "femme rouge"; she was red from head to foot. A year or two later she developed white "geographical areas" on skin which previously had been inflamed. She still had the areas of infiltration with the white patches of pale skin scattered over the whole body.

Dr. E. G. GRAHAM LITTLE (President) asked whether any Members had seen patients in the "homme rouge" stage of mycosis fungoides get well. He did not think that experience would support such a proposition. He had shown at the Section on several occasions a man who showed a general dusky red induration, and Members had regarded it as possibly a case of mycosis fungoides, but this man had got well, and the diagnosis of mycosis fungoides was thereby almost contra-indicated.

Case of Carcinoma Cutis.

By M. G. HANNAY, M.D.

PATIENT is a female, aged 75. There is a history that she had a scar under the left breast subsequent to an abscess forty years ago. Three years ago small lumps began to form in the neighbourhood of that scar, and have been extending ever since. Apparently her health is being maintained fairly well; her weight is only 2 lb. less than it was stated to be five years ago. There is practically no pain, but there is some itching. A large area of the skin of the chest is infiltrated. There are enlarged glands in the axillæ.

I should like advice as to treatment, and would ask whether this type of infiltrating carcinoma is very common.

DISCUSSION.

Dr. G. PERNET said this case came into the category of cancer *en cuirasse* first described by Velpeau.

Dr. A. WHITFIELD agreed with Dr. Pernet's remarks, but said that his experience was that if the cancer appeared at a great age it was slow-growing and senile. In this patient it was of recent occurrence, and so far was not doing much damage, and active interference with it might do more harm than good. He did not think radium was of any use for such cases. He would only advise protection from sepsis; the patient might die of something quite different.

Dr. E. G. GRAHAM LITTLE (President) referred to a case of his own, in a woman whom he first saw when she was aged 45, with a very definite and obvious carcinoma of the breast. She was seen by three surgeons, who recommended immediate operation.

She refused any treatment at all, and passed into the hands of a cancer quack, who gave her some solutions, which had been analysed, and pronounced by Dr. Wynter Blyth to be inert preparations of aconite. The patient remained free from any extension for twenty years; during that time the breast shrivelled. He saw the woman twenty years afterwards, and she then had commencing carcinoma, and she lived with that condition three years. At this stage the late Sir Alfred Pearce Gould recommended radium, but he (Dr. Little) did not think it worth while trying it, and he believed the patient was happier under the policy of non-interference.

Case for Diagnosis (? Urticaria Pigmentosa).

By M. G. HANNAY, M.D.

THIS is a case of so-called urticaria pigmentosa of adults. If the diagnosis depends upon the histology, and the presence of an excess of mast cells is considered a necessary criterion, then some of these cases must be ruled out of the group of urticaria pigmentosa, and another name will have to be found for them.

This man is thirty years of age. The eruption began four years ago on the forearms, and quickly spread to the arms, trunk, and lower limbs. There are two types of lesion: small pigmented macules and papules which have no tendency to urticarial reaction, and slightly red, non-pigmented papules, some of which become distinctly turgescent on friction. There are no subjective symptoms.

Dr. MacLeod has kindly examined a section, and is of opinion there are a few more mast cells than are usually present in an inflammatory exudate, but there is certainly not a marked excess.

DISCUSSION.

Dr. WHITFIELD expressed his gratitude for the opportunity of seeing this case, as there seemed to be some doubt as to whether these cases were, ætiologically, more or less identical with the urticaria pigmentosa of children. Histologically this was unlike the latter. One could say there was some cystic dilatation of sweat glands and an over-richness in cells of the whole corium; but these were mostly fibroblasts, with perhaps an unusually large proportion of mast cells. In the urticaria pigmentosa of children the mast cells were in dense aggregations, forming an infiltration resembling a tumour. It was for Dr. Hannay to devise an appropriate name for the condition; one was required.

Dr. E. G. GRAHAM LITTLE (President) said he thought cases of urticaria pigmentosa varied in the amount of mast cells presented. He had had cases in adults which had shown more mast cells than some cases in children. He agreed that the diagnosis rested on the histology; he would be reluctant to make the diagnosis unless there were mast cells. Some time ago a suggestion was made that a sub-committee should inquire into these cases as the nomenclature was inconclusive, and it would be interesting if Dr. Hannay would collect adult cases which showed mast-cell infiltration and cases which did not.

Dr. J. M. H. MACLEOD said he agreed with the diagnosis. With regard to the microscopical section, he said that he examined the section without any idea of the suggested diagnosis, and came to the conclusion that it was possibly urticaria pigmentosa, as there was a definite increase of mast cells, though they were not in the same profusion as in certain phases of the disease in children.

Case of a Rare Lingual Condition.

By G. H. Dowling, M.D.

This patient came to me with a suggested diagnosis of leukoplakia. I thought that it was not ordinary leukoplakia and was struck with the close resemblance the appearance of the tongue bore to the cases described recently by Fordyce and published in the American *Archives of Dermatology*, of which an example was shown by Dr. Barber at a meeting of this Section two months ago.[1] There are a number of bosses on the posterior part of the dorsum of the tongue, growing up on either side of a deep furrow. The furrow is not produced by contraction of fibrous tissue, and by drawing the bosses aside it can be seen that the furrow contains normal papillæ at the bottom. The remainder of the tongue is smooth and, except for a few small areas, completely stripped of papillæ. There are no milk patches. I have recently seen the case that Dr. Barber showed here, and in this case the anterior portion of the tongue is becoming stripped in a similar way. There are a large number of small papillomata to be seen on the palate. These were also present in Fordyce's cases. I had one of these papillomata removed and the section shows that is a simple papilloma.

Postscript.—I have recently come across the following article, in which a number of similar cases are described. No reference however is made to papillomata. Brocq et Pautrier, "Glossite Losangique Médiane de la Face Dorsale de la Lanque," *Ann. de Derm. et de Syph.*, 5me Serie, viii, 1914-15.

Case of Xanthoma.

By G. H. Dowling, M.D.

I saw this patient, a female, for the first time two days ago. Her history is that seven years ago she began to develop bilateral tumours of the tendo Achillis. At about the same time a group of xanthoma tuberosum nodules developed in the skin of the left elbow. She was operated upon by Mr. Robert Ollerenshaw, of Salford, four years ago; he removed the tendon tumours and the xanthoma nodules of the left elbow. It occurred to me that these tumours might also be xanthoma, and I wrote to Mr. Ollerenshaw asking him for information on this point. He kindly referred me to his published account of the case in the *British Journal of Surgery*, 1923.[2] I have looked up the account, which states that:—

"The tendon was greatly thickened and had many yellow coloured areas on the surface and also infiltrating its fibres. Certain of these patches were also present in the subcutaneous tissue."

Since the date of that operation a fresh group of xanthoma nodules has appeared on the right elbow, and the tendo Achillis on both sides has again begun to thicken. Section of the tendon tumours at the time of the operation showed a considerable number of giant cells among the tendon fibres, grouped round areas apparently containing lipoid. The account states that unfortunately this tissue was thrown away before special staining for lipoid could be done. It appears certain, however, that these tumours were of the same nature as the xanthoma nodules in the skin, and the condition is due to a generalized cholesterinæmia.

[1] *Proceedings*, 1924, xvii, p. 39.
[2] *Brit. Journ. Surg.*, 1923, x, p. 466.

DISCUSSION.

Dr. F. PARKES WEBER alluded to the occurrence of xanthomyelomata ("myeloid tumours of tendon-sheaths") and xanthomyelosarcomata in the absence of any cutaneous xanthoma. He likewise referred to the relationship of arterial atheroma with cutaneous xanthoma. At the old Dermatological Society of London, in 1902, Mr. Willmott H. Evans showed a well-developed man, aged 41, with a nodular patch of xanthoma at each elbow (*British Journal of Dermatology*, 1902, xiv, p. 465). These xanthoma patches were successfully removed by Roentgen-ray treatment. But, about seven years later, the patient commenced to suffer from "intermittent claudication of the lower extremities." The symptoms were at first slight, but gradually increased in severity, so that in January, 1911, when Dr. Weber with others saw him, he could not walk much more than 200 yards without having to rest temporarily on account of a sensation of pain or severe fatigue in the calf-muscles of one or both legs. The pain or discomfort rapidly passed off on resting, and he could then proceed another short distance, and so on. No pulsation could be felt in any of the arteries of either foot, and this classical symptom-complex was obviously due to deficient arterial blood-supply to both legs. The intermittent claudication continued till the patient's death (from cancer of the large intestine) in 1913. The abdominal aorta and iliac arteries (No. 1432b amongst the pathological specimens in the Museum of St. Bartholomew's Hospital, London) showed that the cause of the arterial obstruction was "nodular sclerosis with atheromatous ulceration." Syphilis could be excluded as a cause. Arterial atheroma was histologically a kind of "nodular xanthoma of the arteries," and to some extent pathologically and ætiologically analogous to cutaneous xanthoma. Dr. Weber suggested that patients with nodular xanthoma of the skin—if not being specially treated for diabetes mellitus—should be dieted so as to diminish excess of cholesterin in the blood (limitation of fatty meat, eggs, &c.). In that way, perhaps, a xanthomatous eruption on the skin, serving as a danger-signal, might enable the doctor to ward off or delay the onset of grave atheroma of the arteries and cardiac valves.

Dr. J. M. H. MACLEOD directed attention to an article on xanthoma tuberosum multiplex in the American *Archives* for February. There was in it a careful record of the post-mortem findings.

An Unusual Case of Unilateral Sclerodactylia and Lupus Erythematosus, with Raynaud Phenomena, in a Syphilitic Woman.

By GEORGE PERNET, M.D.

THE patient is a housewife, aged 58. According to her, the trouble started six years ago with "chilblains" about the fingers of the right hand, accompanied by some symptoms of local asphyxia, during the winter time, but the condition went on all through last summer and became permanent. There is a marked difference between the appearances of the two hands, the left one being normal. The right one shows atrophy and tightening of the skin, and also atrophy about the terminal phalanges, especially of the little and ring fingers. A skiagraph demonstrates rarefaction of bone. There is lupus erythematosus about the flexor surfaces of the right fingers. She complains of tenderness and pain in the right hand. There are also similar changes to a less degree about the lower end of right forearm. Eight years ago she noticed changes in the skin of the left thigh and on examination I found a serpiginous infiltrating syphilide leaving behind atrophic scarring of the skin over which it had travelled. There is marked glossitis. On iodide of potassium administration the syphilide has involuted and the condition of the tongue has improved. I am indebted to Dr. Grainger Stewart for the following notes from the neurological

point of view: "No sign of syringomyelia. Condition of hand peripheral. Left ankle-jerk absent." I am keeping the patient under observation; she is very thin and in poor health, though she has improved in a general way as a result of the pot. iod. combined with tartrate of iron treatment.

Last year I showed[1] a woman with bilateral symmetrical sclerodactylia of the same type, with Raynaud phenomena; and disseminated lupus erythematosus: upper limbs, back, scalp, palms. That patient presented irregularity of the pupils, and Argyll-Robertson. Her Bordet-Wassermann reaction was found to be ++ (cerebro-spinal fluid): no lymphocytosis; Wassermann negative. She was quite deaf, but Mr. Banks-Davis, who kindly saw her for me, did not consider the deafness was of a syphilitic nature.

The question arises as to the syphilitic origin of the Raynaud phenomena and sclerodactylia in these two cases.

Case of Acneform Syphilide: Type determined by Camphorated Oil Inunctions to the Skin.

By W. J. O'Donovan, M.D.

A. B., a single female, aged 24, gives a history of exposure to infection with a sailor now at sea.

Six weeks ago she had an acute attack of bronchitis, for which she was well rubbed with camphorated oil. Three days after this rubbing there was an outcrop of spots on the back and front of the chest. When seen first, a fortnight before this meeting, her back and front of chest were freely peppered with small comedones in irregular groups, interspersed with a few red papules. On the bend of the elbows there were red discrete macules. During the last fortnight she has developed a copious coppery maculo-papular eruption, falling hair, adenitis, pallor, condylomata and copious minute blackheads on the forehead. The Wassermann reaction is positive. Treatment has been suspended owing to the interesting point worth demonstrating, that the type of syphilitic eruption has been determined by inunction of oil known to produce grouped comedones. The patient is positive as to her previous complete freedom from blackheads or pityriasis of the scalp.

Psoriasis with a Chronic Ulcer of the Lower Lip.

By H. C. Semon, M.D.

The patient, a man aged 72, also has severe psoriasis of many years' standing, but I do not suggest that there is any connexion between the two diseases. The ulcer, which is situated in the middle line on the mucous surface, and does not involve the skin, is the size of a sixpence. It has been present for two years, and for the eighteen months during which I have observed it, has not altered its appearance in the least. The base is soft, the edges quite flat, and there is very little discomfort and practically no discharge. The microscopic section is exhibited here. The pathologist at my hospital reported it to be a septic papilloma, but as the clinical aspect and the history and results of simple treatment do not accord with this view, I am submitting the case, which of course is highly suspicious of malignancy. Dr. MacCormac, who has seen

[1] *Brit. Journ. Derm.*, xxxv, p. 335; *Proc. Roy. Soc. Med.*, 1923.

the section, believes it to be an early epithelioma. There are no palpable glands, and I should like to know if Members think it would be justifiable, considering the patient's age and the clinical chronicity of the case, to try the effect of a single massive dose of X-rays before referring the case to the surgeons.

DISCUSSION.

Dr. J. M. H. MACLEOD said that inspection of the specimen led to the idea that it was commencing epithelioma. Surgical excision seemed preferable to X-rays.

Dr. PERNET agreed that the growth should be excised, and as soon as possible. Application of X-rays might make it worse.

Case for Diagnosis.

By H. C. SEMON, M.D.

THIS man, aged 48, has a peculiar condition of the tongue, and he has had it now for five years. At first, my diagnosis was leucoplakia syphilitica. The Wassermann reaction was positive in 1921, but has since become negative under treatment with mercury and salvarsan substitutes. All the teeth have been removed, but in spite of all that has be done, the condition has not been improved in the slightest. There are symmetrical patches of sodden desquamating epithelium, without the loss of a single papilla, so far as can be seen. There is complete absence of infiltration, and the patches are confined to the dorsum with no tendency to wander on to the lateral aspects of the organ. No complaint is made of loss of taste and very little of soreness. The patient suffers severely from dyspepsia, and the subjective symptoms on the tongue seem to vary with the gastric disability. In my opinion, we are here dealing with a dyspeptic and functional condition, which may well be a mirror of the condition in the gastro-intestinal tract.

Section of Dermatology.

President—Dr. E. G. Graham Little.

Lantern and Microscopic Slides of a Case presenting Multiple Pre-cancerous Dermatoses.

By L. Savatard.

Through the courtesy of my colleague, Dr. Lancashire, I was able to see and investigate this case which was under his care at the Manchester and Salford Skin Hospital.

The patient was a woman, aged 51. She was in a very weak condition on admission, and, shortly after some of the skin lesions had been excised, she died, apparently from malignant disease, her liver being infiltrated with secondary deposits. No post-mortem examination was allowed. The skin lesions were varied. On the right side of the neck there was a fairly pronounced rodent ulcer or basal-celled carcinoma, or, as the President would call it, "benign erythematoid epithelioma." On the backs of the hands there were numerous discrete lesions of senile keratosis, but the face was not similarly affected. On both legs below the knees there were three or four reddish superficial circumscribed scaly small lesions, not raised above the skin level. The scales were easily removed. On the inner aspect of the right thigh a bright red tumour had become superimposed on one of these lesions, raised above the skin; this was firm though not indurated; it looked like a pyogenic granuloma, but there were no palpable inguinal glands. I suggested that it was probably a carcinoma.

The tumour was of two years' duration and the lesion on which it had become superimposed was one of four years' duration. I show you photographs and slides. You will see that the clinical diagnosis of the lesions of the neck and hands are confirmed histologically. Sections of the small scaly lesions of the legs show simply acanthosis with œdema of the cells of the rete mucosum but no dyskeratosis and no cellular infiltration of the corium. Sections of the tumour present the picture of a carcinoma of the sebaciform type arising from the surface epithelium. I have not had time to prepare lantern slides of this case, but I show you now slides of a similar case in aman, aged 55, who presented such a tumour of three years' duration on the left cheek. I have not had the opportunity of staining for fat, but Masson, in his recent book on tumours, states that these large vacuolated cells contain fat.

This variety of epithelioma is quite distinct, both clinically and histologically, from the sebaceous carcinoma arising from the basal layer of the sebaceous glands, which Darier regards as a basal-celled carcinoma of sebaceous origin.

DISCUSSION.

Dr. MACLEOD asked Dr. Savatard to discuss the relation between this condition and cystic rodent ulcer.

Dr. SAVATARD (in reply) said he thought that these cells were in process of becoming sebaceous glands; they secreted fat. This lesion was not ulcerated. He could not say that all cases of sebaceous carcinoma from the surface epithelium would necessarily present the same clinical picture. In the two cases which he had seen the lesions had been bright red, and had not suggested those of cystic basal-celled carcinoma, which were softer and not so vivid.

Two Cases illustrating the Treatment of Severe Early Syphilitic Recurrences by a Superadded Infection (Malaria).

By J. E. R. McDONAGH, F.R.C.S.

CASE 1.

PATIENT, a male, aged 20, had three follicular chancres on the skin of the penis, January, 1922. Treatment was begun before any other clinical manifestations had appeared, and the patient had a full course of arseno-benzene and mercury injections. While the treatment was still in progress mucous papules appeared in the throat in August, 1922. Another course of arseno-benzene was prescribed. In October, 1922, the mucous papules re-appeared. The patient then received twelve intravenous injections of arseno-benzene in doses of 0·15 grm. made every other day. These were followed by intramuscular injections of mercury. In August, 1923, a nasal gumma appeared, which eventually destroyed the septum and the greater part of the left ala. Several injections of various preparations of bismuth failed to stop the spread of the ulceration, 0·1 grm. doses of arseno-benzene were without effect, and the same was the case with mercurial inunctions undertaken by a trained rubber. In spite of these various measures the destruction of the nose was becoming more complete and I became afraid the whole nose would vanish and that a septic infection would find its way into the air sinuses and bones in the neighbourhood. On January 3, 1924, the patient was inoculated with malaria with the result as shown by the temperature chart. The rigors began on the thirteenth day and were allowed to continue for three weeks, when they were ended by quinine. On February 6 a course of arseno-benzene injections in 0·1 grm. doses, administered every other day, was prescribed. In all, the patient had fourteen injections. The treatment was stopped on March 4; the nasal lesion has now completely healed and the patient feels better than he has done at any time since the disease was contracted two years ago. Formerly, severe syphilitic recurrences which failed to respond to the remedies ordinarily in use were sometimes benefited by Zittmann's decoction. During recent years I have used this decoction while keeping the patient confined to bed and in a heated room and the lesions have vanished, only to re-appear some months later. In the

majority of cases, putting the patient on small and frequently repeated injections of arseno-benzene is sufficient to clear up the lesions. In other cases mercurial inunctions appear to be more satisfactory. In my hands, bismuth has proved unsatisfactory. My reason for inducing malaria was based upon the fact that a super-infection of a milder disease upon a more severe infection does sometimes cause the latter to disappear. This is evidenced by the healing of lupus vulgaris at the hands of erysipelas, and by the spontaneous cure of syphilis should the primary sore become secondarily infected with Vincent's organism at a sufficiently early date. When the position is reversed the milder infection is aggravated. In this case the nose began to heal before the arseno-benzene injections were begun. I have treated three cases in this way to date with what seem to be at present gratifying results. These early recurrences have become more frequent since the introduction of arseno-benzene and to my mind they prove definitely that the action of treatment is not a direct one upon the micro-organisms but indirect, through the medium of the protective substance of the host. In private practice I have known such lesions clear up through treatment being stopped and the patient being sent away for a complete change of food, air and surroundings. A superadded infection leads to an increase in the manufacture of protective substance on the part of the host, while treatment does no more than to stimulate what is in being and sometimes to destroy even that.

CASE II.

Patient, a man aged 32, presented himself with a papulo-erosive chancre on the corona of the penis, of one week's duration, June, 1923. Spirochætes were found, but the Wassermann reaction was negative. Patient had a full course of arseno-benzene and mercury injections. In September, 1923, a gumma of the tongue appeared. The gumma ulcerated, other small gummata appeared and the lymphatic glands in the neck became swollen and painful. The patient was much troubled with salivation, trigeminal neuralgia, and was unable to swallow anything solid. His condition grew gradually worse in spite of 0·1 grm. doses of arseno-benzene, various preparations of bismuth, and mercurial inunctions. On January 3, 1924, treatment by inoculation with malaria was begun. Three weeks later a marked improvement had taken place and under fourteen 0·1 grm. doses of arseno-benzene the lesions disappeared.

DISCUSSION.

Dr. W. J. O'DONOVAN expressed his satisfaction at having seen this case, as he had twice seen patients of this ulcerative type who were under salvarsan therapy die of hæmorrhages; and in such severe cases he would not hesitate to inoculate with malaria.

Dr. H. MACCORMAC asked what was the state of the Wassermann reaction during the treatment of the patients.

Dr. GEORGE PERNET asked whether iodine of potassium had been used in the cases just recorded.

Dr. PARKES WEBER asked whether any non-infective shocks had been tried.

Mr. MCDONAGH replied that the Wassermann reaction had not been done. Both patients had been treated in every conceivable way without avail. Potassium iodide and thyroid extract had been tried without success in these two cases as well as in others of a milder nature.

Case of Multiple Granuloma (Celluloma) Annulare in a Child under Two Years.

By George Pernet, M.D.

Patient, a female child, aged 1 year and 10 months when she first came under observation at end of November, 1923, at the West London Hospital. According to the mother's account, the first lesion appeared on the right leg one year previously, and the others much more recently, namely, two on the left leg, one on the left foot, and one on the flexor aspect of the left wrist, making five in all. I ordered a 1 in 4 ung.-hydrarg. to be rubbed in. When seen two weeks ago, all the lesions had cleared up, with exception of the one on the left wrist, which was fading. I am inclined to put this down to the treatment, though I know lesions sometimes disappear spontaneously.

Case for Diagnosis.

By B. Whitchurch Howell, F.R.C.S. (introduced by F. Parkes Weber, M.D.).

J. S., aged 4 years and 10 months, second daughter of first cousins. Sent to me by two doctors on account of instability of gait and atrophic patches involving muscles, skin and subcutaneous tissues.

History: No previous illness. Began to walk at the age of fourteen months. Spectacles were prescribed two years and seven months ago and have been worn ever since. Patient was fat and well-developed until two and a half years ago; since then she has lost her appetite and become thin. Changes in skin were first noticed a year ago on the outer side of the left leg and next on the arm. There has been a tendency to eruption on skin of chest after certain foods.

On examination: Patient is thin and pale; hair almost white. Heart and lungs apparently normal but there is undue bulging of ribs on each side; spleen and liver normal in size; abdomen normal.

Extremities. (1) Right forearm: A distinctly-defined patch of white, fibrous-looking skin extending from middle of dorsum of wrist, upwards and inwards to just below middle of forearm over posterior border of ulna. This skin is apparently adherent to the underlying muscles and tendons, but is freely movable over the bones; its average breadth is half an inch. (2) Left forearm: A sclerotic patch extending down from internal condyle over inner border of forearm; it is freely movable over underlying structures. (3) Left lower limb: A well-defined area, corresponding exactly to Scarpa's triangle, in which the skin is very thin, almost transparent and freely movable, and the veins are prominent; two patches of skin resemble that on right forearm, being adherent to the subcutaneous tissue and of bluish-white sclerotic appearance; one just below knee-joint, over inner anterior surface of tibia; another on outer side of fibula, just above the middle—this is $1\frac{1}{2}$ in. long and half an inch in breadth. (4) Right lower limb: A similar patch, about 2 in. long and half an inch in breadth extending over inner border of tibia. (5) Feet: Distinct cleft in soles between first and second digits.

On the back there is a circular atrophic patch 1 in. in diameter, exactly over the head of the left femur, which can be felt subcutaneously; a similar patch is present on the right side over the ischial tuberosity; another over the lower left rib and lumbar area has been excised (February 9, 1924).

Nervous system. Eyes: Pupils dilated, equal, react to light; constant lateral nystagmus (ophthalmoscopic examination, negative). Knee-jerks equal. Plantar reflex, flexor; abdominal reflexes present. Sensation (epicritic and protopathic) and motor power unaffected even over sclerotic areas.

Gait: spastic, with tendency to "scissor-legs" and to fall to either side. Difficulty in climbing stairs. No Rombergism.

Wassermann reaction. (1) January 30, 1924: negative, very anticomplementary. (2) February 6, 1924: negative.

Electrical reaction. January 27, 1924: All muscles of limbs, including those passing through sclerotic areas, respond to faradism.

Since admission patient has been kept on ordinary diet and there has been no vomiting or refusal of food.

February 9, 1924. Area of skin and subcutaneous tissue (4 in. by 1 in.) over left lower ribs excised. Wound sutured with interrupted silk ligatures; two long sutures added to relieve tension.

Opinions are asked as to diagnosis and method of treatment.

Dr. F. PARKES WEBER said he regarded this as a case of patchy atrophy of subcutaneous fat, allied to circumscribed sclerodermia or morphœa. The actual skin seemed to be scarcely affected at all, but some of the patches of fat-atrophy were zoniform in distribution. In connexion with patchy atrophy of subcutaneous fat he would refer to a curious case published by Gilchrist and Ketron in the *Johns Hopkins Hospital Bulletin*, in 1916 (vol. xxvii, p. 291). The case was that of a girl, aged 8 years, with atrophy of subcutaneous fat in the lower extremities, irregular in distribution at first, but gradually involving nearly the whole of those limbs. The child's legs gradually almost lost their irregularity in shape, but looked very thin owing to the generalized loss of subcutaneous fat.

Case of Ringed Eruption (Colcott Fox).

By E. G. GRAHAM LITTLE, M.D. (President).

PATIENT, a male, aged 46, whom I first saw in March, 1923, when he had a number of ringed erythematous patches, somewhat resembling ringworm, upon his arms, hip and thighs. There was no fungus in the lesions, and the parts enclosed by the ring presented a fine scaling. He gave a history of twelve months' duration. He came to me again just a year later with a recurrence of the patches, now chiefly distributed upon the thighs. There is very moderate itching, but the patches are extremely chronic. I have seen two other cases extremely like this one.

(1) D. A., aged 60, began to have the eruption about fifteen years ago in the form of circinate patches upon the chest, similar to those now seen. There is no itching at any time and no change in the character of the lesion. Several of the patches on the trunk were treated with X-rays in Australia, where he lives. When seen in March, 1924, he had a number of fairly symmetrical circinate patches, sharply circumscribed for the most part, but in some less definitely so, distributed chiefly upon the thighs, the anterior aspect, the legs, posterior surface, and the buttocks and sacral area. The surface of the patch

was very faintly scaly, in some cases hardly so at all. The limiting line of the patch was slightly more distinct than the rest of the area, but not much more so. The size varied from that of a shilling to a diameter of some 7 to 8 in. The colour of the affected skin was a faint pink. Several of the patches had lasted unchanged for years. They gave him no discomfort, and no further treatment than the X-ray treatment has been used for some of the lesions.

(2) Mr. O., aged 49, began to have ringed patches exactly similar to those described above on the thighs and legs, accompanied by great irritation and with a history of their having persisted for about two months.

Case of Folliculitis Decalvans et Atrophians.

By E. G. Graham Little, M.D. (President).

Patient, a lady, aged 55, with follicular lesions of nine months' duration on the forearm, resembling lichen spinulosus, except that they are somewhat redder; and there are two accompanying patches composed of follicular lesions on the hairy scalp. On one of these there is no alopecia. On a portion of the second patch, in close juxtaposition with the follicular lesions, there is an area of cicatricial alopecia about the size of a shilling. The inference that the alopecia is really the result of follicular keratosis is rendered probable by the presence of the follicular lesions near it. The appearances presented by the cicatricial alopecia are exactly like those in Quinquaud's disease, except that the latter often shows suppuration round the follicles, which is not the case here.

The title under which I am describing this case is a title I used when I showed in 1915 the first case of its kind to be described in this literature (*British Journal of Dermatology*, 1915, p. 183). I am afraid the title was not a very happy one, but as it has been adopted in subsequent literature it may perhaps remain. The peculiarities of that case are reproduced in this lady, the two characteristic features being an eruption resembling lichen spinulosus on the smooth skin, accompanied by cicatricial atrophy, recalling folliculitis decalvans of the scalp. That case was the first of this kind we had met with, and it was followed by another similar case shown by Dore (*British Journal of Dermatology*, 1915, p. 295). Wallace Beatty described a third case of the same condition (*British Journal of Dermatology*, 1915, p. 331), which, I think on insufficient evidence, he regarded as possibly a case of Darier's disease. In my case the essential identity of the lesions on the smooth skin resembling lichen spinulosus and the atrophic patches in the scalp was very clear, and in the discussion upon this case Adamson agreed with me that an atrophic folliculitis explained both symptoms, the "lichen spinulosus" and cicatricial atrophy. I had a second case, even more remarkable, of lichen spinulosus and cicatricial atrophy of the scalp and some lesions of lichen planus. The presence of what was probably lichen planus in this, the fourth case to be described of this syndrome, renders possible the conclusion that all symptoms may be a form of lichen planus. This identification of this syndrome with lichen planus, however, has not been supported by any other case, unless this present case may be regarded as furthering this identification. The only other references to this syndrome which I have met with, I have found in Schaumann's report of a male patient, aged 72, which he regarded as being of the same nature: "Ce cas doit

être rattaché aux folliculites décalvantes et atrophiantes, décrites pour la première fois en 1915 par Graham Little et ensuite par Dore, Wallace (Beatty) et John (Speares) et constatées également, en 1920, par Straszberg de Vienne." (Förhandlingar vid Nordisk Dermatologisk Förenings, Femte Möte, Stockholm, 6-8 Juni, 1922, p. 121.) I have not been able to trace his reference to Straszberg. It is remarkable that all the cases, with the exception of Schaumann's, have been in elderly women.

DISCUSSION.

Dr. A. M. H. GRAY asked whether the exhibitor had gone into the question of tuberculosis in these cases. Not long ago he (the speaker), showed a child who had acne scrofulosorum, associated with a similar condition of scalp. There were some points about the President's present case in favour of its being the lichen scrofulosorum occasionally seen in adults. There was some doubt as to what was the origin of lichen spinulosus in children, and the question sometimes arose as to whether it was tuberculous in origin. The present patient stated that at 7 years of age she had had what was called "brain fever," and that at one time there was some glandular enlargement. The ætiology of atrophic alopecia was still much in doubt, and the possibility of a tuberculous agency was worth consideration.

Dr. A. EDDOWES said he thought the question whether the lesions would be those of lichen *planus* or lichen *spinulosus* was largely one of anatomical distribution. On the hairy parts there was a greater tendency for the lesion to be spinous, and on the sweat gland areas it was more likely to be planus. It was not uncommon to see on hairy shins scars from lichen like those in the scalp of this case.

Dr. H. C. SEMON referred to the case of a woman (aged 40), seen that afternoon at the hospital, who had had on the right thigh, for over two years, a linear patch of lichen planus associated with marked enlargement and varicosity of her internal saphenous vein. He had treated the lesion in every way (including X-rays), without success, probably because the vein underneath was causing atrophy of the skin, and generally disturbed the nutrition of the part. While under observation she developed lichen spinulosus, in exactly the same sites as in Dr. Little's case, i.e., the forearms. He would examine her to see if atrophy of the scalp was present.

Dr. GRAHAM LITTLE (President) (in reply) said he had not gone into the question of tuberculosis in regard to this patient; he had only seen her once. She had had this eruption nine months, which was a somewhat long time for lichen scrofulosorum. The first patient had had atrophic patches in her axillæ as well as on the scalp, and that had also coincided with follicular lesions.

Pemphigus Confined to the Mucosa.

By E. G. GRAHAM LITTLE, M.D. (President).

THIS patient gave the history that she began to have small blisters in the mouth nine years ago, and she had had no condition affecting the skin, only the mucosa. Five years ago she became blind, owing to contraction of the conjunctiva. She has similar inflamed atrophic patches in the mouth and in the vulva also, and the vaginal orifice is narrowed by the contraction to very small dimensions, so that it will barely admit the little finger. She was sent to me for treatment as it was thought it might be a skin condition. A number of years ago she was seen by the late Mr. Nettleship, ophthalmic surgeon, and since I have had her under observation my colleague Mr. Paton, in the

Ophthalmic Department, has seen her at my request, and he agrees it is a case of essential shrinkage of the conjunctiva, which is a manifestation of pemphigus. I do not think a similar case has been shown at this Section, and I shall be glad of opinions.

DISCUSSION.

Dr. L. SAVATARD said that many years ago he saw a similar case, which was under Dr. Brooke's care, and that dermatologist diagnosed it as dermatitis herpetiformis limited to the mucous membranes. He (the speaker) could not recall whether the vulva was affected. The question was whether the disease here manifested should be called pemphigus.

Dr. H. SEMON said a similar case was described in a recent number of the *Archiv f. Dermatologie u. Syphilis*, and it was there discussed in great detail. In that case the lesion was proved to be pemphigus on account of the eruption subsequently appearing elsewhere on the body. The urine showed a marked diminution of sodium chloride, and this was regarded by the author of the article as an essential point in the diagnosis. That deficiency might be investigated in the present case for a confirmation of the diagnosis.

Dr. W. J. O'DONOVAN said he had seen a case parallel to that of Dr. Little, in which there were lesions on the conjunctiva, on the skin, and in the mouth. It was that of a soldier in the Army who was discharged on account of what was called sycosis. He treated that man for some months for impetigo on the body, and then he saw a typical outcrop of dermatitis herpetiformis. His eyes were being treated at the time for what at first was regarded as trachoma, and later as an ocular dermatitis herpetiformis ending in contraction of the eyes and total blindness. It was decided by the House of Lords that dermatitis herpetiformis might be precipitated by war conditions, and therefore a pension had been granted to the patient.

Hodgkin's Disease (?) with Pruritus.

By H. MACCORMAC, C.B.E., M.D.

PATIENT, a male, aged 64 years, came under the care of the exhibitor in December last year. Apart from scarlet fever, and tape-worm infection in August last—of which he has been cured—his previous health has been good.

The first symptoms of the condition for which he is now shown began two years ago as a very severe and generalized itching of the skin. About October, 1923, the glands in the groin were observed to be enlarged. When seen in December it was noted that the skin was generally thickened and infiltrated, but without nodules or other irregularities. The lymphatic glands were distinctly enlarged in the groins, axillæ, supra-clavicular regions, and in some other situations such as the lateral aspects of the thorax. There was no apparent enlargement of the liver or spleen, or hypertrophy of the tonsils.

One of the lymphatic glands was removed for diagnostic purposes, and showed the following features: the lymphoid tissue was generally reduced, and there was increase in the reticular elements. There were no giant cells and there was no increase in the eosinophils. A blood count on January 4 was as follows:

Hæmoglobin, 82 per cent.; red blood cells, 4,800,000; white blood cells, 12,800; polymorphs, 77 per cent.; lymphocytes, 11 per cent.; mononuclears, 10 per cent.; eosinophils, 2 per cent.

While an absolute diagnosis of Hodgkin's disease cannot be made, the condition at least may be strongly suspected, on the combined evidence of the

clinical condition, the histological characters of the lymphatic gland, and the blood count. The patient had, he relates, been treated for this condition with arsenic for some time previously at another hospital. He applied to the Skin Department at Middlesex Hospital on account of the pruritus, and it was decided to give him a series of injections of his own whole blood. These were administered on January 13, 20, 28, and again on February 12. The long interval between the last two injections was decided upon because a considerable febrile reaction followed the third injection.

The result of the treatment has been remarkable. After the first injection the itching became slight, the favourable progress continuing so that the patient is now completely free from his abnormal sensation. The skin gradually became soft and pliable; but the most striking feature is seen in the alteration in the lymphatic glands. These have steadily diminished in size—those in all positions, except the groin, being no longer capable of detection, and in the groin no more enlarged than is often the case in healthy subjects. Thus the treatment has relieved the patient of all external signs of disease.

While the blood injections were being given there was some loss of weight, but since discharge from hospital this loss has been regained.

It might be argued that the enlargement of the glands was secondary to the scratching, as is seen in prurigo, but the histological changes in the lymphatic gland are not in accord with such a view.

DISCUSSION.

Dr. M. G. HANNAY asked what quantity of blood was used for the injection. He believed the usual quantity was 10 c.c., and it had been suggested that less would suffice; recently in the out-patient department he had injected 3 to 5 c.c., and he had had a certain amount of success. He also asked whether a severe reaction, such as the one reported, was to be anticipated in other cases; if so, the treatment could not be used for out-patients.

Dr. A. M. H. GRAY asked whether there were any visceral complications in this case, also what was the duration of the pruritus, and which came first—the gland involvement or the thickening of the skin.

He thought the diagnosis of lymphadenoma was not a very firm one. They knew the enormous glandular enlargement which accompanied lichenization of the skin. The blood picture did not usually help in these cases, and he thought the histology of the glands was just as much in favour of the gland enlargement being secondary to the skin irritation, as due to lymphadenoma.

Dr. L. SAVATARD said he had at present under his care a girl, the clinical description of whose case would agree with that of Dr. MacCormac's case, but in his own patient the blood picture was not striking, and it did not suggest association with Hodgkin's disease. He attributed the enlarged glands to the intense irritation and consequent scratching of the skin. The skin was improving and the glands resolving.

Dr. MACCORMAC (in reply) said that he believed French authorities generally recommended the injection of 10 to 20 c.c., but recently equally good results had been said to follow the injection of 5 c.c.; the latter was the quantity he had used. Patients with Hodgkin's disease were unstable as regards their temperature. He had formerly treated this disease with the old salvarsan, and often found it caused a considerable febrile bout, sometimes lasting for weeks. In the case shown the itching began two years ago, the glandular enlargement five months ago. He had considered the point raised by Dr. Gray, but it seemed to him that the glandular enlargement in this case was much more allied to that seen in Hodgkin's disease than that following a chronic pruritus. The distribution of the glandular enlargement supported this view. The evidence in this case was at least presumptive of Hodgkin's disease.

Case of Unusual Tolerance to Chrysarobin.

By H. C. Semon, M.D.

My object in bringing this case is to raise the question whether the chrysarobin supplied in these days is as effective as it used to be. If so, this patient must have a remarkable tolerance to it. He has had more or less generalized psoriasis for nine years, and while under the care of a doctor in Jersey, he has been using increasing strength of chrysarobin ointments, until now he has been having daily inunctions of a 25 per cent. combination to the whole body, except the face and scalp, for the last fortnight. This has been done in a nursing home under my control, and there has been very little reaction, no sign of gastric or renal intolerance, and the psoriasis has involuted very satisfactorily.

I have been in touch with a firm of wholesale chemists who import the goa powder from India. This substance, which is collected from the cavities in the trunk of *Andira araroba*, was originally imported from Brazil. The export duty payable here resulted in the smuggling of the plant to India where it is now a commercial asset, and where possibly deterioration due to altered climatic conditions, or some other factor, may be responsible for its apparently reduced efficacy.

DISCUSSION.

Dr. Graham Little (President) said he had found a variation in the effects of chrysarobin, and it was largely a question of the quality of the drug, of which some very inert samples had been put on the market in London recently.

Dr. L. Savatard said that the same had been experienced in Manchester.

Dr. W. J. O'Donovan asked whether any Members had experienced the contrary. He had seen one boy in hospital who had been treated by his doctor, and another came to him privately, who had acute exfoliative dermatitis following chrysarobin inunctions. Evidently, therefore, there was an active preparation of the substance as well as a weak one.

Case for Diagnosis.

By H. MacCormac, C.B.E., M.D.

Patient, a male, aged 71 years, gave the following history of his condition. Fourteen years ago he had noted on the upper part of his back a small lesion which has gradually increased to the size of a five-shilling piece. During the last four or five years two similar but smaller lesions have developed one above the right clavicle, and the other on the abdomen. There are no abnormal subjective sensations. The appearances correspond in some slight degree with those seen in Bowen's disease: There is the red plaque of round form, covered with light scales, and the sharply demarcated edges. No atrophy can be detected. A section shows the following features: numerous vacuolated Malpighian cells, œdema in the epidermis and upper dermis, and at one point definite but slight prolongation downwards of the epithelium, rather in the form seen in precancerous than in frankly malignant processes. This feature allows the inclusion of the condition among the precancerous group to which Bowen's disease belongs.

DISCUSSION.

Dr. GRAHAM LITTLE (President) said that some of the cases of this group he had described had no rolled edge. He thought this case would come into the category of the cases which had been so much discussed recently, and which he had ventured to group under the title "erythematoid benign epithelioma."

Dr. L. SAVATARD said he had no doubt this was what was usually spoken of as superficial rodent, but he agreed with the exhibitor that it was a pre-malignant condition. He (Dr. Savatard) did not look upon pure basal-celled epithelioma as a *malignant* growth. The section presented showed sufficient proliferation of the basal epithelium to justify a positive diagnosis. If the block were cut further, bigger masses would probably be seen, though one might strike a patch in which there was no proliferation, because that part was atrophic. In this case there was very little evidence of a raised edge, though that was not essential. In a case of his own, half the lesion was pinkish and presented no edge and was somewhat atrophic, but a section of that part showed proliferation of the basal epithelium. The other part of the lesion was crusted over and ulcerated, and it presented a typical deep rodent ulcer. In some of these cases there was a transformation from basal to typical squamous epithelioma. There was no evidence that this was a precancerous dermatosis of Paget or Bowen. There was no dyskeratosis and no cellular infiltration of the corium.

Dr. MACCORMAC (in reply) said he considered Bowen's disease as one form of precancerous dermatosis. If the condition in the patient exhibited was precancerous, as he believed, it would tend to become malignant, and the downgrowth of cells seen at one point in the section showed this degeneration was actually taking place and thus demonstrated his contention. He had at the present time a similar case under observation in which a definite "rodent ulcer" had, after a number of years, developed on one of the precancerous lesions. He did not regard these conditions as malignant, and he did not think they were of the same type of eruption as those the President had described.

Dr. A. M. H. GRAY said he thought Dr. MacCormac's last point was a very doubtful one; there seemed to be no more tendency for these cases to become malignant than, say, cases of lupus vulgaris. He did not think they were much more malignant than benign cystic epithelioma of Brooke; and there seemed no need to attach the prefix the terms "benign" or "malignant." Dr. MacCormac's case he regarded as characteristic, clinically and histologically, of the cases which the President and others had described. They were a distinct clinical type of basal-celled epithelioma, not the same as the average rodent ulcer seen on the face. It was possible to cut sections through the middle of one of these patches without finding epitheliomatous changes.

Dr. GRAHAM LITTLE (President) expressed his agreement with Dr. Gray; the malignancy was so little that it was a misnomer to speak of the condition as "precancerous." It was as unlike Bowen's disease as it could well be, both histologically and clinically.

Case of Chronic Septic Granuloma of Face.

By W. J. O'DONOVAN, M.D.

THIS patient has an eruption on the right cheek of two years' standing of the size of the palm of the hand. It is red and well raised above the surface, it is without adenitis and without pain, or difficulty of mastication or fever. On its surface there are hair follicles filled with pus, discrete and few in number. The blood shows no abnormality, and the Wassermann reaction is negative. Cultures from the hairs pulled out show staphylococcus and *Bacillus coli*. No fungi were found or grown. X-ray epilation and malachite green afterwards effected a gradual cure.

Section of Dermatology.

President—Dr. E. G. Graham Little.

Case of Multiple Xanthoma.

By L. Savatard.

This patient, a boy aged 12, was sent to me by Dr. Rolland, of Bolton, as presenting a peculiar lichen eruption which began twelve months ago, on the flexor aspect of the thighs. There are no subjective symptoms. I think

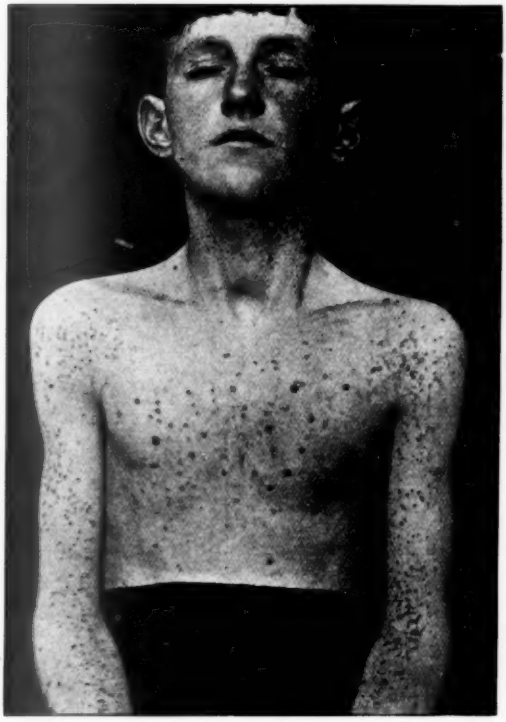


Fig. 1.—Anterior aspect.

you will agree that at the first glance the condition does strikingly simulate lichen planus. The lesions on the trunk and extremities are small superficial flattened papules, brownish-red in colour, of angular outline and not firm to the

touch. There is, however, no striation on the surface, and under diascopic examination the coloration disappears, giving a yellow stain. The palpebral lesions, which are only of three months' duration, are much yellower, and give the clue to the diagnosis. None of the lesions has resolved, and the condition is at present extending. The genital organs and mucous membranes are not affected. You will note, too, that the papules are not in the usual situations at the pressure points about the extensor aspects of the joints.

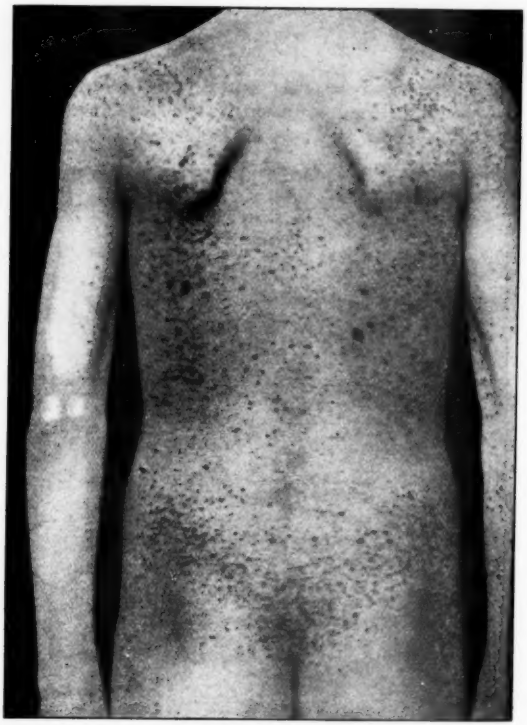


FIG. 2.—Posterior aspect.

The urine is free from albumin and sugar. The boy is otherwise healthy, and his previous medical history and that of other members of his family throw no light upon his condition. Dr. Haworth, of the Pathological Department of the Manchester University, who kindly tested his sugar tolerance, reports that the curve is not a typical diabetic curve, but shows, however, some impairment of the carbohydrate storage mechanism.

Microscopical sections, which I present, show numerous xanthoma cells in the upper part of the corium, apparently in places invading the overlying epidermis, which is stretched, the interpapillary processes being absent. Large

giant cells, with their nuclei arranged peripherally, are numerous. The epithelioid character of the cells is, I think, a marked feature of this section, and I show in contrast a section of another case of multiple xanthoma where the cells are more sebaciform in type and giant cells infrequent. Sections stained with Sudan III show the fatty deposit both within and without the cells. There is no infiltration of the muscular fibres.

I have shown the case because of its rarity, and in order to obtain your opinion with regard to the sections. I should also welcome advice as to treatment.

DISCUSSION.

Dr. F. PARKES WEBER suggested that the blood of the boy and his relations should be examined for hypercholesterolæmia. He supposed that a familial hypercholesterolæmia might be present in several members of a family in which (on that constitutional basis) only one or two members ever developed cutaneous xanthoma. Though probably only one person in many with hypercholesterolæmia developed cutaneous xanthoma, it was almost impossible to imagine the latter lesion developing without hypercholesterolæmia. Also was there, in this family, any frequency of aortic atheroma or atheromatous disease of the cardiac valves?

Dr. A. M. H. GRAY asked whether Dr. Savatard had satisfied himself that all the granules in the cells of this specimen were doubly refractive with Nicol's prisms. (Dr. SAVATARD: I have not tested that.) He (the speaker) had had three cases of xanthoma recently, and had made a careful examination with Nicol's prisms, and though there was a fair amount of doubly refractive substance in this case, it was not all so; some of the material he regarded as fat. There were some Maltese crosses of cholesterin, but also many globules which he did not regard as that substance at all.

Dr. J. M. H. MACLEOD said he did not know whether it was due to the early stage of the lesions, but in this case there were more connective tissue cells than in some cases of xanthoma. He knew what Dr. Savatard meant when he spoke of the sebaceous type or sebaciform cells.

Dr. SAVATARD (in reply) said he agreed that not all the substance seen in the section was cholesterin; probably some of it was pure fat, the fatty globules running together to form larger globules. But he did not think that had much bearing on the condition, or the diagnosis. The exhibitor would carry out Dr. Parkes Weber's suggestion of having the patient's blood and that of the other members of his family tested for hypercholesterolæmia, and would report the result later. In speaking of "sebaciform" cells he did not wish to imply that he considered the xanthoma cells of sebaceous origin.

Acrodermatitis Perstans in a Woman aged 44.

By S. E. DORE, M.D.

I THINK this case comes under Hallopeau's acrodermatitis perstans: and I have brought it hoping to receive some assistance from members as to classification and treatment. I think that in Hallopeau's cases there was generally some history of septic infection, such as septic onychia and peri-onychia. I have had at least five of these cases, but I have not seen, in them, any septic lesion which could account for these discrete pustules which occur on the palms and sometimes on the soles. In two of the cases I examined bacteriologically, the lesions have been sterile, and examination for ringworm fungus has always been completely negative. This patient had had the disease five years, and she was under Dr. Adamson's care nine months, and was treated with X-rays, and

with various tar preparations, but with temporary improvement only. In one of my cases the lesions cleared up rapidly under treatment by salicylate ionization, but the condition generally resists all methods of treatment.

DISCUSSION.

Dr. H. W. BARBER said he had recently studied several cases of the kind; the condition had usually been bilateral, and affected that part of the palm nearest the thumb. The eruption occurred more commonly in hot than in cold weather, and commenced with the formation of acute vesicles on an erythematous base. When caught at an early stage they were, as a rule, sterile; but they soon became secondarily infected with staphylococci. He also had searched for the ringworm fungus, without success. X-rays were often startlingly successful at first, but relapses had occurred in all his cases, whether he treated them with the rays or with crude coal tar. Most of these patients were very seborrhœic, and had lesions of seborrhœic dermatitis elsewhere; also they freely sweated, and on the palms, where there were no sebaceous glands, the sweat was more strongly acid than normal sweat. He thought the condition was not due to external infection, but, possibly, to the excretion through the sweat glands of some toxin, the inflammatory changes being secondary. The only successful treatment he had experienced had been along dietetic lines; he had not found any local treatment succeed permanently.

Dr. H. C. SEMON said he recently had a case with the same condition on the back of the hand, the history having been of three years' duration. The patient was a woman, aged 35, and she had been through every form of treatment, the condition eventually clearing up under X-rays. Against the view that it was an external infection was the fact that X-rays cleared it up; this one would not expect if it were a ringworm or yeast infection. He strongly supported Dr. Barber's idea that the condition was related to sweat secretion in the palm, and thought that what he had said as to diet was important.

Psoriasis affecting Mucous Membrane of Lip in a Girl aged 17.

By S. E. DORE, M.D.

I BROUGHT this as a possible case of psoriasis affecting mucous membrane. A patch began on the red surface of the lower lip as a result of a slight injury by a hair ribbon in a patient with well-marked psoriasis on the hands and fingers. In the May number of the *American Journal of Dermatology and Syphilology*, Dr. Max Sheer described one case and mentioned others in which the mucous membranes were attacked in psoriasis; in his case the hands and fingers were also mainly affected, but there was apparently a definite silvery scaling on the mucous membrane of the lips. The lesion in this case is not characteristic, but it is impossible to exclude psoriasis owing to the similarity of the lip lesion to those on the hands and fingers.

Ichthyosis in a Girl aged 13.

By S. E. DORE, M.D.

THIS is a well marked example of severe ichthyosis. Patient has been under Dr. MacCormac's care at the Middlesex Hospital for some time, and she appears to have been taken to Dr. Colcott Fox when she was only five years old. Two other members of the family are similarly affected, four are healthy

In this case the flexor surfaces are as much affected as the other parts, the palms to a large extent escaping. Dr. Barber suggested that it was what Brocq described as ichthyosiform erythrodermia, which he differentiated from ordinary ichthyosis.

DISCUSSION.

Dr. HALDIN DAVIS asked whether there was a history of the mother having had a desire for a large quantity of salt during her pregnancy. A French report had been published of a family of which several members were similarly affected, and the mother said she had always had an intense longing for salt during her pregnancies.

Dr. H. W. BARBER agreed that the condition shown by this patient must be differentiated from true ichthyosis. An important point was that the flexors were even more affected in this condition than the extensor surfaces, whereas the contrary was found to occur in ichthyosis. He regarded the condition as a gigantic nævus.

Case of Xerodermia Pigmentosa.

By Capt. DOBELL, R.A.M.C.

I SHOW this case partly because of its rarity, and partly because it is a typical text-book case. The patient's sister freckles in the summer also, but this passes off in the winter. This patient seems to have had nothing but freckling until he went to Salonica. Three weeks after going there he had septic sores of the skin, and the skin generally became very rough and painful. After some time he was invalided home on account of malaria. He was in this country a year, and then went out to Mesopotamia. While there he had various lumps, one or two of which were cauterized, and one or two cut out. There were also some enlarged blood-vessels. Then he went to Constantinople, where he suffered from the same condition. The case was not diagnosed until three or four months ago, when sections were cut, and the diagnosis of squamous-celled carcinoma arrived at. When he was sent to me I regarded his case as one of xerodermia pigmentosa. I propose to have him treated with diathermy.

Dr. L. SAVATARD referred to a patient with a similar condition who came to him at the outbreak of the war, and on having the condition certified the patient was not sent on active war service. With the history that Captain Dobell had given it was possible that the precaution might have saved that patient from developing numerous carcinomata of the skin. He had seen other cases in which the lesion had developed later in life on what had apparently been normal skin. It was interesting to know that it was a squamous-celled carcinoma; he did not think a basal-celled carcinoma would develop on this condition.

Case of Lymphangioma Circumscriptum of the Axilla.

By E. G. GRAHAM LITTLE, M.D. (President).

PATIENT, a woman aged 32. The history is that this patch has persisted since infancy. It is now of the size of a five-shilling piece, and from time to time it becomes hæmorrhagic. It is notable that the lesion occurs in the axilla and on the left side of the body. Both these features have been noted as of frequent occurrence, especially in American reports.

DISCUSSION.

Dr. PARKES WEBER asked whether there had been lymphorrhœa (running of lymph) rom the lesion.

Dr. L. SAVATARD said that recently he had treated such a case with diathermy; at present there were excellent results; there was a very nice scar. At first he thought the scar would be thickened, but it was supple. It was on the neck of a boy, and there was considerable running of lymph in that case.

Case of Multiple Benign Tumours of Schweninger and Buzzi.

By E. G. GRAHAM LITTLE, M.D. (President).

PATIENT, a middle-aged woman, who suffers much from rheumatism. She has never paid much attention to the tumours, which give her no discomfort and which have always, as far as she knows, been of this same type. They are distributed chiefly upon the buttocks and thigh, and are few in number. The swellings are of the colour of the normal skin, or, if anything, slightly whiter. They appear like flaccid folds of skin and the finger can be pressed into them with the sensation of passing into a shallow depression. At no time, according to the history, has there been any solid content. She shows in addition to these lesions considerable linear atrophy of the skin—striæ cutis distensæ—both on the abdomen and on the thighs.

The case seems exactly like that described by Pringle and MacCormac in the *British Journal of Dermatology*, 1919, xxxi, p. 43.

Professor OPPENHEIM (Vienna) said he could not see in the present case any tumours or pigmentation of the skin, which must be present in cases of Schweninger and Buzzi's tumours. He agreed there was a feeling as if the examining finger were passing over a hole in the skin. He regarded the case as atrophia maculosa cutis or anetodermia maculosa, and considered that the tumours were due to a prominence of the atrophic skin, hernia-like, the so-called tertiary stage described by himself in such cases. Histologically, one found in such cases fat-tissue in the higher layers of the skin instead of the degenerated connective tissue of the stratum reticulare. Such macular atrophic skin changes were mostly observed in elderly women after the menopause or in women with troubles of menstruation. He supposed that there was some internal secretion influence which destroyed the elastic tissue and the atrophy was the consequence of it.

Case for Diagnosis.

By E. G. GRAHAM LITTLE, M.D. (President).

PATIENT, a male, aged 49. Eight months ago he developed a small growth upon his tongue which was removed by Mr. Hogarth, of Nottingham, and pronounced to be a papilloma. His teeth were removed at this time. The present condition has slowly spread under the tongue at its base behind the tip. There are no lesions of the skin anywhere visible. Covering the under surface of the tongue for an area about an inch long and half an inch wide there is a flat white patch, hardly raised above the level of the skin and apparently of thin consistency; in some places the reddened tongue shows through. He suffers no discomfort and no treatment has so far been applied.

A Case of Epithelioma developing upon Lupus Erythematosus, X-rayed.

By E. G. Graham Little, M.D. (President).

The history of this patient is rather remarkable. About ten years ago she was supposed to be suffering from ringworm, as diagnosed by a doctor in Ramsgate, who appears to have sent hairs to be examined for ringworm and to have found fungus. He accordingly either administered X-ray treatment or ordered it to be given. Upon the patch so treated lupus erythematosus subsequently developed and spread over a large area of the scalp, and the case was shown by me in 1918 as a probable example of lupus erythematosus set up by radiation. She has now come to me again after an interval of six years with this new growth upon a portion of the scalp, upon which there had been lupus erythematosus. (Sections demonstrate epithelioma.)

Pemphigus limited to Mouth and Larynx.

By F. Parkes Weber, M.D.

The patient (D. A.) is a Polish Hebrew tailor, aged 51, who has complained of more or less hoarseness since December 1923, and of a little pain occasionally in his throat, especially on swallowing. In February, 1924, before I saw him, he was suspected of suffering from faucial or intra-oral diphtheria, and was sent



Fig. 1.—To illustrate the condition in Dr. Parkes Weber's case of pemphigus of the mouth, June 17, 1924.

to a fever hospital, but was soon discharged with a negative diagnosis. Examination for diphtheria bacilli was again negative in March. He has been under my observation on and off since the second week in March, that is to say, for over three months. During all this time, but with variations from time to time in the degree, he has had somewhat (by no means perfectly) symmetrical red

patches of superficial inflammation or "erosion" in the mouth, including the floor of the mouth, below and at the sides of the tongue, the soft palate, the posterior pharyngeal wall, the buccal mucosa, and the mucous membrane of the lips; at one time a small white patch was located exactly at the tip of the tongue. The patches are generally more or less completely covered by white macerated epithelium, often resembling false membrane (compare fig. 1, from a photograph by Dr. E. Bock, taken in June, 1924). The upper part of the larynx (Mr. W. Wilson) has been found similarly affected.

Bacteriological examination of the whitish material (Dr. E. Bock, May, 1924), shows the ordinary mouth flora, including staphylococci, streptococci and a few spirilla. Film preparations show pus-cells, but no definite yeast-like or oïdium-like cells. The Wassermann reaction for syphilis, taken twice (March and May, 1924), is completely negative. Röntgen-ray examination of the lungs (Dr. J. Metcalfe) suggests the presence of tuberculous lesions, but clinically there seems to be no active tuberculosis. Examination of the mucous-looking sputum reveals no tubercle bacilli and, though the Pirquet cuti-reaction is strongly positive, I believe that his tuberculosis is quite quiescent. No evidence of any other visceral disease has been discovered. Urine (June, 1924) free from albumin and sugar; the collected twenty-four hours' urine contains 8 grammes sodium chloride (not abnormal). Blood-count (Dr. G. Welsch, June, 1924): erythrocytes, 6,240,000 per c.mm. of blood; hæmoglobin, 85 per cent.; white cells, 9,100 per c.mm. of blood (polymorphonuclear neutrophils, 60 per cent.; lymphocytes, 31 per cent.; large mononuclears and transitionals, 1 per cent.; eosinophils, 7 per cent.; and mast-cells, 1 per cent.).

Treatment has been local—by various mouth-washes, gargles and glycerine —and general, especially by subcutaneous injections of sodium cacodylate and (later) by intravenous injections of neosalvarsan. At one time he improved greatly, but has since relapsed, and on the whole the condition is now worse than it was in March. I suggest the diagnosis of *pemphigus, as yet limited to the mouth*, unless the spreading of the pemphigus beyond this limit is already suggested by the recent appearance (two weeks ago) of a small inflammatory patch on the upper lip (somewhat sycosis-like) just below the left nostril (this patch is shown in the illustration), and a little inflammatory redness about the inner canthus of each eye.

In the third edition of Radcliffe Crocker's "Diseases of the Skin" (1903, pp. 269-275) various references to cases of pemphigus vegetans commencing in the mucous membranes of the mouth, &c., are given. Sir Jonathan Hutchinson seemed to have met with a milder type of cases in which the skin was only slightly affected. Sir Malcolm Morris and H. L. Roberts (*British Journal of Dermatology*, London, 1889, vol. i, p. 170) referred to several cases of pemphigus, commencing in or confined to the mucous membranes (conjunctiva, mouth, throat, &c.). E. M. Cocks (*Journ. Amer. Med. Assoc.*, Chicago, 1906, xlvii, p. 1736) has recorded a fatal case in which the eruption was completely limited to the mucous membranes. A summary of cases of pemphigus of the mucous membranes was made by J. Méneau at about the same time (*Journ. des Maladies cutanées et syphilitiques*, Paris, 1905, sér. 6, vol. xvii, pp. 1-59).

DISCUSSION.

Dr. GRAHAM LITTLE (President) said the clinical appearance of this case was so like that of a case he himself showed at the last meeting, pemphigus limited to the mucosa,[1] that he thought it was as Dr. Weber said. In his own case the conjunctiva, mouth

[1] *Proceedings*, 1924, xvii (Sect. Derm.), p. 75.

and vulva were involved. The conjunctival condition had caused blindness after seven years' duration of the disease; and the vulva was so seriously affected that its orifice was reduced to the calibre that would only admit an ordinary writing pencil.

Dr. A. M. H. GRAY said he had suggested to Dr. Weber that this might be erythema multiforme of the mouth. Eighteen months ago he (the speaker) brought to the Section a case of recurring superficial ulcerating plaques in the mouth for diagnosis. There had been these recurrent attacks in the mouth for three years, and Dr. Pernet suggested at the meeting that the condition might be herpes iris. Three months later he (Dr. Gray) brought the patient with typical herpes iris of the hands, the first attack of it he had had. The present case of Dr. Weber's was not quite like that. The other case had well marked intermissions; he was free from lesions for some time, then they



FIG. 2.—To illustrate the condition of the skin in Dr. Parkes Weber's case, about August 25, 1924.

came out in crops, and behaved in much the same way as erythema multiforme of the hands behaved. In Dr. Weber's case the lesions were continuous. The areas affected in the two cases were identical.

He had recently had a patient with pemphigus vegetans whose lesions began in the mouth, and she died in less than a month from the commencement of the skin disease. The lesions had been present in her mouth for some months before that. He was inclined to think that this was a case of pemphigus.

Dr. HALDIN DAVIS said the case reminded him of one he showed three years ago, in the person of a middle-aged female. In her case the first sign of illness had been a

hoarseness of the voice, and she was treated for some time under the suspicion that she would exhibit a positive Wassermann reaction, but when the reaction was taken it was found to be negative. Later, the diagnosis of pemphigus was made, and it was confirmed by the appearance of blebs on the skin. This diagnosis was generally supported by members of the Section when the case was shown, and most of those present had agreed with the speaker in taking a grave view of the outlook. He was glad to say that that prognosis had not been justified, for the patient had remained fairly well up to the present time. He had found the administration of quinine in fairly large doses very beneficial to her; she took 5 gr. three times a day, and while doing so no fresh lesions had appeared on the skin, but she had never been entirely free from lesions on the mucous membranes. Sometimes these appeared on the tongue, sometimes on the lips, or the palate or within the larynx. He quite agreed with Dr. Weber's diagnosis in the present case.

Professor M. OPPENHEIM said that pemphigus was much commoner in Vienna than in London, and Neumann regarded the prognosis as very grave if the pemphigus began in the mouth. He (the speaker) regarded the present case as one of pemphigus, and he believed this patient would develop later the cutaneous lesions of pemphigus vegetans. If this were erythema multiforme there would be more inflammatory symptoms in the mouth. He remembered smiliar cases beginning at about the age of 35, with irregular mouth affections, and two years later the patients died of pemphigus vegetans.

Postscript, August 27, 1924.—Since the meeting a condition of generalized cutaneous pemphigus has gradually developed, which is well shown in the accompanying illustration (fig. 2) from a photograph taken about August 25, 1924. No " vegetation " has as yet occurred in connexion with any of the lesions. The case cannot therefore so far be called one of " pemphigus vegetans." The mucosal pemphigus has become rather less. Various methods of treatment have been tried, including quinine.—F. P. W.

Persistent Erythema Multiforme.

By G. B. DOWLING, M.D.

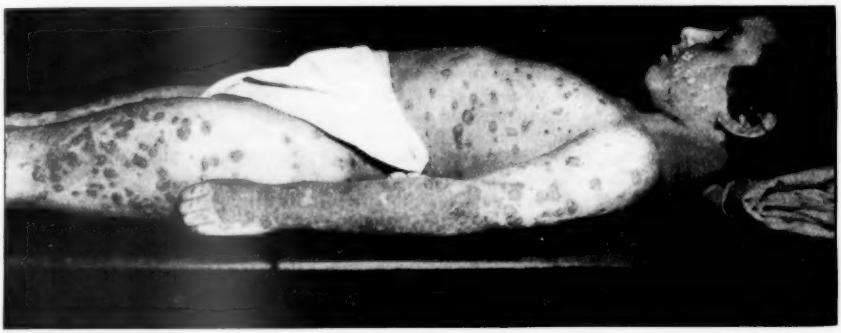
FEMALE, aged 17. I saw this case three years ago at an infirmary. I considered then that she was a case of bullous erythema multiforme, and would probably clear up satisfactorily in the course of weeks. She exhibited the erythematous lesions with bullæ scattered over the greater part of the surface, with involvement of the mouth and conjunctivæ. Two days ago she came to Guy's Hospital, and apparently there has been no tendency for the lesions to clear up. There are now to be seen a large number of circinate and gyrate erythematous lesions covering the general surface, and, to some extent, the face and hands, pigmented in the centre, with a slightly raised, pinkish margin. Elsewhere there are bullæ, which are not persistent, but appear in crops, and disappear in three or four weeks. The mouth lesions at one time got almost well. Some of the bullæ have developed upon healthy looking skin, others in the centre of the erythematous patches.

Case of Psoriasis Juvenilis treated by Mercury Vapour Light Baths.

By W. J. O'Donovan, M.D.

This is a record of an experimental treatment in which it is essential that I first show the portrait of the patient before treatment to contrast it with her present state. This well built little Jewess, aged 6½, was admitted into Dr. Sequeira's ward at the London Hospital on March 8, 1924, suffering from a generalized psoriasis of at least six weeks' duration. The face and arms, trunk and limbs were all affected; the eruption was discrete centrally, and confluent on the forearms, legs and cheeks. The family is free from skin complaints.

For a month the child was kept in bed and treated only with thymus extract, 5 gr. t.d.s., without any improvement. She was then ordered light baths, a fifteen-minute exposure daily of the whole body to a Cooper



Hewitt M2 lamp at a distance of a yard. No ointments were applied and no medicinal treatment was attempted. After fifteen exposures to light, i.e., in a fortnight, the trunk was clear of all lesions, showing pale areas where the scales were against the slightly pigmented unaffected skin. Six carious teeth were now removed. In another fortnight it was noted that the extremities showed only a little improvement. The treatment was persevered in, and after a total of fifty-two exposures the child's skin was, as is shown to-day, quite free from any psoriatic lesions. Throughout there was no local or general reaction to the light therapy.

Dr. Graham Little (President) said the treatment had been extensively used in America. Sir James Galloway brought a case which had been treated for psoriasis for three or four months in the Rocky Mountains, where the patient had been able to go about practically without clothes. The psoriasis diminished enormously as a result, but lupus erythematosus developed on the back.

Case of Psoriasis persisting for Four Years; Complete Clearance after Treatment by Irradiation of the Thymus Gland.

By H. C. SEMON, M.D.

PATIENT, a girl, aged 7, is the fourth of a series I am treating by irradiation of the thymus. I have found that my dosage has been incorrect, and that is the probable explanation of my three other cases having failed. The correct dosage, according to Walter Brock's original paper,[1] which is well worthy of notice, is, for adults, half an epilation dose through a 2 or 3-mm. aluminium filter. In children up to 4 years old, not more than a fourth to a third of a pastille dose should be given, and these doses must not be repeated within two months. Therefore I have probably been paralysing the thymus by overdosing, instead of stimulating it. Brock, in practically all his cases, brought about involution of the lesions, and in two or three of them there was no relapse. This might be the method of choice in children, when you wish to spare the mother the trouble of rubbing in the ointment day by day.

DISCUSSION.

Professor OPPENHEIM said that in a series of about thirty cases there was a relapse in nearly all in a short time. In some cases there was relief, but there was relapse after a time. In the case of girls up to 6 years of age they became free sometimes in eight days, but they also had the seborrhœic condition and dandruff of Unna, and excess of sweat in the axillæ simultaneously. In some cases the treatment had no effect. Psoriasis should be treated by all known methods, not by one chosen method. Among thirty cases in his department, only one remained free for six months. It was a question of the influence of the internal secretions. He had not seen any benefit follow the administration of thyroid gland. The treatment rendered the causal influence in psoriasis clearer.

Dr. M. G. HANNAY said he put a certain number of psoriasis patients on thymus gland by the mouth, at the same time keeping them on external treatment. He was not satisfied there was any improvement attributable to the thymus gland.

[1] *Strahlentherapie*, 1920, xi, p. 563.

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SECTION OF ELECTRO-THERAPEUTICS.

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Section of Electro-Therapeutics.

President—Dr. W. J. TURRELL.

Electrical Energy as a Curative Agent.

PRESIDENT'S ADDRESS.

By W. J. TURRELL, M.D.

[DR. TURRELL, after thanking the Members of the Section for his election to the presidency, continued as follows] :

In a rapidly developing specialty such as ours, Membership in a section of a society where we can meet to hear and discuss the views and ideas of our fellow workers, is absolutely indispensable for the advancement of knowledge and for our mutual advantage. To country members, after a hard day's work, it requires, as I know from personal experience, a certain amount of energy and resolution to journey to London, take part in a discussion, and arrive home about 2 a.m., and to those living farther away the sacrifice of time is proportionately greater.

But I am convinced that country members, however distant their homes may be, are more than repaid for any trouble they may take or any sacrifice they may make by the many advantages they derive, by the friendships which they establish with their fellow workers, and by being brought into closer contact with modern views and into personal touch with the most recent work. In fact I think that it may be laid down as a general rule, that the farther removed from London a member may reside the more essential it is in his own interests that he should be regular in his attendance at our meetings. I should very much like to see a considerable increase in our country Membership and more representatives from a distance taking part in our discussions; the Section as a whole would undoubtedly benefit thereby, but not to the same extent, perhaps, as the Members themselves.

I think that I shall make the best use of this opportunity by making my address take the form of a general survey of the present position of electrotherapy; that is to say, a brief consideration of the steps by which we have arrived at our present position, where we stand, and whither we are trending.

A study of the history of electrotherapy shows us that the earliest kinds of electrical treatment took the form of the administration of electrical shocks, and this has largely proved an "hereditas damnosa," since many patients, to this day, invariably and timorously associate all forms of electrical treatment with the administration of shocks.

It was by shock from a torpedo fish that Anthero, a freedman living in the reign of the Emperor Tiberius, was cured of gout about the year 1 A.D. It

was by the shock from an early form of static machine that Kratzenstein, in 1744, cured an apparently functional contraction of the thumb. It is interesting to note that even in the earliest times the application of electricity in medicine was neither purely accidental nor empirical, for Pliny, in his "Historia Naturalis," logically deduced from the attributes of the torpedo fish that considerable therapeutic benefit might be derived from a creature endowed with such natural powers.

In 1747 and 1748 the now much disputed theory of ionic medication was foreshadowed by the attempts of Privati of Venice and Veratti of Bologna to introduce by electrical means the medicinal virtue of drugs, so that medicine might be made to act without being taken into the stomach. The theory attending such a procedure was at that time short lived, for by reason and ridicule Priestley and others promptly demonstrated the fallacy of the theory. It was upon electric shocks that Lovett, Wesley and Marat chiefly relied to obtain therapeutic effects by the administration of electricity, a view also supported by Franklin.

About 1780 the Abbé Bertholon, in a paper entitled: "The Influence of the Electricity of the Atmosphere on Diseases" originated the theory that all diseases were due either to excess or deficiency of the electric fluid. In the first case he drew off the excess fluid from the back of the hand of an electrically charged patient, in the second he used the simple static charge. Marat the notorious revolutionist, in an acrimonious disputation with the Abbé, rejected the idea that atmospheric electricity had any influence on the human economy. In his interesting book, Marat also definitely laid down the theory that artificial as opposed to atmospheric electricity alone possessed any therapeutic value. It was upon shocks and sparks from the static machine of his period that Marat relied.

In 1791 Galvani's great work on the excitation of muscular contraction by electricity was published. It should not be overlooked that Galvani himself distinctly states that he was induced to undertake this work in order to render electricity safer in the treatment of disease. He at first maintained, as the result of his work on animal electricity, that the current came directly from the muscles and nerves themselves, in fact he is stated to have held the view that he had in this way discovered the origin of life. The apparent restoration of function in the leg of a dead frog, on the application of the galvanic current, naturally led to its use for the restoration of the apparently dead. Valli, for instance, claimed that he had restored to life fowls which had been drowned. The fundamental idea which appears to have obsessed the workers of that period was that the electrical force was life itself. Napoleon, indeed, went even further, and was more exact and definite in his views, for on seeing Volta's pile, he is reported to have exclaimed: "Behold the representation of life; the vertebral column is the battery, the bladder is the positive and the liver the negative pole."

The remarkable work of the great Englishman, Faraday—to the value of whose experiments perhaps the highest testimony is to be found in the extent to which many of his views and theories have stood the test of modern discoveries—paved the way for the accurate and valuable investigations of Duchenne, of Boulogne, which were of the greatest importance in laying a scientific foundation both for electrotherapy and neurology.

In this country, Faraday's investigations instigated the scientific and philosophical researches of Dr. Golding Bird, of Guy's Hospital, into the action of electrical energy in its application to medicine. From this time until the

commencement of the present century little, if any, progress was made in the rational explanation of the mode of action of electrotherapy. Attention was mainly directed towards the physical side of the question, to the design and invention of new apparatus and new methods. In this direction great progress was made, as is perhaps best shown by the far-reaching work of D'Arsonval, who commenced his experiments on the high frequency currents in 1888. In this country it is difficult to overestimate the debt we owe to Dr. Lewis Jones, who by his courteous tact tided electrotherapy over a period during which it was viewed with great suspicion by the medical profession.

A perusal of Lewis Jones' writings gives me the impression that he was more interested in the physical than in the biological side of electrotherapy.

Partly as the result of the neglect of the biological and physiological aspect of electrotherapy by the writers of this period, and partly as the result of the blatant advertising of such apparatus as the Harness Electric Belt, a quack and impotent instrument advocated by some of the leading members of the medical profession, we find that ideas relating to the curative action of electricity were exceedingly crude, indefinite and inaccurate; they had, I almost think, receded rather than advanced since the time of Duchenne and Golding Bird. An outcome of this electrical treatment was regarded by the general public as the administration of some mysterious vital force, the *vis nervosa* of the older writers; and the medical profession looked upon it as a form of treatment by suggestion, a form of treatment which in those days was scoffed at and held in scorn instead of being rightly extolled, as is the case at the present day.

Confirmation of this view is to be found in the more advanced books on electrotherapy of that period. Dr. H. Bordier, in the preface to his valuable book, "Précis d'Electrotherapie," 1902, finds it necessary to refute the views of Moebius (Congrès d'Électrothérapie à Francfort, 1893), that electrotherapy "rests altogether on suggestion," and of Crocq, fils, that "electricity frequently acts by suggestion" (Congrès de Med. de Bordeaux, 1895), and of Rosenbach, that "the cures which follow the application of electrical treatment are the result of natural causes" (Congrès d'Électrothérapie, Francfort, 1893). The spectacular character of the high frequency sparks naturally contributed to such views, but from these impressive discharges has developed the convincing method of diathermy.

In the foregoing remarks I am far from criticizing adversely the earlier workers; the course of events has been in the natural process of evolution, for it is necessary to discover the physical methods, and we must possess the necessary apparatus before we can investigate their biological action.

During the past ten or twenty years, in the absence of any striking innovation in our physical equipment, we have turned more to the consolidation of our position by studying the physiological and biological effects of the powerful and fundamental methods we possess of utilizing electrical energy as a curative agent.

A study of the modern text-books of physiology and biology affords the electrotherapist a most encouraging outlook, and one pregnant with immense possibilities, when we consider the ever-increasing part which electrical energy is found to play in all vital phenomena. Contrast for instance the physiology text-book of twenty or thirty years ago with such a modern work as Professor Bayliss's "Principles of General Physiology." It is to the work of the physiologist and biologist that we have looked in the past few years, and must look more and more in the future for the explanation of the action of electricity in medicine.

From the investigations of the physiologists we deduce that the action of the electrical current, especially in its interrupted forms, is mainly the outcome of the relatively high velocity of the hydrogen ion. That is to say, the velocity of the hydrogen ion is so high compared with that of other ions that when we abruptly make or increase in strength an electric current through the body, the hydrogen ions momentarily concentrate in excess of the normal equilibrium at the negative pole; thus a hydrogen ion, or acid concentration, takes place at the negative pole and causes a muscular contraction or some phenomena of a sensory or other character in the neighbourhood of the negative pole.

When we break or abruptly decrease in strength an electric current, already established through the body, the hydrogen ions have been travelling with such relatively high velocity from the positive pole that at the moment of break there is a temporary deficiency of these ions in that region, leading to an hydroxyl ion or alkaline concentration which in turn excites similar phenomena in the neighbourhood of the positive pole.

Thus we see that the old idea of polar inversion is incorrect, for the contraction at make invariably proceeds from the negative pole and the contraction at break from the positive pole, consequently there cannot be such a thing as A.C.C. or K.O.C. such contractions being in reality K.C.C. and A.O.C. deeply seated and diffused in the tissues, and this explains the sluggish and diffused character of the contractions so excited.

The more abrupt the interruption and the higher the voltage of the current, the more definite will be this hydrogen ion concentration and the more vigorous will be the resulting contraction.

A certain minimum duration of time is, however, necessary for a resulting contraction to take place, and, the higher the voltage employed, the more brief will be the minimum duration required to excite a contraction. These factors explain the phenomena of high frequency and diathermy currents, for the reversals are so frequent that the duration of the current is insufficient to permit the ionic concentration.

You will remember that D'Arsonval, in his original paper in 1888 on his investigation into the action of rapidly interrupted alternating current, an investigation which led him to the discovery of the high frequency currents, found that until 2,500 or 5,000 interruptions a second were reached, the voltage remaining the same, the resulting contractions increased in vigour as the frequency of interruption increased: but after reaching this frequency the vigour of the contractions decreased with an increase of frequency. It was not until two years later that D'Arsonval was able to obtain an instrument of sufficient frequency to abolish all contraction and sensation, thus obtaining the characteristic phenomena of high frequency currents.

The theory of the hydrogen ion not only explains the more obvious phenomena of high frequency and diathermy currents, but it also shows why some so-called high frequency currents of very high voltage excite a certain amount of muscular contraction and sensation, the voltage of such currents being proportionately too high for their frequency.

We naturally and correctly conclude that the heat generated by these currents, which we find of such great and far-reaching therapeutic value, is the result of the violent oscillation of the ions. Apart from the heat generated, there is, I am convinced, though the proof of this cannot be so clearly demonstrated, a very valuable therapeutic action from the vibratory or oscillating character of these currents on the tissue cells, which is not only

of a stimulating nature, but has a useful effect in accelerating diffusion through the various tissue membranes. We have also to consider a possible condenser effect on the tissue cells insulated by their non-conducting cellular membrane from the oscillations of the ions occurring in the electrolytic medium with which these cells are bathed. The very marked diminution of effusion which occurs in the direct path of the current when treating a recently sprained ankle or a recent Colles's fracture by diathermy, cannot be fully explained either by the heat generated by the current or by the pressure of the pads applied. Take, for example, a bottle containing leaden shot, the orifice of which is closed by a parchment membrane pierced with holes barely large enough to permit the passage of the shot. On merely inverting such a preparation the shot will not pass through, but on shaking the bottle violently some of the shot will be forced through the holes in the membrane by the force with which they bombard it, and the more vigorously the bottle is shaken the more quickly will the shot pass through. This simple illustration affords a possible explanation of what occurs with high frequency and other electrical currents when the tissue and cellular membranes are bombarded by the ions conveying the current: the force of the bombardment and the rate of diffusion being directly proportional to the voltage employed.

The recurrent hydrogen ion concentration also explains that most interesting form of treatment, the Leduc current. We may not unnaturally expect striking results when we employ currents of high intensity or voltage, but we are hardly prepared for the very remarkable results which we can obtain from the rapid interruption of currents of such low intensity as 1 or 2 milliamperes.

The Leduc current is obtained by interrupting a unidirectional current of 1 or 2 milliamperes about 100 times a second. By utilizing such a current, Leduc has claimed that he can produce, experimentally in animals, single contractions, tetanus, local anæsthesia, general anæsthesia, coma, paralysis of the heart and respiration, and finally death.

It occurred to me during the war to use this current in the treatment of the terrible and intractable pain of causalgia. The treatment was attended by unfailing and most marked success, and in a similar condition in an undergraduate last June at the Radcliffe Infirmary, the treatment was completely successful in relieving the pain, after all the usual methods available in the hospital had failed. In this connexion it is interesting to note that in the Research Committee's report on the diagnosis and treatment of peripheral nerve injuries, referring to the treatment of causalgia, the following passage occurs: "Little benefit can be hoped for from electricity." This action of the Leduc current can be explained by the exhaustion of the sensory receptors from over-stimulation: a phenomenon analogous to that of the fatigue resulting in the recovering end-plate of motor nerves from over electrical stimulation, the over-stimulation in each case being occasioned by the frequently recurring hydrogen ion concentration.

When we turn to the consideration of the constant or continuous current the matter is not so simply explained, for we have to deal with it in two parts: (1) its polar and peri-polar action, and (2) its inter-polar action. The velocity of the hydrogen ion does not in this case play so dominating a part, for when the current is established and undergoes little or no variation in intensity, there is no marked disturbance in the equilibrium of hydrogen ion concentration. The higher velocities, however, of the fastest kation (the hydrogen ion) and the fastest anion (the hydroxyl ion) and the immense numbers of these ions render them by far the most important factors in the conveyance of the current

through the electrolyte of the body; for the conductivity, and conversely the resistance, of an electrolyte depend on the number of the ions present and their velocities. Such an overwhelming proportion of the current is carried by the hydrogen and hydroxyl ions when transmitted through the tissues that the once cherished theory of ionic medication is impossible of realization; the path of by far the least electrical resistance is that through the tissue ions, the medication ions, of far lower velocity and far less numerous, have only a negligible share in the transport of the current and are consequently introduced in an almost inappreciable quantity. Considerable time must necessarily elapse before a theory, so widely held as that of "ionic medication" can be fully eradicated, but even those who still hold this theory have very considerably modified their views: the theory of the deep penetration of medicated ions has been entirely abandoned, and the very small amount of the drug capable of being introduced beneath the cuticle has been more accurately stated. A distinguished French worker ("Dosage de l'iode introduit et élimne dans l'ionisation."—G. Bourguignon, *Journal de Radiologie et d'Electrologie*, Mai, 1923, p. 217) has recently attempted to show that superior results are likely to follow when iodides are electrically introduced than when they are administered by the mouth, because in the former case he claims that they are more slowly eliminated. Unfortunately, although his experiments to show the slow elimination of iodides, when electrically introduced, were most carefully performed, no control experiments were made to show the time of elimination when iodides were taken by the mouth. Reference to pharmacological literature, however, shows that the iodides are in part slowly excreted when taken by the mouth. Most of a single dose is excreted within a few hours, but the remainder is eliminated very slowly. With a dose of 7 to 15 gr. the iodide reaction persists in the urine for forty hours, after larger doses or continued administration traces persist for twenty days. With a single dose of 15 gr. administered to a man, 80 per cent. was excreted in twenty-four hours, and 20 per cent. between twenty-four and forty-eight hours. Iodized fats and fatty acids, given by the mouth, are eliminated even more slowly, so that, even if the theory of the slower elimination of the electrically introduced iodides were proved, which in the absence of control experiments is certainly not the case, nothing would be gained, for a still slower excretion can be obtained by the more simple procedure of administering iodized fats by the mouth.

In regard to the polar and peri-polar action of the constant current, much misapprehension has arisen from a loose and erroneous terminology; when we speak of zinc or copper ionization, we are in reality referring to a process of de-ionization rather than one of ionization. Since the current beneath the cuticle is mainly carried by the tissue ions, those ions conveying the current from the pad or electrode to the surface of the body on reaching the superficial tissues, are free to enter into combination with the chlorin, oxygen or other ions set free by the electrolytic action at the poles. The process would, I think, be better and more correctly termed "electro-deposition"; it is analogous and in every way comparable with electro-plating, except that a caustic compound is deposited instead of an inert metal. And, as in electro-plating, the deposition takes place on the superficies of every crack, crevice, and cavity of the surface treated.

This powerful, permeating caustic action, limited in its depth penetration, renders this process the safest, simplest, most thorough and most valuable method for the treatment of septic surfaces, sinuses and cavities. Its general

adoption in the past appears to me to have been hindered by its consideration, chiefly in reference to its application to special parts and regions under distinctive and bewildering titles, such as the treatment of gonorrhœa by electrolysis, the electric ionization of otitis media, the treatment of endometritis by zinc ions. A different name in each case for fundamentally identical methods! No wonder that confusion exists in regard to the value of this treatment.

If we clearly define the fundamental principle upon which this treatment is based, namely, the superficial deposition by means of electricity of a caustic agent, not only electrotherapists, but even the surgeons themselves will be able to realize the value of its application to every suppurating or indolent surface, to suppurating sinuses, and to a diseased lining of any accessible cavity of the body. Dr. Golding Bird, who with Sir Thomas Spencer Wells introduced this form of treatment about the year 1849, as clearly realized and explained its mode of action as any modern writer on the subject.

We have been too much inclined in the past to regard this merely as a surgical action of the constant current and to disregard the importance of this polar action when administering the constant current for medical purposes. When we use solutions of less caustic and irritating substances, such as sodium salicylate, sodium chloride, quinine sulphate, we obtain a varying stimulation, in place of a caustic action on the skin, and this directly and reflexly influences our treatment to the benefit of our patient. In this way may be explained the difference in results obtained by some electrotherapists when using different solutions with which to moisten their pads, for instance the superior results claimed in the treatment of tic douloureux when soaking the positive pad with a solution of quinine sulphate.

In reference to the above point, I have recently performed a series of experiments, which simple as they were, are not, I think, without interest.

I will not weary you with all the details of the experiments, but will briefly refer to their essential points and the conclusions I draw from them. The patient treated was an intelligent and tolerant man suffering from early rheumatoid arthritis of both knee-joints. Electrodes and pads of the same size were similarly placed in each experiment, eight thicknesses of Turkish towelling were employed, new pads being used for the different solutions. A voltage of 6·3 was employed in each case, the regulating resistance being quickly and evenly reduced so that this voltage was reached in three seconds.

The following solutions were in turn tried under their active poles:—

Distilled water, Oxford tap water, 2 per cent. sodium chloride, 2 per cent. sodium salicylate, 2 per cent. quinine sulphate in 2 per cent. sulphuric acid, 2 per cent. potassium iodide in tap water, 2 per cent. potassium iodide in distilled water. A 2 per cent. solution of sodium chloride was used to moisten the indifferent pad in each case. Records were taken of the milliamperage as soon as the maximum voltage of 6·3 was reached, and again in five, ten, and fifteen minutes.

The first, or three-second reading, did not vary much with the drug employed, usually amounting to three or four m.a. In ten or fifteen minutes the milliamperage obtained with 2 per cent. potassium iodide was about double that obtained with tap-water. With the potassium iodide the skin was more reddened, was more irritated, and more discomfort was experienced by the patient than with tap or distilled water at this given voltage. Quinine sulphate, dissolved in 2 per cent. sulphuric acid, produced still more irritation, redness and discomfort. If the area treated was thoroughly well

warmed by diathermy before the constant current was applied, nearly the maximum milliamperage was at once recorded and varied far less with the solution employed. It would therefore appear that the milliamperage obtained depended more upon the dilatation of the cutaneous vessels and the consequent diminished skin resistance than upon the velocity of the ions with which the pads were moistened: that is to say, the more irritating the polar action of the drug, the greater will be the counter irritation, the greater will be the cutaneous vaso-dilatation and the higher will be the milliamperage obtained. Thus, where superficial pain is under treatment, the best results, at a given voltage, may be obtained by utilizing a solution which causes considerable counter-irritation, such as quinine sulphate in sulphuric acid solution. The increased efficiency which some workers have experienced in the treatment of tic douloureux by moistening the active pad with a solution of quinine may be thus explained. By increasing the voltage, however, we shall obtain with less irritating solutions a similar counter-irritation and vaso-dilatation, and such solutions may be preferable in the treatment of deeper parts, where the inter-polar action of the constant current is desired. It would further appear that the polar action thus obtained is not due to any specific therapeutic action of the solution employed, but is merely the measure of its irritating action. When after a previous heating by diathermy relatively high milliamperage was obtained, it was found, when using quinine solutions, that the voltage of 6·3 had to be reduced on account of the irritation produced. It should be remembered, however, that this irritation is not due to the quinine ions, but to the 2 per cent. solution of sulphuric acid in which the quinine is dissolved. This counter-irritation at the polar surfaces is, perhaps, the most important point, but it is not the only point to be considered.

Considerable heat is necessarily generated when such large currents as those of 100 m.a. are passed through a limb for forty-five minutes or more, as is frequently done in modern electrotherapeutic practice. The fact that this heat is mainly generated in the skin, where the resistance is greatest, does not materially affect the question, for it can be shown by placing a surface thermometer, well removed from the pads, that the whole temperature of the limb is raised two or more degrees. It is quite unnecessary for me to dwell upon the consecutive benefit which must result from the application of such heat throughout a wasted, atrophic sciatic limb with its subnormal temperature. Moreover, the increase of temperature induced by prolonged intensive application of the constant current with its consecutive increased circulation and improved nutrition cannot be overlooked in explaining the benefits which we have all seen after treating such conditions as rheumatic knees in this manner. The unidirectional movement of the ions, moving and colliding with such aggregate force as to produce this amount of heat, must have some effect, though to what extent it is impossible to estimate at present, in accelerating the diffusion through the various membranes in the interpolar path.

To sum up, current electricity, in its slowly interrupted form, exercises a stimulating action on the cells in the path of the current, the excitation of contraction being a manifestation of this stimulation, both this stimulation and contraction being the result of hydrogen and hydroxyl-ion concentration. By greatly increasing the rate of interruption we are able to annul the effects of this hydrogen and hydroxyl-ion concentration, and so are enabled to produce any desired degree of heat in the tissues with a complete absence of chemical stimulation. In addition to this heating effect, the kinetic action which produces it has a valuable influence in accelerating diffusion through the tissue

membranes. The action of the constant current depends upon its caustic or stimulating effects and its reflex irritation at the poles, on the heat it generates, and the diffusion it facilitates in its deeper interpolar path.

Viewing it in this way, we can understand how electrical energy in the form of current electricity acts as a curative agent, and we can both predict in what conditions it will produce beneficial effects, and by what means and by what mechanism these effects are brought about. In such a way, and in such a way only, can we practise our specialty in a scientific manner, in a manner convincing to ourselves and to the medical profession.

If electrotherapy merely consisted in cataloguing the various forms of treatment with the various diseases they are said to cure, such as, for chilblains the faradic current, for rheumatism ionic medication, it would cease to be a scientific and progressive method and could equally well be practised by any masseuse as by the most highly trained medical man. But an empirical procedure such as this cannot be as successful as the scientific procedure, until Nature alters her whole scheme and makes each disease and ailment respectively amenable to one specific method of treatment.

When we turn our attention to the curative action of radiant energy, we are brought face to face with a mass of problems so diverse and so numerous that it is almost impossible to take even a bird's-eye view of them this evening.

As Guilleminot says, nature has provided us with an enormous keyboard of vibrations for curative purposes, extending from the infra-red to the gamma rays of radium.

Provided that we do not imagine that the line of demarcation is a sharp one and that the adjoining divisions do not largely overlap, this keyboard may be divided for practical purposes into radiant heat, light, ultra-violet radiation, X-rays and gamma radiation.

The most discussed portion of the radiant heat and light treatment at the present time is that of heliotherapy. The value of this treatment is admitted by all; every medical man in general practice has seen diseases caused or aggravated by its absence, and cured or relieved by its presence. Its curative action is apparently due to a stimulating action on the skin and its glands brought about by radiant energy; that is to say, the consecutive effects are increased cutaneous circulation, increased cellular activity, increased cutaneous nutrition. The superior results in some cases following heliotherapy would appear to be due to its more powerful actinic action more effectively exciting chemical changes in the cells or tissues by which its radiation is absorbed.

The pigmentation of the skin in Addison's disease, the dermatographic lines so readily induced in Graves' disease, the increased translucency of the skin of tadpoles, when these embryos are fed on pineal gland, all tend to show an intimate relationship between the skin and the endocrine glands. It has, indeed, been suggested that "the pineal influences the body by varying the degree of light reaction" (Berman).

The results obtained from heliotherapy in rickets are very suggestive of an increased liberation of hormones. The reaction of the skin to stimulation by electrical energy calls for further investigation: the skin has been for too long regarded by the medical profession as merely a thing of sores and pimples.

In connexion with ultra-violet rays and X-rays, I should like to emphasize the importance of Kohler's experiment on the gill plate of the salamander larva. Kohler obtained by means of ultra-violet radiation at 2,800 A.U., photographs of the dividing nuclei in the gill plate of the salamander larva, in which the chromatic substance was so opaque as to appear stained, this showing that the

chromatin absorbed the radiation to a very marked degree before the other tissues arrested any appreciable amount. Now, this is a very significant fact when we remember that light and other radiations are only effective when some of their energy is absorbed, and that the cells when their nuclei are in a state of division, that is when the chromatin is most in evidence, are far more receptive of the X-rays. It is, indeed, this selective reception of the cells when undergoing mitosis that enables X-rays to be utilized as a curative agent.

It appears to me that the attention of most radiologists is too much directed at the present time to the desire for more and more powerful radiation. The mere clinical worker would think that, when a radiation of sufficient power and penetration to cause ulceration and perforation of the bladder and bowels had been obtained, enough had been achieved in this direction for the present, and that more attention might now be turned to the far more important question of the fundamental biological action of X-rays, for until this question is settled X-ray treatment must of necessity be largely empirical.

Moreover, in the pursuit of the more penetrating radiation we seem to have reached a deadlock, at any rate as far as the treatment of cancer is concerned. For though the radiations may have sufficient power to destroy cancer cells in one part of the body, if the treatment is applied to more than one region for the destruction of disseminated cells, the patient dies from resulting blood changes. Hence it is clear that the difference between the radio-sensibility of the cancer cells and the blood cells is insufficient to enable a cure for cancer to be found by merely increasing the power of the radiations. We may be able to cure cancer by X-rays, if it is not disseminated, but the mere surgeon can do that.

Possibly further success may be achieved in the radiotherapy of cancer by the discovery of some means of increasing the mitosis of the cancer cells, and hence the radio-sensibility, previous to the administration of the lethal X-ray dose. An increase in the radio-sensibility of the skin can be readily obtained by the previous administration of ultra-violet rays. In treating acne, for example, I found some time ago that, after the application of ultra-violet rays, the application of X-rays in about one-third of a pastille dose produced a definite erythema. Rapp ("Strahlen-therapie," vol. xii) also, though his experience was less fortunate, points out that he has seen burns of the second and third degree due to a combination of ultra-violet rays and X-rays.

No photo-sensitizer or other substance capable of increasing the radio-sensibility of cancer cells has yet been discovered, and apparently the most we can say in this connexion, at present, is, that in X-rays we have an agent capable of destroying cancer cells, but that the technique of their efficient administration in the cure of cancer has yet to be discovered.

It is quite unnecessary for me before such an audience as this to recite in detail the special diseases and conditions in which electrotherapy is indicated; but I should like to direct your attention very briefly to a most important field of research in the application of electrotherapy, to the threshold of which we have as yet barely approached. It is one which I believe is capable of yielding most far-reaching results. I refer to the power which electrotherapy places at our disposal for both stimulating and restraining the action of the endocrine glands. This action can only be fully exercised when we possess a full knowledge of the action of these glands, and in the application of such treatment we need the active co-operation of the biologist and serumtherapist.

In the static charge we have an efficient means of stimulating the adrenal

glands, and their excitation in this way explains the beneficial effects of the static charge in neurasthenia, a form of treatment in this disease strongly advocated years ago by Charcot, and more recently by Benham Snow and other writers. It is of interest to note that an American biologist, referring to neurasthenia, which he terms "the great American disease—dementia Americana"—states that it "seems to be adrenal disease" (Berman).

In diathermy or galvanism we have the means of stimulating the action of such glands as the thyroid, and it is a well established fact that in X-rays we have the means of restraining the action of this gland and other glands.

With such powerful agents at our command, surely it is only a matter of time and research to acquire an increasing control over the many and varied diseases arising from a disordered action of the endocrine glands.

From this very incomplete review of electrical energy as a curative agent, it is evident that much has been accomplished in the past, but that far more remains to be done in the future. We are, indeed, but on the threshold of our science; before us is opening up a vista of possibilities unequalled in any other branch of medicine. Electrotherapy, certainly at the present time, does not meet with its due recognition, but its unexplored field for investigation is so immense that any careful worker may attain in some degree to the true goal and reward of the research student, so well expressed in the lines of Wordsworth:—

> "Enough, if something from our hands have power
> To live, and act, and serve the future hour."

High-frequency Currents in Relation to Cellular Physiology.

By M. PHILIPPSON, M.C. (Brussels).

(ABSTRACT.)

ALTHOUGH the applications of electricity to physiology date from the origin of electrical science, very little is known of the nature of the electrical resistance of living tissues.

It is impossible to measure the electrical resistance of tissues for a continuous current; polarization establishes a counter-electromotive power and the apparent value of resistance grows with the duration of the current.

By the use of alternating current methods one can obtain a definite value for the electrical resistance of tissues. This value is very high and has been ascribed to the resistance of cell membrane.

Before the war Höber, a German physiologist, showed that with the use of high-frequency currents the electrical resistance of living tissues had a very small apparent value which could not be exactly determined.

We have endeavoured to apply the methods of radio-engineering to the study of the electrical resistance of living tissues so as to explore the full range of frequencies between the low frequencies generally employed in physiological work and the high ones attained by Höber.

Our method is the following:—

We use as a generator of alternating current a triode lamp generator similar to the heterodyne employed in radio-technique and constructed so as to give from 500 to 3,000,000 cycles per second.

This generator induces a current of the same frequency in a secondary circuit containing in series the piece of living tissue in a proper cell and a calibrated measuring resistance. When the measuring resistance and the unknown resistance are equal, the potential drop across these resistances is equal also. To ascertain the equality of potential drop we use the Abraham voltmeter amplifier—a triode lamp device which allows of the comparison and eventually of the measurements of electromotive powers of any frequency.

We measured the resistance of different types of living tissues, guinea-pig liver, guinea-pig muscles, potato, &c. Our measurements were always reduced to specific resistance, i.e., resistance of a cube of 1 cm. side.

If we plot resistance against frequency, we always find a similar curve, decreasing first slowly from a high resistance at low frequency, then quickly between 10,000 and 1,000,000 cycles and then approaching a minimum limiting value at higher frequencies.

This curve suggests that in the living tissues there are elements acting as capacities the apparent resistance of which diminishes with frequency.

Let us suppose that the resistance of living tissues is constituted by a capacity resistance (C) shunted by a high ohmic resistance (r), in series with a much smaller ohmic resistance (R).

R represents the electrolytic resistance of the cytoplasm.

C the capacity of the cell membranes.

r the leak through the membranes and between the cells.

The equation of a leaky condenser is expressed as follows:—

$$Z = \frac{r}{1-(j)\,r\,c\,p} = \frac{r}{1+r\,c\,p^2} - (j)\,\frac{r^2\,c\,p}{1+r\,c\,p^2}$$ [1]

in which $\frac{r}{1+r\,c\,p^2}$ is an ohmic resistance and $\frac{r^2\,c\,p}{1+r\,c\,p^2}$ is a capacity resistance. To the first step we must add the ohmic resistance R of the cytoplasm and we obtain for the magnitude or value in ohms of Z:—

$$Z = \sqrt{\left(R + \frac{r}{1+r\,c\,p^2}\right)^2 + \left(\frac{r^2\,c\,p}{1+r\,c\,p^2}\right)^2} = \sqrt{R^2 + \frac{2Rr + r^2}{1+r\,c\,p^2}}$$

The complete range of measurements we have made enables us to find the different constants in this equation:—

When p tends towards infinity Z tends towards R.

When p tends towards zero Z tends towards $R + r$.

C is given by the equation $C = \sqrt{\frac{2Rr + r^2}{Z^2 - R^2}}\,\frac{1}{p\,r}$ It is generally found that C is not a constant but is a function of frequency following the equation:—

$$C = C_1 . n^{-a}$$

C_1 being capacity at frequency one.

If we compare calculated and observed values, we find they agree as shown in following example:

[1] $p = 2\pi n$ where n is the number of cycles of the alternating current.

GUINEA-PIG LIVER

R = 200 ohms. r = 1·780 ohms. C_1 = 5,910^{-7} farads. a = 0·49.

Frequency	= observed resistance	− calculated resistance
500	1,950	1,957
1,000	1,948	1,934
5,000	1,763	1,775
10,000	1,645	1,610
50,000	990	1,036
100,000	780	800
500,000	425	417
1,000,000	325	326
2,000,000	275	270
3,000,000	245	242·5

We can prove in another way that our assumption of a capacity resistance in living tissue is correct. We found by adding an inductance to the circuit that the apparent resistance decreases: this can only happen in the case of a capacity resistance.

For instance, at frequency 100,000 the resistance of a cubic centimetre of a potato was 468 ohms; by adding inductances we reduced it down to a minimum of 288 ohms. This value is greater than the calculated value 243, but we did not measure the high-frequency resistance of our inductance.

We believe that in this way we have ascertained the nature of the resistance of living tissues, and have paved the way to the measurement of the true electrolytic resistance of cytoplasm and to the appreciation through resistance and capacity measurement of the membrane of the value of its physiological integrity.

We found that by allowing excised guinea-pig muscle to rest for an hour before measuring its resistance r dropped from 1,490 to 790 ohms, C_1 rose from $4{\cdot}2.10^{-7}$ farads to $6{\cdot}05.10^{-7}$ farads, R remaining unchanged. This showed the main alterations to be in the cell membranes.

For physiological and medical purposes it was of interest to ascertain if the leak across the membrane lay in the membranes themselves or between the cells. In order to discover this we used as artificial living tissues centrifugalized blood, in which the resistance of intercellular liquid, i.e., blood serum, was known to us. By adding to the blood isotonic sugar solution we were enabled to raise the resistance of this intercellular liquid.

The known quantities in two sets of parallel measurements with and without the addition of sugar allow of the relative volume of serum and corpuscles being ascertained. If the specific resistance of the serum be known, one can find out the conductivity of the serum. The measurement of total resistance at different frequencies gives the total conductivity at those frequencies. If we deduce from the total conductivity the serum conductivity which is constant we obtain the conductivity of the corpuscles and by it the relative value of the current flowing through the cells to the total current.

The following example shows the results:—

Centrifugalized blood yielded a volume of corpuscles of 96·5 per cent., a volume of serum of 3·5 per cent. The specific resistance of the serum was 85 ohms. The conductivity of the serum in the preparation $\frac{0{\cdot}035}{85} = 4{\cdot}1.10^{-4}$.

Frequency	Total resistance in ohms	Total conductivity	Corpuscular conductivity	Percentage of current through the corpuscles
500	2,050	$4{\cdot}9.10^{-4}$	$0{\cdot}8.10^{-4}$	16
20,000	1,954	5·1	1	20
500,000	1,010	9·9	5·8	55
1.000,000	534	18·1	14·7	78
3,000,000	250	40	35·9	90

Very roughly speaking, when the high-frequency resistance is half the low-frequency resistance, a little more than half the current flows through the cells.

This holds good for guinea-pig liver at a frequency of 50,000, for guinea-pig muscle between 20 and 50,000.

These data are of interest in the interpretation of the heating action of high-frequency currency used in therapeutics.

Section of Electro-Therapeutics.

President—Dr. W. J. TURRELL.

The Physiological Effects of Light.

By LEONARD HILL, M.B., F.R.S.

(Director, Department of Applied Physiology, National Institute of Medical Research.)

THE classic sculptures of the Greeks such as the Belvedere Apollo were modelled from men who kept themselves fit by exercise of the nude body. Mr. Bateman gaily satirizes in his comic drawings the feeble ugly bodies of citizens stripped for enlistment, produced by modern life spent in confined smoky atmospheres. Exposure to sun and wind has a wonderfully good effect on health and happiness, as shown by the result obtained in children affected with rickets or tuberculosis through bad feeding and upbringing in close tenements, when received into sanatoria under the care of Bernhardt and Rollier in the Alps, and Gauvain at Alton, and the many followers of these pioneers.

The experience of open-air schools, of which there are some 120 in New York against two or three in London, tells the same tale.

Exposure to cool air increases the evaporation from the respiratory membrane and washing this keeps up immunity to infection. The adding of water vapour to, and warming up of the inhaled air, which is exhaled at almost body temperature, entails a greater blood flow through the respiratory membrane, when the inhaled air is fresh and cool, than when it is warm, stagnant and moist as in crowded rooms. Exposure to cool moving air also puts up the basal metabolism, exciting the body to keep up its temperature by greater heat production, with consequent good effect on the general muscle tone, appetite, digestion and utilization of food, and a deeper breathing and better circulation of the blood.

Argyll Campbell and Leonard Hill have measured the resting metabolism of patients in sanatoria at Alton, Montana, &c., and found exposure to open air puts it up some 50 per cent. in summer and some 100 per cent. in winter above the standard figures of Bennedict obtained in closed rooms. The tone of the muscles and skin of children so exposed in spite of splinting and immobility in bed, is firm, and the children are happy and ready to eat five good meals a day.

The effects of open air already described are very important ones. Turning now to the biological action of light I recall Gauvain's statement that he sees far better results at Alton after a long spell of fine sunny weather. The Alps offer a great climatic advantage in this respect. At Montana there is an average of five hours a day of sunshine in winter against two or less in Britain, and such sunshine, free from mist and smoke is far richer in visible and ultra-violet rays. Cold frosty dry air, warm sunshine, reflection of light from

snow fields, beautiful surroundings taken together afford a wonderful health-giving set of conditions. In England the strip of south-east coast gives us most winter sun and driest climate; on the cliff or shore light is intensified by reflection from the sea. It is here that sanatoria ought to be placed, while branch sanatoria should be run in the Alps by our big cities. Belgium and Geneva each have a sanatorium for the poor in Montana.

It has been shown that rickets is a disease produced by deficient diet and want of exposure to light (Hess and others). Whilst daily exposure to ultra-violet rays can neutralize the effect of a deficient diet so far as stopping rickety changes in the bones; it cannot do much in the way of promoting growth and continuance of life. The addition of cod-liver oil to the diet, on the other hand, effects these three things. The exposure of young rats to a diet deficient in vitamin A and the anti-rachitic substance leads to rickets, xerophthalmia, and infections of the ears, cessation of growth, and death. While exposure of the rats to ultra-violet rays merely stops the rickets, exposure to summer sun and open air in a fine weather climate does more, for it greatly lessens inflammatory infection and prolongs duration of life (Park, Powers and Simmonds). It has been claimed (Hume and Smith) that growth of such rats may be benefited by irradiated air, that is, by air in a chamber first exposed to ultra-violet rays and then withdrawn. This claim has not been confirmed by A. Webster and Leonard Hill.

Wounds treated by exposure to sun and open air do well. Lymph and leucocytes are thus drawn into the wound, the wound cleaned of microbes and the cells stimulated to repair at the same time as the general health is improved by the exposure (Bernhardt). The world will in time come to learn that verandahs are far more important than wards for the treatment of fevers, wounds, rickets, &c., as well as of tuberculosis. As to the question of the biologically active rays it is claimed that infra-red (Wiesner), visible, and ultra-violet are all effective. The infra-red rays are effective as a heating agent; if the heating effect is prevented by a suitable cool irrigation, their effect on living organisms is *nil* (Eidinow and Leonard Hill). The dark heat rays are absorbed by water and so, heating the surface layers of the skin, do not penetrate deeply. The visible rays penetrate to the cutaneous blood-vessels (some may pass through the ear), and being absorbed in the blood locally heat that. Sonne estimates that the blood in the skin may be locally heated to 46·5° C., and that this heating has a powerful effect in promoting the immunizing powers.

A. Campbell and Leonard Hill have confirmed this heating power. The heating is local while the whole body is kept cool by exposure of the shaded parts to the cool open air. Overheating of the whole body by sun exposure in hot, confined places is to be avoided, except in the treatment of rheumatic conditions. The ultra-violet rays have a lethal effect on protoplasm, but very little power of penetration. The shorter the rays the greater their killing power, the less their penetration. Very short ultra-violet rays will not penetrate through more than one microbe—one microbe shields another shaded by it (J. E. Barnard). The middle ultra-violet rays produce erythema and pigmentation by reaching the deeper cells of the epidermis and acting on these; the products of their action raise the hæmo-bactericidal power of the blood in rabbits and pigs greatly, and less, but distinctly so, in man.

The hæmobactericidal power can also be put up by producing considerable erythema and other consequent inflammatory effects by means of exposure to dark or red heat, sufficiently hot to superficially burn the skin. The absorption of destroyed tissue products appears to put up the immunizing

powers however produced. An excessive dose is poisonous, as in extensive burns. Exposure of the whole body to the carbon arc has been found efficacious by Reyn and Sequeira for cases of lupus which defied local Finsen light treatment, also for tubercular laryngitis. The mercury vapour lamp is also used for these purposes. The arc, like the sun, is rich in visible rays and longer ultra-violet rays, which penetrate and warm the cutaneous blood. The mercury vapour lamp is rich in short ultra-violet and poor in visible rays. The shorter rays do not penetrate the skin. The difference of penetration is shown by exposing the frog's tongue suitably kept cool and wet, (1) to the Finsen light (condensed arc), (2) to the mercury vapour lamp. The arc affects the superficial vessels while the mercury vapour lamp produces no reaction in these vessels even after two hours or more. The erythema produced in man's skin by the mercury vapour lamp must result, we think, from damage of the deep epidermic cells and spread of damaged tissue products to the underlying blood-vessels, e.g., similar to the blush following a bee sting or application of mustard. While the ultra-violet light will kill the infusoria kept at cool temperature in a shallow quartz cell very quickly (e.g., five minutes at eight inches distance), it will not kill infusoria put in the aqueous fluid under the cornea of a cat's eye. The cornea and lens are not penetrated by short rays.

Sensitization of tissues by injection of various substances, e.g., eosin hæmatoporphyrin, allows longer (visible) rays to produce an effect which before was not apparent. A very little of the latter substance causes the death of albino guinea-pigs on exposure to light. Meyer Betz thus made himself very ill with œdema of the face and hands and remained sensitive to light for a long time. Hæmatoporphyrinuria and sensitivity to light have been found together in cases of hydroa æstivalis. Buckwheat sensitizes cattle.

Pigment formed in the epidermis, in reponse to the action of the ultra-violet and visible rays, acts as a screen and protects the deeper living structures from excess of light. Melanin is a derivative of the aromatic amino-acids, derived from protein, a product of cell damage. It has been observed to form in fresh preparations of epidermic cells bathed in a solution of diphenylalalanin on exposure to light. It absorbs visible and ultra-violet rays and turning these into heat activates the sweat glands probably through the posterior root nerve plexus. The sweat protects the body from over-heating by reflecting light and evaporating off heat. There is no evidence that pigment is a transformer of visible rays into chemically acting ones. Rollier and Gauvain maintain that those cases which pigment well do well on exposure to the sun and air.

Non-pigmenters may naturally be less immune to infection by tubercle bacilli. Skin tanned and browned by exposure is more resistant to infection, e.g., vaccination, chicken-pox, boils, affect it far less than the white and unexposed parts.

Screens are partial, not absolute, in filtering off light. Thus window-glass filters off rays shorter than 3,300 Angström units, but very thin glass (soap-bubble thick) lets through very short rays. Sunlight in rooms filtered of ultra-violet by glass is of no use in treatment of rickets, &c. Thin open-meshed garments let rays through, thick garments in layers stop all visible and ultra-violet rays. Sun treatment then, can be carried out in a thin bathing costume. Water-coolers for the Finsen arc-light treatment have to be made of quartz which allows rays to pass through down to 2,000 Angström units. The oxygen

in the air absorbs still shorter rays and to study these a hydrogen-filled tube with fluorite ends is used. Hydrogen made to glow in such a tube by passage of an electric current instantly kills and scatters into granules infusoria placed on the fluorite window. These very short rays merge into the longer X-rays. The very long rays, beyond the infra-red used in wireless, have as far as is known no biological action. Some biological interference occurs, according to Pech, Hess, Eidinow and Leonard Hill, when red and yellow rays are added to ultra-violet: a longer exposure is required to produce a given biological effect than when ultra-violet acts alone.

The ultra-violet rays probably act by knocking off electrons from atoms in the molecular structure of the colloidal particles in the living substance, and so altering the electrical charge of these particles that aggregation results. This aggregation may be observed taking place in infusoria and bacteria.

The measurement of dosage of ultra-violet rays can be carried out by means of the bleaching of a standard acetone methylene blue solution in a standard quartz tube. The amount of bleaching is tested against a set of standard glass tubes containing various standard solutions of methylene blue and water.[1]

The acetone-blue method has been standardized biologically against the killing of infusoria in a quartz tube and against the production of erythema in the white skin of the inner surface of the upper arm. The degree of erythema is measured by the Lovibond tintometer. (A. Webster, Leonard Hill and A. Eidinow.)

DISCUSSION.

Professor S. RUSS spoke of the many points which the lecturer's address opened for discussion. He proposed only to refer to one or two of these experimental matters in which he had been engaged. There seemed to be no doubt that sunlight inhibited the growth of many bacteria, but nevertheless, it was, he thought, true that parts of the ultra-violet radiation were much more germicidal than others and that this especially effective region began at a wave-length of 2960 A°. U.

Professor Leonard Hill had referred to some experiments illustrating the fact that the major portion of ultra-violet radiation was absorbed by the cornea. No doubt this was often the case, but he (the speaker) had determined experimentally the degree to which ultra-violet radiation could penetrate the cornea, lens, and humours of the eyes of a variety of animals, and it was of great interest to find that in nocturnal animals considerably more of these rays reached the retina than was the case in man.

Sir HENRY GAUVAIN said that clinicians generally had reason to be grateful to Professor Hill for the brilliant investigations he and his colleagues were engaged upon in the physiological effects of light. Sun-treatment had been spasmodically employed for ages, the value of sunny climates being obvious to the least observant, even though the mode of action of light remained obscure. Only this century had sunlight been used intensively in therapeutics to any considerable extent, and while to clinicians, skilled in its employment, the advantages of this method of treatment were clear, yet in practice sun-treatment had been of necessity empirical owing to lack of accurate knowledge of the manner in which it acted. It had been his (Sir Henry Gauvain's) privilege to introduce intensive sun-treatment into this country, and speedily certain facts became evident as the result of purely clinical observation. It was apparent that certain patients reacted more favourably to this form of treatment than others, and that patients who pigmented well as a rule improved more rapidly than those in whom pigmenting power was defective. While ultra-violet radiation was of importance in the production of pigment, more was required. Equal exposure to a similar source of ultra-violet light in different patients produced differing pigmentary effects, and there

[1] The apparatus is made by Siebe, Gorman and Co., Ltd., 187, Westminster Bridge Road, S.E.1.

was, therefore, some property in the individual himself concerned in the production of pigment. While some had denied that good pigmentation of the skin was of value in treatment, yet this was certain, that good pigmenters could tolerate prolonged exposure to light and extremes of heat and cold much better than non-pigmenters. In a large institution it was obvious that the sum of the progress made was much more rapid when light treatment was available than when it could not be applied. He had first been impressed with this fact in the fine summer of 1911, and the wonderful summer of 1921 entirely confirmed this observation. In surgical tuberculosis a general conclusion could at once be arrived at solely from clinical observation, that sun-treatment had a marked accelerating effect on the cure. Pigmenting power was also of prognostic significance in surgical tuberculosis. Non-pigmenters, whose local lesions healed, were often the subjects of latent tuberculous infection, and in these recurrence was more frequent than in pigmented patients, and indeed might generally be anticipated, perhaps at a distant date. The evolution of a new lesion followed by its cure was not infrequently associated with development of pigmenting power; in its absence later relapse might again be anticipated. To particularize, sun-treatment, properly employed, might be said to have an effect on the mind as well as the body. It was of great psychological value. Like alcohol it would stimulate and enliven, pressed to excess it would intoxicate and depress. Light was the important therapeutic factor, not heat, and for that reason exposure might be more effective at the seaside than inland, on the mountains than in the plains, in temperate zones rather than in the tropics, or even at times under a cloudy sky rather than in direct sunlight.

On the body light might be said to have a local or direct effect, as evidenced by its bactericidal power, aided by the favourable inflammatory response which was elicited by properly graduated exposure, and a remote or indirect effect infinitely more complex, much more important, and one which held out great therapeutic possibilities. The fact that ultra-violet radiation had so little penetrative power had led many to deny this latter action, but recent researches had confirmed and proved these properties, and to these researches the lecturer had contributed investigations of immense importance and significance. Professor Hill had already dealt most lucidly upon many of these remote actions. They opened up a wide horizon of great therapeutic promise. If he might venture a prediction he was of opinion that further research would indicate that light rays of special wave lengths would be even more largely used in the future, either separately or in combination to meet individual needs, and that the successful actinotherapeutist would be he who in the light of increasing knowledge most skillfully utilized these various rays.

Dr. J. H. SEQUEIRA said that Professor Hill had given a lucid description of his work on the biological actions of light. He (Dr. Sequeira) commented on the advantages which accrued from the co-operation of the physiologist, the physicist and the clinician. He said he had been engaged in the treatment of tuberculous affections by the local action of concentrated actinic light by Finsen's method for a very long period, and had found, as had others working in this field, that although about 70 per cent. of the patients suffering from lupus could be cured by the local measure alone, the residue were but little influenced. In a number of the patients the causes of failure were: (1) too rapid an extension of the tuberculous process for it to be coped with by a tedious and slow local procedure; (2) extensive disease of the mucous cavities, particularly of the nose, which could not be reached by the light nor adequately treated by surgical measures. It was in these types that the greatest benefit had followed the use of the general electric light bath, and a combination of local light treatment with the light bath had raised the proportion of cures to 90 per cent.

It was not yet certain which was the best form of illuminant, but experience at the Finsen Light Institute at Copenhagen tended to show that the carbon arc gave the best results. At the London Hospital he (Dr. Sequeira) had been using with great advantage carbons with a core of tungsten paste, which gave a spectrum very closely approximating that of the sun. When treating a number of patients the large carbon arc lamp was most convenient, but there were many advantages, especially with smaller installations, in the mercury-vapour lamp.

Dr. Hill had called attention to the different modes of action at the two ends of the spectrum, and he (Dr. Sequeira) was reminded of the therapeutic use of a Neon lamp giving a red light, which recalled Finsen's treatment of small-pox by the exclusion of all but red light from the patient. Finsen held that if the patient were as carefully protected from actinic light as a photographic plate, the eruption aborted, the secondary fever was abolished and scarring diminished. This, of course, was negative phototherapy, but it appeared possible that the red rays themselves might be the therapeutic agent.

Dr. MURRAY LEVICK said that during the past three years he had been giving experimental treatment by means of artificial light at St. Thomas's Hospital. He decided to begin with up-to-violet rays only, with a view to determining the share of these rays in the therapeutic value of sunlight

The apparatus consisted of a 1,000-watt filament lamp with gas-filled globe on a 240 volt circuit (D.C.) with a nickel reflector. The patients reclined on a couch, exposed to the rays of the lamp and to wind from an electric fan, care being taken that no area of skin was exposed to the wind that was not also exposed to the rays from the lamp. The effect of the wind was to prevent the skin from becoming overheated and promptly to remove the sweat.

The patients came from the Children's Department, and he (Dr. Levick) worked in collaboration with Dr. Jewesbury and with Dr. Gardiner Hill, who worked out the basal metabolism of the patients. The first cases selected for treatment were those of children suffering from debility, with anorexia, listlessness, general malnutrition and fretfulness but without any visible cause for this state. In short, a type of child very common in the out-patient departments in large towns. The treatment was administered for three-quarters of an hour on five days a week for one month, i.e., twenty treatments. The result was a complete transformation in the condition of each child. This was evident in some cases long before the conclusion of the treatment. The children slept well and their appetite, cheerfulness and desire to play all returned. The most remarkable sign observed was the great improvement in the condition of the skin, which exhibited remarkable capillary development and took on a consequent coloration although in most cases it was of a waxy whiteness at the beginning. The musculature shared in the general improvement—a greater firmness and fullness being evident at the conclusion of treatment, and in this respect it seemed well to bear in mind that the improvement in musculature exhibited by patients undergoing heliotherapy at Leysin was ascribed by Dr. Rollier to the infra-red rays which it was known penetrated deeply into the tissues.

He (Dr. Levick) considered these results of great importance and interest because up to this stage of the experiments practically no ultra-violet rays had been used. These latter rays were now added to the treatment by the use of a mercury vapour lamp in combination with that already described. The results of this combination were decidedly better and truly remarkable. He showed on the screen skiagrams of the arm and hand of a child suffering from acute rickets, one taken before and one after a course of nineteen treatments spread over a period of one month. The first photograph showed the lower ends of the long bones with blurred and irregular outline from deficient calcification, and the carpal epiphyses indistinct. The second, taken after the nineteen treatments, showed the perfect outline of normal bone and healthy active epiphyses.

A child fed on vitamin-free diet recovered just as quickly as specially fed children when undergoing treatment. It was interesting to note that a little negro boy made very tardy progress when treated alongside a white boy who made a rapid recovery.

Some cases of tuberculous peritonitis and tuberculous glands, and of osteo-arthritis had shown very good results. He (Dr. Levick) said that he had been particularly impressed by noticing that children suffering from acute rickets exhibited the symptoms of acute neurasthenia and that these symptoms were among the first to clear up under his artificial light treatment. Acting on this observation he had already treated some cases of neurasthenia in adults with great success and was inclined to regard neurasthenia in the adult as a manifestation of light starvation. Light starvation was now

concluded to be the cause of rickets, in which case he had found the real cure for this condition.

With regard to the choice between the carbon arc lamp and the quartz mercury vapour lamp plus the combination he had described, he had used both sources extensively and now unhesitatingly chose the latter on account of its more rapid effect and greater cheapness in the long run.

The irritating far-ultra-violet rays, so disliked by those who used light for the treatment of skin diseases, he regarded as a great advantage when heliotherapy was used for its distant effect on the body, because they rapidly brought into the skin an excess of blood which was then exposed to radiation.

The treatment with these powerful lamps had to be given with care and discretion, and it was much to be desired that this form of treatment should not be allowed to fall into the hands of laymen and laywomen, as had electro-therapy, so as to lead to disrepute of the method and its lasting retardation.

Dr. C. B. HEALD showed an interesting case of a patient whose back had been exposed on two occasions in August, 1923, to the light of a mercury vapour lamp, with a Chance's luminous light-ray filter interposed. The back had been previously smeared with ordinary vaseline except for a small central area. The area over which the vaseline had been spread appeared to have been definitely bleached even in comparison with the palest portions of the body; and more distinctly so in comparison with the areas that had received the direct filtered rays, which showed the ordinary pigmentation. During the exposures the vaseline became a brilliant fluorescent green, and he (Dr. Heald) wondered what was the cause of that reaction.

Section of Electro-Therapeutics.

President—Dr. W. J. Turrell.

DISCUSSION ON THE CLINICAL RESULTS OF DEEP X-RAY THERAPY.[1]

Dr. William Mitchell (Bradford),

in opening the discussion, explained that his remarks would be based on twenty years of experience in radiotherapy at Bradford, and that his technique was that recently described by Wintz in his book. As a result of much unwise comment in the press, a much greater demand for X-ray treatment had been made than was justifiable, many false hopes were raised, and those who had been feeling their way on the subject had found their hands forced, for it came to be regarded as almost a crime to refuse to treat a case in this way, however hopeless it might be judged. Of 133 malignant growths treated in the first year 30 were carcinoma of the breast, 28 epithelioma of jaws, tongue, and larynx, 1 of forehead, 8 cancer of the pelvic colon and rectum, 7 of cervical glands, 2 of superior maxilla, 4 sarcomata, 2 carcinoma of the body of the uterus, 20 of the cervix uteri, 2 of the pancreas, 2 sarcoma of skull, 1 chondro-sarcoma of pelvis, 1 epithelioma of ear, 2 carcinoma of kidney. Stomach, bladder, and rectal cases, 1 osteo-sarcoma of clavicle, and 1 myeloid sarcoma of the head of the femur, sarcoma of buttock and lympho-sarcoma of neck, Hodgkin's disease, uterine fibroids, carcinoma of thyroid, splenic leukæmia, and some minor conditions were also treated. Forty-eight of the 133 cases were hopeless before treatment was commenced, and 46 of the cases were post-operative recurrences. At the end of a year 41 had ceased attending before completing their course, many because their doctors advised them that the treatment was probably doing them more harm than good, some because they felt the treatment upsetting and trying. Of two cases of cancer of the cervix uteri, both of which he (Dr. Mitchell) at one time thought were cured, one had a recurrence and refused to have further treatment. In two cases of carcinoma of the pelvic colon in which colotomy had been performed, the result of the treatment seemed good, and one patient was still living, though with a fæcal fistula. One case of carcinoma of the bladder, in which that viscus had been opened and cauterized, he had treated six times with a 100 per cent. dose, at the end of which course the man was in good condition, nothing could be felt per rectum, and urine was passed easily. After each treatment the urine remained clear for three months, but then blood reappeared in it. Four breast cases appeared to do very well. A case of lympho-sarcoma of the neck at one time seemed cured, but there was a recurrence, and death took place from

[1] For the account of this Meeting the Section is indebted to the Editor of *The Lancet*.

pulmonary metastasis. A myeloid sarcoma of the scapula, in a girl aged 6, was cured by the treatment; he exhibited skiagrams showing the regeneration of bone which had taken place. Tuberculous dactylitis did well, as indeed it did in former days, with the old treatment. It was, he thought, impossible to promise cure of malignant disease by X-ray therapy, chiefly because metastasis was an unknown quantity before treatment was commenced. If a case were treated while in a cachectic condition, the patient was probably made worse than before. The most favourable cases were those with a good blood count and high hæmoglobin index. There was a difficulty in regard to beds in hospitals for the treatment of these cases, hence in too many cases these patients, after each séance, had to return to their squalid and insanitary homes, a fact which seriously militated against good results. Practically all cases, excepting the cachectic patients, received temporary benefit from the treatment, and the patients expressed themselves as much fitter in general health, and freer from pain.

Mr. Sampson Handley

said the time had passed when surgery alone could claim to deal with malignant disease; in nearly all cases the help of the radiologist was necessary. Yet he was not clear that what was known as deep X-ray therapy was the sort of help the surgeon would ask. The effects of deep X-rays on patients with malignant disease should be placed under two headings: (1) the effects on the patient; (2) the effect on the growth. The former still stood in need of investigations by the biochemist, and those made at Middlesex Hospital by Dr. E. C. Dodds and Dr. J. H. D. Webster[1] deserved careful consideration. The former had thoroughly investigated a few cases, rather than more cursorily examining a large number—an intensive method of study which the speaker commended. These observers had found that whilst irradiation of the head neck, and thorax had little effect on the urine, irradiation of the abdomen profoundly modified the urine, producing an immediate large drop in its amount and a variation in its essential constituents. They attributed this to a temporary inhibition of the functions of the principal abdominal glands, such as liver, pancreas, and kidneys. When the abdomen was irradiated there was a marked fall in the blood-urea content. Patients subjected to heavy X-ray doses, especially applied abdominally, suffered from profound asthenia and loss of vitality, i.e., to a degree greater than could be accounted for by the disease for which they were treated; in this X-rays afforded a striking contrast to the results of treatment by radium, which appeared to have a stimulating action, giving the patient a sense of increased well-being. Even sufferers from inoperable malignant disease had told him, when being treated by radium, that they had never felt so well in their lives. The effects of deep X-ray therapy in amounts necessary for the surface treatment of deep-seated tumours were so marked that in such amounts the use of the rays was undesirable and deleterious. As to the effects of irradiation on the tumour itself, in certain cases there was undoubted benefit, while in others actual stimulation of the growth took place. Mr. Handley proceeded to describe some cases. In the case of three patients who were subjected to deep X-ray therapy, within a short time of the commencement of the treatment, new superficial metastases developed within the area treated. One was a case of breast carcinoma with recurrent nodules. Deep therapy had been carried out for the nodules, but fresh nodules appeared

[1] *Lancet*, March 15, 1924, p. 533.

there. Another was a carcinoma of the neck, with a recurrent mass just below the jaw secondary to carcinoma of the larynx, and shortly after deep therapy had been applied to the neck, the scar was a mass of secondary nodules along its length. In the third case the speaker excised part of the stomach for gastric carcinoma. Deep therapy was applied prophylactically, because he felt that his operation must have been an incomplete one. Within two months of the treatment having been commenced, masses could be felt deep down in the abdomen. In one case of retroperitoneal carcinoma, the primary growth probably being in the pancreas, for a mass could be felt there, enlarged glands existed in the right iliac fossa. That was in September, 1922. He thought the patient's life would be about three months. Dr. Douglas Webster applied deep therapy, after which the glands in the right iliac fossa disappeared, the patient seemed much better, and began to get about again. But the mass above began to grow again, and death took place two months ago. In that case he believed the deep therapy prolonged life about a year. For a man with carcinoma of the prostate, Mr. Handley buried radium into the lateral aspect of the prostate through punctures in the perineum. Improvement followed this, but he still had a mass in the right vesico-sacral fold, palpable per rectum, and he developed some glands in the right iliac fossa. After this one radium application, Dr. Webster treated the patient by deep X-ray therapy, having begun it in February, 1922. If the case had been left the patient would probably have been dead by now, but he was still living on, all the glands in the left iliac fossa had disappeared, and the former œdema of the left leg had now gone. An opinion as to the efficacy of the treatment was only possible after a long period of observation of cases. The technique was still fluid. His general view was that a medium must be observed between an X-ray dosage which was inadequate to act on the growth and that which was harmful to the patient, and he did not think that that balance had yet been effectively struck. The use of buried radium, in suitable cases where the area to be dealt with was small, possessed great advantages over deep X-ray therapy, in that, with the radium, application of a maximum amount of irradiation of the growth could co-exist with a minimal irradiation of the tissues generally. In deep X-ray therapy, however, irradiation of the tissues generally was hard to avoid, and was usually deleterious in its effects.

Dr. G. Cooper (Leeds)

speaking from an experience of 400 cases of all varieties of malignancy, said that in most of the cases in which X-ray therapy was applied there was a marked improvement in general health and an increase in weight shortly after the commencement of the treatment; it seemed as if the growth had been temporarily checked in the intensity of its malignancy. *Pari passu* with this there seemed to be a staying of toxic action. A decline in general health seemed to be associated with a return of activity of the growth. There was in many cases a cessation of discharge and relief of pain following the treatment, and, particularly, when hard rays were used, a formation of fibrous tissue, the latter being probably an important factor in limiting the spread of the disease. Healing of rodent ulcer coincided with the formation of a dense layer of scar tissue, which cut off the normal from the malignant cells. Among the factors influencing the degree of response to the treatment were the amount of metastasis, the situation of the growth, the condition of the patient, especially the blood condition, the nature of the neoplasm, and the amount of irradiation

administered. His best results had been obtained in genito-urinary conditions and pelvic conditions generally, but cancer of the cervix had shown a tendency to relapse, after the first improvement which practically always took place. By means of this therapy many cases adjudged inoperable had been rendered operable. There were also encouraging results in cancer of the rectum; pain was controlled, and the size of the growth reduced. Except in one case (in which the radiation was given at the time of operation) he had found the treatment in cancer of the stomach very unsatisfactory; nor had there been a favourable response in cancer of the tongue. A much more favourable report could be made concerning the treatment of primary glandular enlargements. In cancer of the parotid and thyroid glands, he had had good results at first, but nearly every such case subsequently developed metastases. The urgent dyspnœa of malignant disease of the mediastinum was often relieved by the treatment, but relapse occurred in three or four months. In spleno-medullary leucocythæmia an enormous reduction in the size of the spleen was effected in two or three weeks. Since the introduction of deep therapy more advanced and definitely hopeless cases were brought for treatment; it offered the only treatment likely to bring relief, and it could be said that more had been done for malignant disease by it than had hitherto been achieved outside the domain of surgery.

Dr. Robert Knox

reminded the meeting that deep X-ray therapy was not by any means a new departure, and it did not originate at Erlangen; in this country it had been steadily developed along sound lines. It was assumed that a particular lesion of the skin stood a certain dosage, and that if there were a deeper-seated lesion it would respond to a similar dose. Deep therapy was based upon that; it was a logical extension of surface therapy, and was a question of wave-length. The points he wished to make were largely based upon Mr. Sampson Handley's work on the natural process of cure in cancer embodied in his book on cancer of the breast. It was necessary to supplement nature's efforts at cure; instead of depressing the tissues, efforts should be made to enable them to overcome the disease. In many cases that was what the radiologist did. In treating blood conditions in this way it was found that the blood cells responded in various ways to irradiation, and one of the lines of inquiry should be concerning the alterations in blood, blood serum, and the various biochemical processes. The work of Dodds and Webster was very fine in this respect, and if pursued would take the profession a long way in a rational therapy, and probably standardize the technique. The response of enlarged lymphatic glands to irradiation he had found to be remarkably rapid in the inflammatory type; not so rapid in lymphadenoma, less rapid still in lympho-sarcoma (with a greater tendency to recurrence). In sarcoma the reaction was not so certain, and in carcinoma there was an uncertain reaction. Dr. Knox then referred in detail to a number of cases and projected slides of them on to the screen. One of the most remarkable was that of a man who six years ago had a testicle removed for a carcinomatous condition. He had fairly frequent doses of X-rays for two years. He then came with a large mass in his abdomen, which was regarded as malignant. A fairly heavy dosage was applied, and it disappeared in a remarkable manner. Early last year he came with a mass of glands in the supra-clavicular region, and had pleural effusion at the left base and a very large mediastinal tumour. Penetrating rays in considerable dosage were

administered, and the left pleural effusion cleared up. The tumour was very much reduced in size, but there was later effusion at the right base. In the course of time the right-sided effusion cleared up, and at present he was carrying on his duties. Dr. Knox expressed himself as a supporter of the idea that rays of lesser potency than those directed at the original growth had a stimulant action on secondaries which such rays reached, and he quoted a case supporting that view.

Dr. Reginald Morton

recorded some general impressions which he had formed as a result of his clinical experience. He said that the great advantage of this modern development of X-ray treatment was that it enabled one to treat centrally situated lesions with greater efficiency than superficial ones. As to the relative advantages of single and of multiple sittings, there was much to be said on both sides. The idea of the single full dose was to avoid stimulating a growth to increased activity; now and again there were cases the increased activity of which could not be accounted for on any other supposition. These, however, were but a very small percentage of the whole. There was a smaller margin of error in dosage in the administration of a single dose. Even in deplorable cases giving but little hope, if X-ray treatment were clamoured for it should be given. He did not think the percentage of successes from X-ray treatment by modern methods was greater than formerly since in this country it was still the custom to send to the radiologist only the practically incurable cases. Hard tubes and heavy filtrations formed a homogeneous beam of rays, but they did not appear to have any more specific action on living cells. A series of cases of rodent ulcer which he had treated by the Erlangen method gave no better results than a similar series dealt with by the older technique. Some of his best results had been in pelvic conditions. Cases involving the air passages and the neck generally were unfavourable: cartilage did not respond well to the rays. Some cases of cancer of the rectum responded very well, and prostatic involvement yielded successful results in a high percentage, especially simple enlargement of that gland as distinct from malignant conditions. In none of his cases had irradiation brought about any serious fall in essential blood cells, and he thought too much stress was usually laid on this danger. In this connexion, length of exposure was an important factor.

Dr. Curtis Webb

mentioned the favourable results he had secured by the Erlangen method during eighteen months, and related a number of cases. He favoured at present the single massive dose, in contrast to the divided dosage as mostly practised in France, chiefly because under the latter one could not be certain that the full intensity of irradiation reached the deeply situated disease.

Dr. N. S. Finzi

spoke of the good results achieved by deep X-ray therapy a number of years ago, and instanced a case of his own, that of a boy with proved retroperitoneal lympho-sarcoma, whom he treated in 1911 with hard rays filtered through 2 mm. aluminium. He got well and was seen in good health last year. He was decidedly of the opinion that modern methods were superior to the old,

and that they gave better results. The feeling was too prevalent that either X-rays only or radium only must be used; often the best results were secured by a combination of the two. The primary growth was often best treated by radium, while the region permeated by the malignant cells might be irradiated more effectually by X-rays. Splitting the radium into numerous foci was valuable. Radium therapy was advancing in parallel with X-rays. He agreed that the changes produced in the blood by radio-therapy had been exaggerated. Septic cases did not do so well as others, under treatment by either method. When breast cases were complicated by mediastinal involvement he regarded the outlook as bad. Giving the patient chloretone before the irradiation helped to ward off sickness.

Dr. Louisa Martindale

related her experiences based upon 160 cases. She had seen very little sickness, a fact she ascribed largely to her preparation of the patient—i.e., as for an abdominal operation. In 100 female castration cases, sickness occurred only in twelve. Severe menorrhagia yielded well to X-rays.

Dr. J. H. D. Webster

advocated a close study of pathological processes by radiologists. In some types of cases the best prospect from X-ray and radium treatment was afforded by the production of a so-called caustic effect on the malignant cells, and this was effectively brought about by multiple small radium tubes. In other classes of case the right course was to try to restrain the growth by building up the patient's health and powers of resistance. He agreed that small doses of the rays might result in a stimulation of the new growth. There was always the danger that the area harbouring minute seeds of the disease was not coming under the influence of the treatment. More investigation should follow the important discovery that irradiation caused the disappearance of certain types of rare blood cells.

Dr. W. J. Turrell (President)

said he thought a third treatment which was worthy of consideration was diathermy. He did not support the treatment by deep X-rays of fibroids at the menopause, reminding members of the importance of the endocrine functions of the sexual organs, which were not yet fully understood. Small repeated doses seemed to be the best course, as being calculated to injure the endocrine activities as little as possible.

Section of Electro-Therapeutics.

President—Dr. W. J. Turrell.

The Teaching of Normal Radiology.

By R. W. A. Salmond, M.D.

(ABSTRACT.)

The writer drew attention to the great lack of real teaching in normal radiology except at one or two centres, and pointed out that the logical time when it should be taught is when students are doing anatomy and physiology.

He next described the various rooms of the X-ray department in the Institute of Anatomy at University College, and their relation with the other departments of the Institute, the Medical School and the Hospital. The staff and the work already going on were also described. The work of the department was discussed under three headings:—

(1) As a reference for normal radiology for students while they are dissecting.

(2) Systematic teaching both by lectures in the theatre and screen demonstrations in the radiographic room.

(3) Research.

Dr. Salmond touched on the very wide field for research, mentioning in particular developmental and fœtal radiology and also comparative radiological anatomy and pathology in conjunction with animals from the Zoological Society's Gardens.

DISCUSSION.

A discussion followed, in which Dr. Finzi drew attention to the work which was being done in Manchester by Professor J. S. B. Stopford and Dr. Woodburn Morison.

Dr. Morison briefly outlined the Manchester scheme, and thought that full credit for the introduction of radiology into the teaching of anatomy should be given to Professor Stopford, who three years ago had equipped an X-ray department in connexion with the anatomy department of Manchester University. It was of use not only in the teaching of anatomy but also in research work. Dr. Morison also showed a number of lantern slides illustrating the research which was going on in the X-ray department.

Electro-therapeutic Methods in the Treatment of Fractures.

By C. B. Alexander, M.R.C.S.Eng., L.R.C.P.Lond.

In considering the subject of this paper to-night, I hope you will forgive me if I chiefly refer to the methods we use in the treatment of fractures at the Orthopædic Department to which I am attached in Liverpool.

The correct time for application and therefore the efficiency of physiotherapy in these cases depends to a large extent on the means employed in the fixation of the broken bone. In my opinion it is unwise to attempt to lay down any hard and fast rule about the early or delayed use of massage and electrical

treatment when we remember how rarely two apparently similar fractures run the same course or require exactly the same treatment. The successful management of any fracture depends, before everything else, on careful and constant supervision by the surgeon, aided by frequent radiographs.

One is only asking for trouble, for instance, if after reducing and splinting a Colles's fracture the patient be casually instructed to return in a week Anything may happen in that time, and the chances of a good functional result may be ruined. If one had an unlimited staff of specially trained masseuses at disposal, I think that the immediate employment of physio-therapy would be distinctly indicated in the majority of cases, but this is unfortunately not possible, and therefore it is my invariable rule to see even the most simple and straightforward cases on the first, third and seventh day after splints are applied. Quite frequently some slight alteration or adjustment is found necessary. At the end of seven days, weekly inspection is generally all that is required. I emphasize the importance of supervision in the early stages, for by this only can those fractures which chiefly need the early use of physio-therapy be selected.

As I have already said, one cannot be dogmatic, and in a sense every fracture met with is a problem of its own; but I think most will agree that in any case what may be termed local treatment in fractures of the shafts of long bones, is, for various reasons, contra-indicated for the first three weeks. On the other hand, physiotherapy can frequently be employed with advantage within the first few days, if the injury is in the vicinity of a joint, particularly the shoulder or wrist.

It is impossible to-night to deal in detail with every type of fracture met with, so I intend to refer to a few of the commonest, and indicate the methods I adopt.

Colles's Fracture.

This is the commonest of all. If at the end of the third day after reduction and the application of splints, or in those cases which do not require reduction, but merely the application of splints, any of the following conditions are present, I generally obtain the aid of a specially trained masseuse, who acts under my instructions and, at first at any rate, under my supervision.

(1) *Excessive œdema of the hand and increasing pain.*—We put up our Colles's fractures in the Orthopædic Department of the Liverpool Royal Infirmary in two metal gutter splints, one running from the external epicondyle of the humerus to the heads of the metacarpals, and the other from the internal epicondyle to the level of the thenar eminence. Pads are applied if necessary, and each splint is slightly curved on its long axis, so that the distal ends tend to approximate about the level of the radial styloid. If, then, œdema of the hand and increasing pain supervene, the posterior splint is removed daily, and while the injured limb rests on the anterior splint, effleurage is given to the back of the hand and forearm, and sometimes very gentle active movements of the fingers encouraged afterwards. We do not employ faradic stimulation at this stage, and if there has been much comminution, or if there is any doubt about complete reduction, do not even allow active movements.

(2) *Pain or swelling in the elbow and shoulder.*—The articular surfaces of the elbow and shoulder-joints are frequently bruised when the accident, which results in a Colles's fracture, occurs, and sometimes pain and swelling are quite early symptoms. The elbow is difficult to treat without removal of splints, but the shoulder, which is merely supported in a triangular bandage, is easily accessible. I find massage and faradic stimulation most helpful in these cases The faradic current, which should be only sufficient to cause a slight contraction

is applied to all the muscles about the shoulder-joint and is best given by a coil of the Smart-Bristow type.

(3) *Fracture in that type of patient who knows best how to treat his or her own injury*, included in those who appear at the out-patient clinic bearing a newspaper parcel containing the splints which one so carefully applied a few days before. She—for I regret the patient is almost invariably a woman—will tell you that she thought her wrist was all right because the pain was so much better. After seeing a few hundred cases one is able to recognize the type quite readily, and the reason for selecting them before others for early massage, &c., is again *supervision*, and to remind them daily that they have a serious injury. Often, of course, the patients return in a very chastened spirit because their arm is much worse as a result of their efforts, than it was directly after the accident. On the other hand, they may return merely to bring the accusation of not having been properly treated in the first instance.

(4) *Fracture in aged people.*—As a rule, these patients do not readily regain function in an injured joint by their own effort after that joint has been kept at rest for any length of time. Therefore, they need constant encouragement.

These I consider, when the staff is limited, are some of the indications for what is practically the immediate employment of physiotherapy in Colles's fracture.

When, in other cases, at the end of seven or fourteen days, the splints are removed for examination and the case is found to be proceeding normally, these are re-applied and the patient returns again in a week. If, however, at this period, in a case which has apparently been proceeding normally, there is not a satisfactory range of active or painless passive movement at the wrist, electrical treatment should be employed to deal with the situation. The condition is most frequently seen in women, of the ashen-grey, toxæmic looking, arthritic type, and it tends later to develop into what is termed traumatic arthritis. Because the wrist is at rest in splints, but the arm is being moved more each day, the patient may first complain of pain in the shoulder.

In my experience diathermy is of the greatest help in these cases. If the trouble is confined to the wrist, it is applied through and through, one electrode being placed on the anterior and one on the posterior aspect of the joint, but if the shoulder also is involved—and it generally is—I give it by the longitudinal method from wrist to wrist. Treatment should be given daily and for at least thirty-five to forty minutes. It should be followed by active movements and gentle faradic stimulation of the muscles about the affected joint.

In recent cases relief following the application of diathermy is almost immediate, but in old-standing cases the patient will often complain of severe aching pain in the wrist or shoulder during treatment. This pain is due to the increased flow of blood following the heating up of the affected part and it can be overcome by passing a smaller current without however reducing the length of the sitting. From day to day it is generally possible to increase the amperage, and the shorter the time elapsing before the patient is able to take the maximum dose without pain, the better the prognosis.

If diathermy is not available, ionization or radiant heat may be employed, although personally I have not been impressed with the results obtained from either of these. Of the two I think ionization is the better, but treatment should be given for at least forty to fifty minutes.

It must not of course be forgotten that with any arthritis, however mild, there is always a tendency to the formation of peri-articular adhesions. These are best broken down at once under gas or ether, care being taken that during the operation the opposing joint surfaces are not further injured; it is only

necessary to move the limb once through its full normal range to obtain the desired result.

Fracture of the Surgical Neck of the Humerus.

What has been said about Colles's fracture applies equally to fracture in the region of most joints. It is our practice, for example, in fracture of the surgical neck of the humerus, to fix the arm to the side—after reduction if required—with a big pad of wool in the axilla. Daily gentle faradic stimulation of the muscles about the joint, particularly the external rotators, is then employed and continued for three weeks, when an abduction splint is applied holding the arm at about 90° abduction with the forearm raised slightly above the level of the shoulder. Fairly strong faradic stimulation may be given at this stage with longitudinal or transverse diathermy, if pain is a prominent symptom. After three weeks, in addition to having electrical treatment, the patient, still wearing the splint, is instructed in active movements to promote external rotation and abduction at the shoulder.

Fracture of the Neck of the Femur.

In fracture of the neck of the femur treated in an abduction frame, or fracture of the shaft of this bone treated in a Thomas bed-knee splint, faradic stimulation of the quadriceps is most urgent as early as possible. In either case there is a tendency for the muscle to shorten, due to the formation of a certain amount of fibrous tissue following disuse atrophy, with resulting limitation of flexion at the knee. The faradic current maintains muscular tone, upon which depends extensibility as well as contractility, and in addition the formation of intra-muscular and peri-articular adhesions is prevented.

Any likelihood of a stiff ankle or stiff toes is equally easily prevented by graduated contraction and active movements.

Fracture of the Scaphoid.

In that fairly common injury to the carpus—fracture of the scaphoid—two varieties are met with—(*a*) the transverse fracture, in which generally only fibrous union occurs; and (*b*) the comminuted fracture which, if neglected, is frequently followed by arthritis. The transverse fracture is best treated in a short cock-up splint and almost immediate massage with faradic stimulation of the flexors and extensors, particularly the latter, about the joint. In the comminuted fracture removal of the bone by operation, followed by similar treatment, produces the same result. In either I have given transverse diathermy and found it most effective.

Fracture of Long Bones.

These few examples will serve to show the type of case in which we usually employ electro-therapy. Reference, however, should be made to the early treatment of fracture of long bones. There is no doubt that when a limb is confined in splints for any length of time atrophy of bone, as well as of muscle, occurs and can be easily demonstrated by X-ray examination. Acute atrophy of bone means delayed union. Any method, therefore, which can be used to prevent such atrophy will be followed by earlier restoration of function, always providing that the immobilization of the broken bones is not interfered with.

In a most interesting paper, which appeared in the *Archives of Radiology and Electro-therapy*, for December, 1918, Dr. Barclay advocated the use of the sinusoidal current, and pointed out that risk of movement at the site of fracture might be avoided by the use of special splints in which electrodes were incorporated. The sinusoidal current, it is claimed, acts by increasing

the blood-supply to the part, preventing muscle and bone atrophy. If the periodicity of the current is slow, about 500 per minute (8·3 per second), a slight fibrillary contraction occurs in the muscle fibres, without any gross movement in the muscle itself which would be likely to disturb the ends of the fractured bone.

Lately I have been employing diathermy in delayed union or ununited fracture of long bones. The method I adopt is first of all to heat the area about the fracture by giving the current transversely for about ten miuutes. The apparatus is then switched off for about one minute, when it is turned on again for about two minutes. I continue these periods of flow and no-flow for about half an hour. The effect one attempts to attain practically amounts to a "contrast bath" for the deep tissues, and what we particularly aim at is the vascularization of the condensed or condensing fibrous tissue between the ununited ends of the fracture in the hope that bone formation will be stimulated. I am controlling this form of treatment with frequent radiographs, but at present have not been able to collect a sufficiently large number of cases to form a definite opinion as to its efficacy.

Injuries to Peripheral Nerves.

It is often necessary to treat, as a complication of any fracture, the condition resulting from injury to some peripheral nerve or other. The musculo-spiral nerve is the one usually involved, and after this, in order of frequency, the external popliteal, ulnar and median nerves.

Sherren quotes Bruns, who found that out of 189 cases of nerve injury in simple fractures seventy-seven were musculo-spiral, twenty-five external popliteal, nineteen ulnar and seventeen median.

The lesion is generally incomplete and electrical treatment is usefully employed in dealing with both sensory and motor changes which follow. When the skin area supplied by the affected nerve is hypersensitive I have found the sinusoidal current most helpful and prefer it, when possible, with the limb in a bath.

Paresis, or paralysis, of the muscles involved requires daily electrical stimulation, massage and re-education. The patient's electrical reactions determine the type of current to be used. If faradism is present then the faradic current, preferably from a coil of the Smart-Bristow type, is the best to employ, but if the muscles respond only to galvanism daily interrupted galvanism should be given. During treatment each muscle should be picked out and stimulated separately. It is important not to cause fatigue—an overworked muscle atrophies, it does not develop. Six to ten contractions at a time, between which care should be taken to obtain complete relaxation, are quite sufficient. No benefit is derived, but actual damage is often done, if the affected limb is placed in a bath and an interrupted current passed. The muscles which most readily respond are naturally those which are unaffected and often, by their contraction, only stretch their opponents requiring treatment.

I remember, during the late war, seeing a soldier with musculo-spiral paralysis receiving interrupted galvanism. His arm was immersed in a bath and an indifferent electrode was placed over the brachial plexus. Every time the current flowed the flexors of the wrist and fingers were thrown into contraction while the paralysed extensors were stretched. Such treatment, after which a cock-up splint was solemnly re-applied, was of course doing very much more harm than good.

Summary.

(1) Constant supervision and proper splinting are of the first importance in the treatment of any fracture.

(2) Early application of physiotherapy is most useful in recent cases, if the immobilization of the broken bone is not interfered with.

(3) At a later stage faradism should be applied to help to restore function, and diathermy to alleviate pain if necessary.

(4) In delayed union diathermy and the sinusoidal current tend to help bone formation by increasing the blood-supply to the part.

(5) Daily faradism or interrupted galvanism is necessary in any nerve injury complicating fracture.

In conclusion I would draw attention to an important point which is often forgotten in the running of any electrical department. Efficiency cannot be judged by the number of cases treated per diem, but only by results. The border line between effective and non-effective physiotherapy is a narrow one. Treatments must be correctly and conscientiously given, and never rushed, with the idea of showing a good statistical return at the end of the year. The skilful application of electrotherapy and massage is an essential in orthopædic surgery to-day, and if carelessly given serves no other purpose than to bring all concerned in this special treatment into disrepute.

The type of fracture principally seen in an orthopædic department will be seen from the following list dealt with amongst civilians in the Liverpool Royal Infirmary during 1923:—

CASES OF FRACTURES ADMITTED AND TREATED IN THE ORTHOPÆDIC DEPARTMENT OF THE LIVERPOOL ROYAL INFIRMARY DURING 1923.

Upper Extremity.		
Upper extremity of humerus	29	
Shaft of humerus	14	
Lower extremity of humerus	48	
Shaft of radius and ulna	37	
Shaft of radius	16	
Shaft of radius and ulna styloid	4	
Head of radius	33	
Neck of radius	6	
Shaft of ulna	3	
Olecranon process	13	
Coronoid process of ulna	8	
Colles's fracture, separation of epiphysis and fracture of radius and ulna within 1½ in. of wrist-joint	242	
Scaphoid	7	
Semilunar	1	
Trapezium	1	
Phalanges	40	
Metacarpals	50	
Scapula	10	
Clavicle	36	
Ribs	10	
Jaw	5	
Nasal bone	1	
		614
Lower Extremity.		
Pelvis	1	
Femur	12	
Patella	14	
Tibia and fibula	24	
Tibia	28	
Fibula	36	
Metatarsal	29	
Phalanges	24	
Os calcis	11	
Scaphoid	2	
Cuboid	1	
		182
Total		796

Section of Electro-Therapeutics.

President—Dr. W. J. Turrell.

Some Problems of Bone Growth.

By H. A. Harris, M.B.

(*Institute of Anatomy, University College, London.*)

(ABSTRACT.)

As a result of the praiseworthy gift of the Rockefeller Medical Foundation, University College now possesses an Institute of Anatomy with an up-to-date Department of Radiology, especially equipped for the teaching of the normal radiographic appearances of the human body to the medical student during the years devoted to the study of normal anatomy and physiology. This new departure is already justifying itself, and is proving a worthy testimony to the breadth of vision of Professor G. Elliot Smith and Dr. J. Woodburn Morison, who for many years have advocated the necessity of providing every anatomy school with a radiographic department. The Anatomy Schools of Manchester and University College, London, alone, as yet, have managed to accomplish this. The students thereat now receive a course of demonstrations on the skeletal and visceral organs at the time when they are dissecting the cadaver. The age-changes in the skeleton are studied, and opaque injections are largely employed for the topographic anatomy of the alimentary, circulatory and uro-genital systems. Simultaneously certain post-graduate workers are devoting special attention to skeletal deformities and certain metabolic diseases. The whole process of bone formation in its anatomical, histological and metabolic aspects is being submitted to a series of experimental researches.

One of the first functions of the Radiographic Department at University College is to acquire a collection of normal radiograms of the various ages, male and female. Such a collection will be invaluable for reference purposes, and it is to be hoped that the members of the Section of Electro-Therapeutics, in addition to their clinical advice and co-operation, will forward to University College any normal radiograms which they can spare, of known age and sex. Such a collection will be at the disposal of any interested worker, and will be of far greater use than the incomplete Continental atlases which we are compelled to rely upon at present.

(*A*) Ossification in the Embryo.

Centres of ossification, epiphyses, the three parts of the hip-bone, and the fontanelles of the skull, were first described by Hippocrates. Two thousand years have passed—years characterized by conflicting descriptions, varying methods, rediscoveries and much scholasticism. Fortunately radiographic

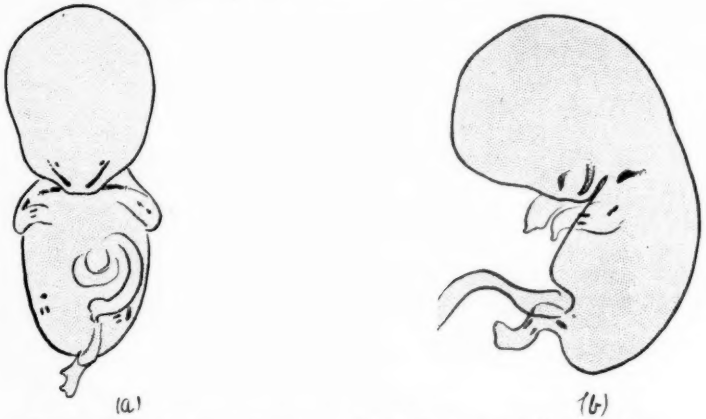


FIG. 1.—Human embryo. Crown-rump length 27 mm.; probable age 52 days.
(*a*) Antero-posterior view. (*b*) Lateral.

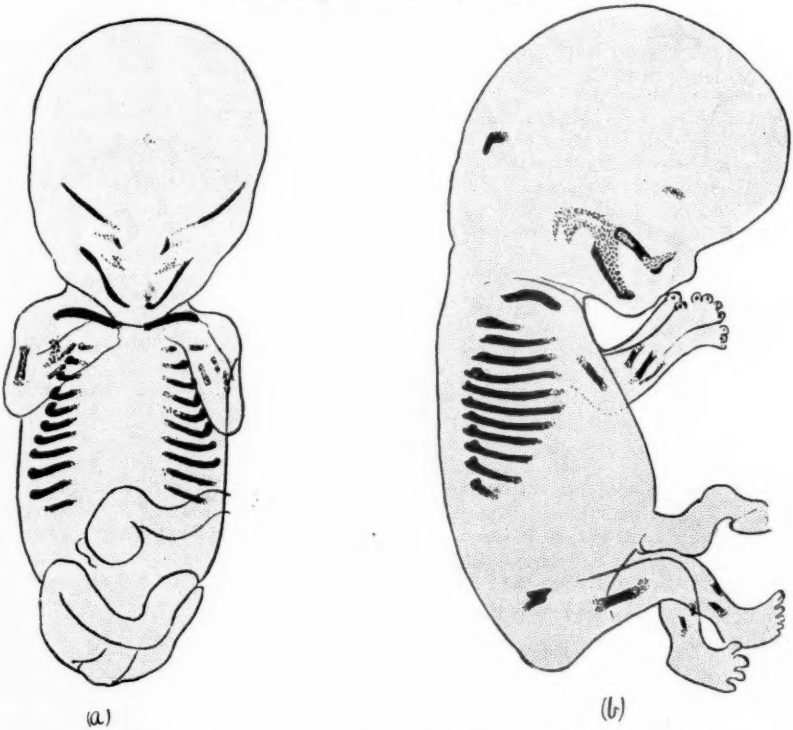


FIG. 2.—Human embryo. Crown-rump length 47 mm.; probable age 68 days.
(*a*) Antero-posterior view. (*b*) Lateral.

examination of the embryo gives immediate, precise, and accurate information as to the extent and order of ossification. Given the embryonic material, and the age of it, the facts which have not been elucidated by two thousand years of slow and laborious research can be laid bare in a short space of time. This is, therefore, a fitting time to make an appeal to the surgeons and physicians in order that they may forward all abortion material to the radiographic department. Outline drawings of radiograms of embryos are reproduced to show the nature of the information gained by this method of examination.

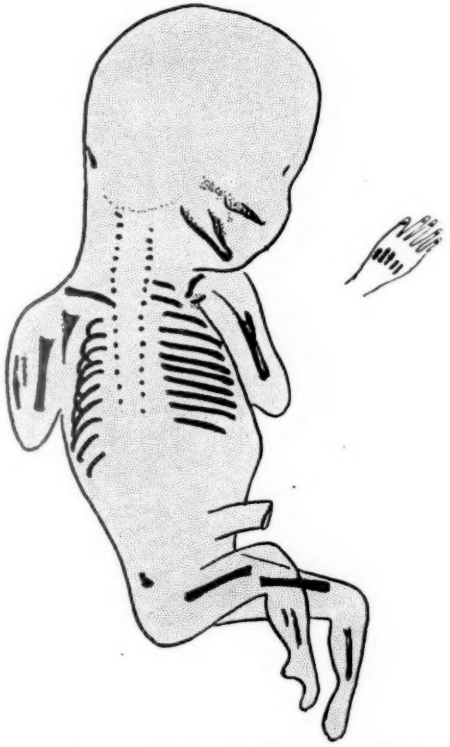


FIG. 3.—Human embryo. Crown-rump length 65 mm.; probable age 80 days.

The first radiograms (fig. 1, *a* and *b*) show an embryo, crown-rump length 27 mm., probable age 52 days, obtained from a case of ectopic gestation. The clavicle, mandible and superior maxilla show distinct ossification; so do the long bones of the limbs. There is no trace of ossification in the vertebral column. The second radiograms (fig. 2, *a* and *b*) show an aborted embryo, crown-rump length 47 mm., probable age 68 days. The face shows ossification in the mandible, superior maxilla, premaxilla, palate; and the cranium shows a definite supra-occipital (tectum posterius) and ali-sphenoid. Twenty-two ribs

are ossified before there is any trace of the vertebral column. The third embryo (fig. 3) is an abortion of crown-rump length 65 mm., probable age 80 days, which now shows ossification of the lateral masses of the vertebral column, commencing in the upper cervical region and extending caudally. The fourth

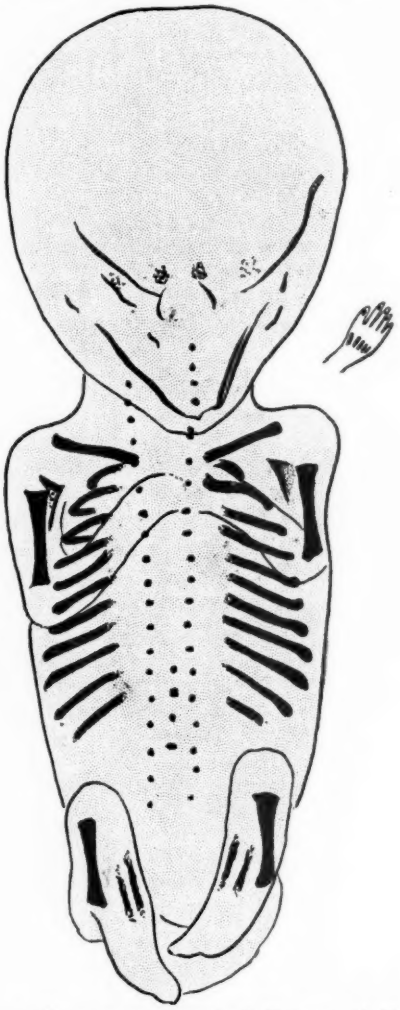


FIG. 4.—Human embryo. Crown-rump length 69 mm.; probable age 83 days.

embryo (fig. 4), crown-rump length 69 mm., probable age 83 days, was removed from a uterus on account of ventral fixation. The process of ossification in the lateral masses of the vertebræ has proceeded to the lower lumbar

region, and the centres of ossification for the centra of the vertebræ have commenced to appear, the process starting in the region of the eleventh dorsal centrum.

The fifth embryo (fig. 5) is an abortion of crown-rump length 70 mm., probable age 85 days. The ossification in the centra of the vertebræ is well advanced. The hand shows five metacarpals, five proximal phalanges and five distal phalanges. The foot shows five metatarsals and the distal phalanx of the great toe is the only ossified phalanx in the foot. The time and order of the appear-

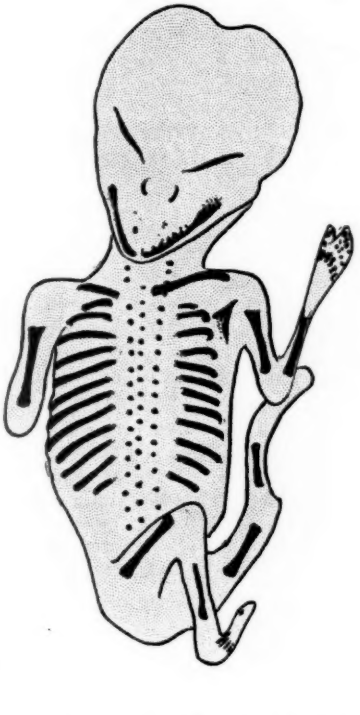


FIG. 5.—Human embryo. Crown-rump length 70 mm.; probable age 85 days.

ances of the centres of ossification of the metacarpals and phalanges effectively dispose of the lengthy quarrel dating from the time of Galen, who first suggested that the metacarpal bone of the thumb was really a first phalanx, the true metacarpal being suppressed. The first centre of ossification in the hand is the terminal phalanx of the thumb. This is followed by the terminal phalanges of the fingers. The centres of ossification for the metacarpals then appear, and are followed by those of the proximal phalanges, so that at eleven weeks the metacarpals, proximals and distal rows of phalanges are ossified. Later, centres

appear for the middle row of phalanges. Thus the thumb has a true metacarpal ossifying in keeping with the other metacarpals, a distal phalanx ossifying in keeping with the other distal phalanges, a proximal phalanx ossifying in keeping with the other proximal phalanges, and it is the middle phalanx which is suppressed. This conforms with the deductions which can be made from the actual insertions of the muscles of the thumb. The detailed ossification of the skull (fig. 5) has been omitted, but it may be pointed out that the mandible displays distinctly the areas of absorption for the dental sacs.

The actual dates of appearance of the various centres of ossification are liable to wide variation, in much the same way as the dates of eruption of teeth.

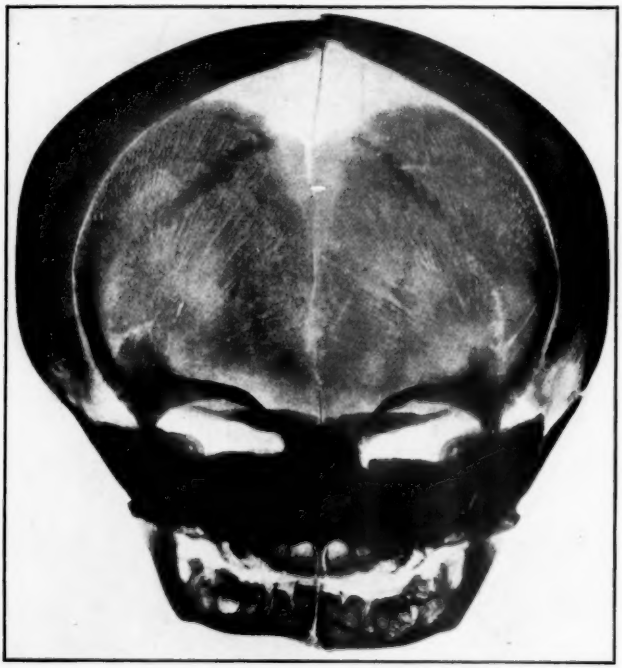


FIG. 6.—(*a*) Norma frontalis, full term fœtus.

The immediate need is to record the findings of a sufficiently large number of radiological examinations to establish a "norm." The variations with sex, race, and ante-natal history can then be approached more scientifically. A point of great importance is this: the radiological study of one embyro, obtained by the removal of a uterus for malignant disease associated with pregnancy, is of far greater value than the study of many abortions. The aborted embryos are somewhat macerated, display varying degrees of shrinkage or dropsy, and are frequently injured in the slow process of abortion. Moreover, many of these aborted embryos are aborted because their degree of development is no longer

compatible with a normal intra-uterine existence. In other words, the embyro is aborted because it is an embryonic monster. Many of the discrepancies which are found in the descriptions of the development of the embryo by

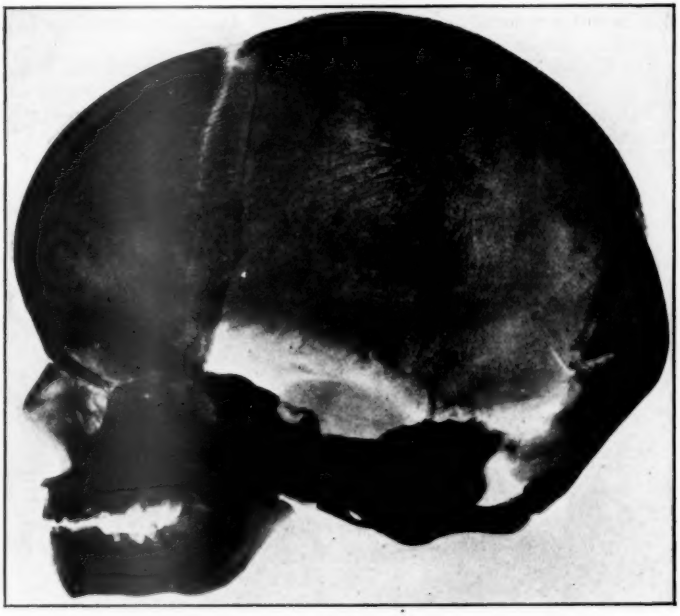


FIG. 6.—(*b*) Norma lateralis.



FIG. 6.—(*c*) Mandible, from above.



FIG. 6.—(*d*) Mandible, side view.

different investigators are due to this fact; the investigators have been examining aborted embryos which were aborted because their development was far removed from the normal.

(*B*) Radiograms of Skull of New-born Babe.

The radiogram of the dried skull displays many structures which are not visible to the ordinary method of examination. In fig. 6 (*a* to *d*) are shown several views of a normal full term fœtal skull. Particular attention is drawn to the following points:—

(*a*) The norma frontalis displays the superior semicircular canals and the mental ossicles at the symphysis menti.

(*b*) The norma lateralis displays the pituitary fossa and tympanic ring, which looks almost downwards and so appears as a narrow ellipse.

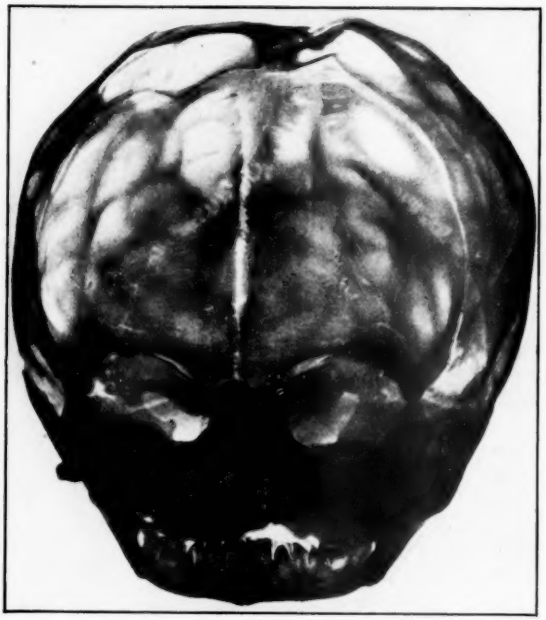


Fig. 7.—(*a*) Craniotabes: Norma frontalis.

(*c* and *d*) These views of the mandible show clearly the advanced stage of enamel formation in the temporary teeth at birth; and the first permanent molar, which will erupt at seven years of age, already presents some degree of enamel formation in its cusps. This displays the futility of ascribing all causes of dental caries to post-natal factors associated with dietary deficiencies. The enamel of the temporary teeth begins to be laid down in the fifth month of fœtal life and the enamel of the first permanent molar in the last month of fœtal life. This is a clear anatomical justification for ante-natal consideration of the dentition of the offspring, and the toothbrush is certainly not the panacea.

(*C*) RICKETS.

In no department of medicine has radiographic evidence been of greater value than in the diagnosis, the assessment of the severity, and the prognosis of rickets. The Medical Research Committee report on "Rickets in Vienna, 1919-

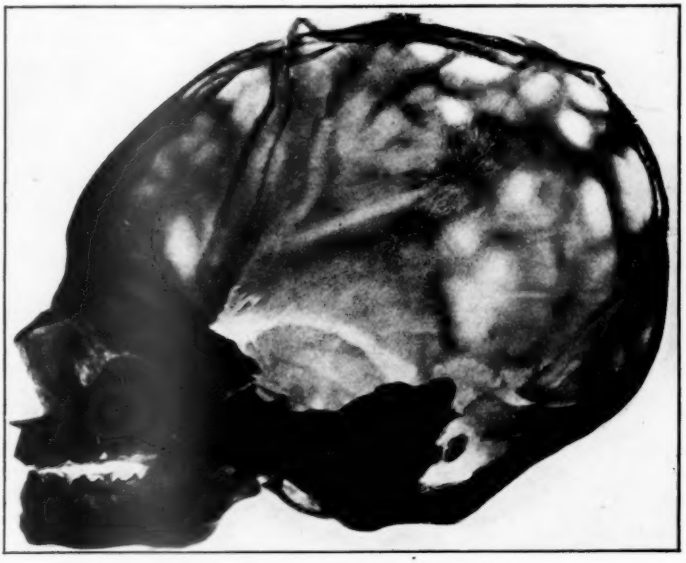


FIG. 7.—(*b*) Craniotabes: Norma lateralis.



FIG. 7.—(*c*) Craniotabes: Mandible from above.



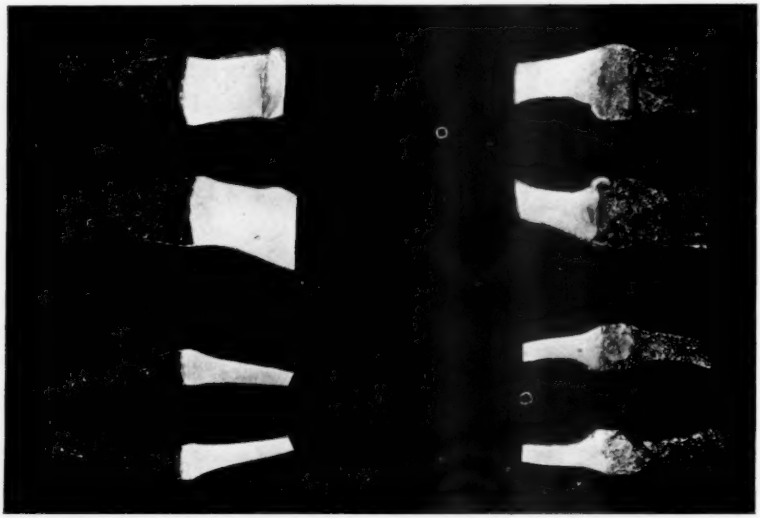
FIG. 7.—(*d*) Craniotabes: Mandible, side view.

1922," lays special stress on craniotabes, beading of the ribs (rickety rosary), and cranial bossing as signs of early rickets. These signs are difficult to assess, are not invariably associated with rickets and may be effectively hidden from

the hand of the most expert observer. It is radiographic examination and histological examination which give the most reliable evidence of the existence of rickets.

Craniotabes is not commonly associated with rickets in this country, and the accompanying radiograms (fig. 7, *a* to *d*) are from the only marked case which has been encountered in a series of 1,000 children at University College Hospital. The delay in the formation of the temporary teeth is very marked. The first permanent molar appears simply as a rarefied dental sac and there is no trace of cusps or enamel formation. This should be compared carefully with the normal mandible shown in fig. 6 (*d*).

Beading of the ribs (rickety rosary) is another equivocal sign of rickets. It is difficult to assess, for enlargement of the costo-chondral junction is normal



(*a*) Normal. (*b*) Rickety.

FIG. 8.—Sections of costo-chondral junctions.

in the same way and for the same reasons that enlargement of the ends of the long bones is normal. The degree of enlargement in the normal rib and in rickets is shown (fig. 8), the sections being made in two directions at right angles to each other. The degree of enlargement may be very marked in certain diseases other than rickets, and cases of acromegaly and Pott's disease have been encountered in which the enlargement was greater than that seen in rickets. Moreover, the beading of the ribs may be confined entirely to the deep surface of the chest wall, and so effectively escape the examining finger. The superficial and deep appearances of the chest of a baboon suffering from marked rickets are shown (figs. 9 and 10). The absence of "beading" on the superficial aspect is in marked contrast with the excessive "beading" on the deep surface. Thus absence of beading clearly does not negative a



FIG. 9.—Breast-plate. Baboon. Superficial aspect. No visible or palpable marked beading of the ribs.

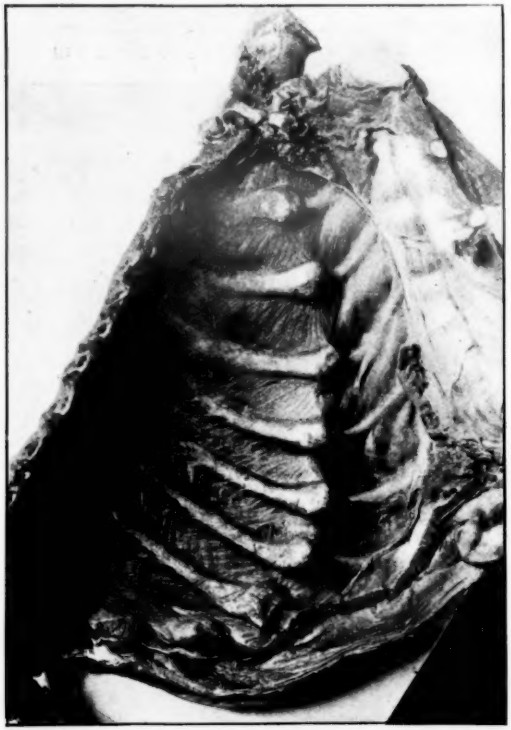


FIG. 10.—Breast-plate of same baboon as fig.9. Deep aspect with marked beading of the ribs.

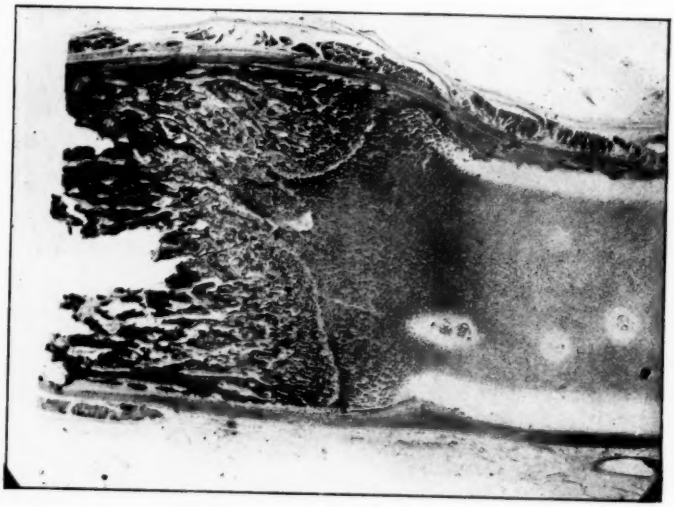


FIG. 12.—Costo-chondral junction. Babe, newborn Congenital syphilis.

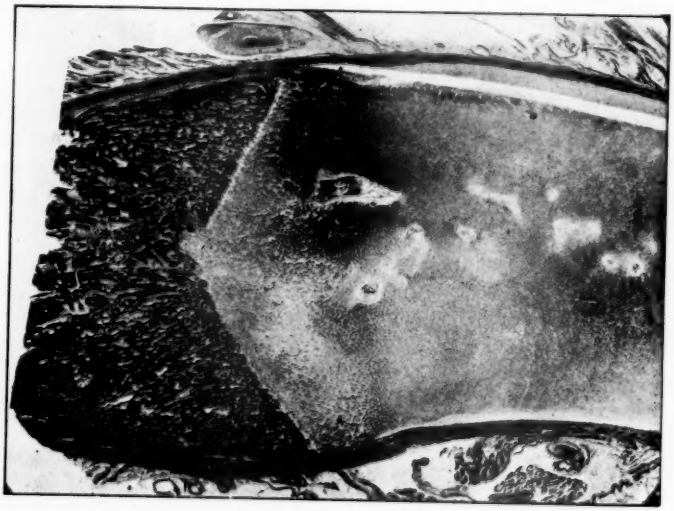


FIG. 11.—Costo-chondral junction. Babe, 6 months.

diagnosis of rickets; the presence of beading alone should not necessarily lead to the diagnosis of rickets.

One might add that another clinical sign of exaggerated importance in the diagnosis of rickets is sweating. Since visible perspiration is not usually seen on the normal child until the fourth to the sixth month, it is very difficult to find a mother of a baby of seven months who will not reply to a leading question that "baby sweats a great deal on the head."

Thus, since histological examination is impossible in living patients, there is no sign of such value as radiographic examination in diagnosing the existence



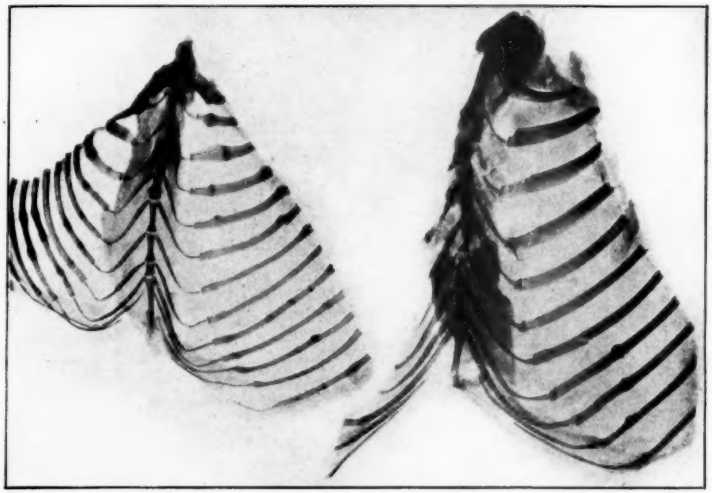
FIG. 13.—Costo-chondral junction. Monkey, rickets.

and in assessing the extent and course of rickets. Instead of reproducing the familiar radiograms of the epiphyses in rickets, photomicrographs of the costo-chondral junction are shown in order to demonstrate the condition seen in some of the specimens submitted to histological examination. The first case (fig. 11) illustrates a normal costo-chondral junction in a babe of 6 months; the second (fig. 12) is from a case of congenital syphilis, the third (fig. 13) from a case of marked rickets in a monkey which had been fed on an "all-round" diet.

In the routine examination of a series of chests of animals at the Zoological Gardens it has been found that whereas monkeys easily acquire a disease closely corresponding to clinical rickets as regards radiographic and histological

findings, the carnivorous animals which spend their time restlessly and unceasingly walking up and down the cage, present a condition corresponding in some respects to the rare cases of osteo-sclerosis fragilis generalisata, or "Marmor-Knochen," which is known as Albers-Schönberg disease. The breast-plates (fig. 14) of *Canis thous* and *Vulpes chama* show marked absorption of bone, numerous fractures in all stages of healing, and unaltered costo-chondral junctions.

Every science has its larger aims and purposes. Electro-therapeutics is now in process of extending its horizon beyond routine auxiliary work as an aid to clinical medicine and surgery. Electro-therapeutics is concerning itself with the most romantic of subjects, the general topic of generation, and so it takes within its purview the phenomena of impregnation, problems of heredity, the origin of sex, the conditions of gestation and pregnancy, and the physiological causes



(*a*) *Vulpes chama.* (*b*) *Canis thous.*

FIG. 14.—Radiogram of breast-plates.

of birth. Radio-biology is throwing much light on the general problems of cyto-morphosis and the chemical and physical factors of cell development. In particular is it necessary to stress the strictly anatomical aspects of radiology. Radiological study of embryos and growing children can furnish the morphological interpretation of many organs and the explanation of many anomalies of adult structure. Obscure anatomical points are illuminated by radiology, and much light is thrown on the general search for laws of growth. It is to be hoped that the members of the Section of Electro-Therapeutics will follow the admirable example of several of their past Presidents and assist in a practical manner the establishment of radiographic departments in the anatomy schools.

This opportunity is taken of acknowledging the valuable advice of my colleagues at University College Hospital and at the Zoological Gardens, and the ready help of Mr. Melville, the radiological assistant in the Institute of Anatomy.

Section of Electro-Therapeutics.

President—Dr. W. J. TURRELL.

Some New Observations and Experiments in Electricity.

By Professor STEFAN JELLINEK

(University of Vienna).

BEFORE beginning my lecture I must express great pleasure at having been invited to discuss before your Section a subject on which I have been working for so many years, namely, *the nature and motion of electricity.*

I am all the more conscious of the difficulty of my task when we bear in mind that it is in England that the greatest discoveries in electricity have been made; we need only to remember the names of Faraday, Maxwell, and Crookes amongst many others.

Only the quantity of material and the fact that all my observations seem to point to one certain law give me the courage to display the following slides in order that you may form your own conclusions with regard to the same.

These slides represent photographs of various marks and traces left either by atmospheric or technical electricity on both organic as well as inorganic matter. The most noticeable feature of these designs resides in their *morphology.* It was through studying the pathological conditions of the electric mark that I was drawn to the study of this branch of science.

Even since 1912,[1] when I had the honour of delivering my last lecture here, I have been pointing out that these marks must not be confused with the marks produced by burns; they have their own particular clinical and pathological-anatomical features as well as their own histological and radiological characteristics, as I am prepared to demonstrate by a single example.

No particular medical knowledge is required to realize that these marks constitute a special class in themselves; even a single glance at the outer form is quite sufficient to convince quite an ordinary layman that it is this outer form which plays the dominating part. Again and again we see the following three forms: the *circular*, the *spiral* and the *straight line.*

These *fundamental* forms appear not only on human beings and on organic matter such as wood, &c., but also on inorganic matter such as glass, &c. On this account I have divided the whole of my observations into *three large groups*, corresponding with the circle, the spiral and the straight line. These are followed by a *fourth group* in which a whole series of any one of these traces or marks appear as if forming a *rhythmical design.*

We must remember, however, that it is impossible to make any exact classification; very often traces appear which might with equal justice be placed in more than one group.

[1] *Proceedings*, 1912-13, vi (Sect. Electr.-Therap.), pp. 17-36.

In each group I am showing first of all marks on the human body.

The *second* part of my lecture deals with my own *experiments*, in which I have tried to reproduce such marks artificially. These can also be arranged in three different groups;

(*a*) Transient phenomena of the light arc-current.

(*b*) Transient phenomena in Crookes' tubes.

(*c*) Discharges of high voltage on organic and inorganic matter.

First of all I studied the *electric arc-current* between two electrodes at a distance of 35 cm. from a transformer of 70,000 volt and $\frac{1}{10}$ ampere, and then photographed both by *cinema* and *stereoscope*.

Secondly, I observed the different phenomena produced by the spark current in a *Crookes' tube* according to the degree of vacuum; of these I am showing eight different slides corresponding to the various degrees of vacuum up to the highest degree of the same when the X-rays appear.

Both groups consist of *transitory phenomena*; the following third group consists of *permanent* marks on organic or inorganic matter.

Thirdly, I observed the effect of a *discharge of high voltage* on various organic and inorganic matter, such as on the cornea of the larva of salamander, &c.

In connexion with the above-mentioned experiments I should like to point out that I was able to produce fulgurites *artificially* by a heavy discharge on the sand taken from the Danube.

The *results* of these experiments, some of which I am now illustrating by lantern slides, have not only supplemented our observations, but also seem to have furnished us with a clue as to how electricity flows and works, i.e., the *modus procedendi of electrical energy*.

These two problems, namely, the *morphology* and *the dynamics* of these traces or designs, represent the alpha and omega of our researches, and it is from this standpoint that I must beg all those present here to-night to follow them.

Before proceeding to the groups I must relate one case, which stands quite by itself and yet is sufficient to prove the *characteristics of the marks* in question.

Fig. 1 represents the case of a boy, aged 18, who, in order to carry out a wager, climbed up an electric post and touched a wire of 15,000 volts with his left hand intentionally.

Fig. 2. The photograph of his left hand with deep open wounds.

Fig. 3. Mummification of arm showing naked bone visible far into the axilla; blood-vessels and nerves have completely disappeared.

Fig. 4. Here the arm has fallen off painlessly leaving only the stump and without the slightest traces of bleeding.

Fig. 5. Spontaneous healing of the stump.

Fig. 6. Inspection of arm by X-ray; nothing whatever is visible.

Fig. 7. A photograph taken a few days before the spontaneous amputation; a diagonal line is seen crossing from axilla to the lower part of deltoid muscle; this diagonal marks exactly where the arm afterwards fell off.

Fig. 8. We see the stump showing parallel lines or rather the cylindrical formation round the bone; this appearance is not easily explained.

Slight Skin Injuries.

Slight *skin injuries* leaving *current marks*; the *histological* changes also possess their own characteristics.

Fig. 9. A cut through the epidermis and cutis of such a current mark. The outer layers of epidermis, where the current had passed through, are compressed tightly together; the cells of the rete Malpighii and their nuclei are drawn into long threads, five or six times their ordinary length.

Fig. 10. Part of the same specimen greatly enlarged.

Fig. 11. *A cross section of a hair-sheath; here in one part we find a few single cells drawn into long threads.*[1]

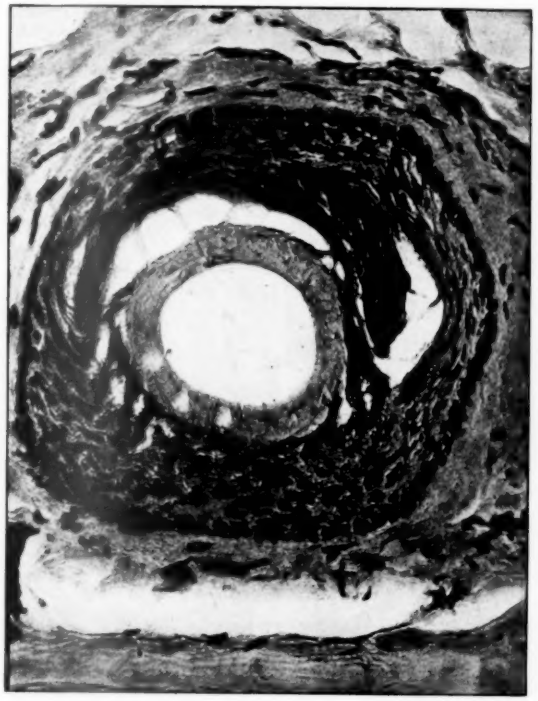


FIG. 11.

I. CIRCULAR TRACES.

Fig. 12. A forefinger of a man, wounded by an electric current, shows the wound in its first stage.

Fig. 13 shows the wound in healing stage.

Fig. 14 shows the scar. In all three stages the *circular* form is visible.

Fig. 15. The hand of a woman struck by lightning; several very small sharply defined circular spots, some with concentric rings.

N.B. the similarity to picture of experimental eggshell (*vide* fig. 81, p. 60).

[1] The legends of the figures which are reproduced are printed in italics.

Fig. 16. *The head of a soldier struck by lightning, showing circular spots on scalp; the hair here has completely disappeared, yet no traces of burning are visible.*

Fig. 17. A bird killed by an electric discharge. The body shows a sharply defined circular wound. Yet the feathers show no trace of burning.

Fig. 18. *A black spot visible on the wall of a house struck by lightning. N.B. the sharply defined circular form, the gradual fading of the colour towards the periphery*

Fig. 19. A shoe struck by lightning showing clearly defined circular holes. The leather is not singed.

Fig. 20. A piece of underlinen, struck by lightning, showing several holes with tendency to circular shape, no signs of scorching.

Fig. 21. A brick struck by lightning showing a smooth spherical cavity.

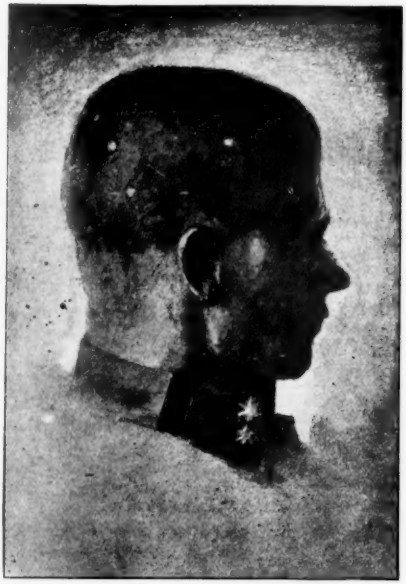


FIG. 16.

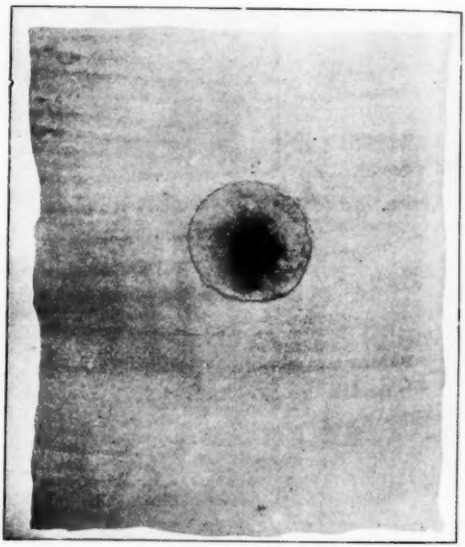


FIG. 18.

Fig. 22. A piece of carborundum-rod hollowed out by lightning showing a calibre with circular cross sections.

The foregoing example, together with the two following, might with equal justice be placed in the third group (*straight* line), if we regard them according to their length.

Fig. 23. Piece of glass tubing found under the ground in the place of a short-circuited cable shaped somewhat like a bottle.

Fig. 24. Piece of cylindrical glass tubing, not hollow but solid, found on a field at a place where a wire had dropped from a porcelain insulator along the tract at which it had come into contact with the ground.

Fig. 25. A porcelain insulator struck by lightning showing a formation of a funnel penetrating entirely through the insulator.

Fig. 26. The funnel enlarged, absolutely symmetrical in form, the inner surface remarkably smooth, no signs of melting whatever to be seen save at the narrowest extremity. The wide upper rim is surrounded by a series of five circular segments. Here the question must be asked: What kind of force or motion would be able to produce a funnel possessing such characteristics as absolute symmetry, absolute smoothness as well as a series of segments? The answer is, that only a *boring and progressive motion* could possibly produce such an effect.

Fig. 27. *A piece of root of a fir-tree struck by lightning showing a circular hole, the inner sides of which are lined with melted glass, showing formation of fulgurite within the hole in the fir-tree. Note particularly that the centre of this hole is entirely free from glass, whilst the glass sides are of equal thickness.*

N.B. This formation can also be rendered possible by a screw-like motion of the energy.

Fig. 28. A spherical bulb showing semicircular traces in series of threes. This figure will be shown again under the group of *rhythmical* designs.



FIG. 27.



FIG. 30.

Fig. 29. A flat pair of pincers showing several circular concentric traces of molten iron. On closer observation we see that these apparent circles are in reality spirals. This figure constitutes a transition to our second group.

II. SPIRAL TRACES.

These are more often produced by lightning than by technical electricity.

Fig. 30. *Arm of a boy struck by lightning showing a flat brown spiral mark.*

Fig. 31. Forehead of a soldier struck by lightning, showing a zigzag spiral mark.

Fig. 32. Back of a man struck by lightning, showing a series of broken spiral lines extending diagonally from the right shoulder to the left hip.

Fig. 33. Piece of pear-tree struck by lightning, showing spiral traces penetrating into the wood itself, forming a groove.

Fig. 34. Piece of oak-tree struck by lightning showing a wider groove of remarkable regularity as though done by a turner.

Fig. 35. Transverse section of the same piece of wood showing that here the effect of lightning is not limited to the outer surface, but has also changed the inner texture; this change assumes a circular form and is apparent even to a naked eye.

Fig. 35A. Microscopical inspection of the foregoing: separate vessels are torn, their sides displaced; no signs of burning or explosion.

Fig. 36. *A cherry-tree 10 m. high struck by lightning, showing an unusually fine spiral formation extending from the top to the bottom of the tree. We can distinguish four complete screw-like turnings. The cross section of these turnings shows three different levels: the groove in the middle runs throughout the whole length of the spiral.*

All these above-mentioned traces show no signs whatever of burning.

Fig. 37. A piece of the bark of an oak-tree struck by lightning showing the flat sign of an S as though branded with a hot iron.



FIG. 36.

Fig. 38. A piece of copper wire from lightning conductor bent in the form of a double S with the ends distinctly molten.

Fig. 39. An iron bar crossing a window struck by lightning bent into an obtuse angle.

Fig. 40. A carborundum rod struck by lightning showing a spiral formation as though moulded in plastic. Throughout the whole length of this plastic spiral runs a narrow groove. Compare the carborundum rod in Group I (fig. 22, p. 52) with a straight calibre.

Fig. 41. A porcelain switch struck by lightning showing a series of spiral fissures as though caused by some sharp instrument.

Fig. 42. A view of the former taken under a microscope showing deposits of molten copper on the surface of the sides of these fissures.

Fig. 43. A glass tube formerly containing a safety fuse struck by lightning showing traces of sharply defined windings, partly curved, partly angular.

Fig. 44. A porcelain insulator destroyed by short circuit showing several wave-like lines running parallel; in reality these are delicate fissures.

Fig. 45. An electric bulb struck by lightning showing partly regular, partly irregular spiral lines on the glass. Throughout the inner rod a spiral extends, which is broken off towards the end and is met by a similar spiral facing it and showing the same relation towards it as any object to its reflection in a mirror.

Fig. 46. *A photograph of sheet lightning taken by Dr. Walter, in Hamburg, remarkable on account of the spiral formation.* This has been explained away in various ways but most scientists seem to consider it due to a flaw in the film or the result of the action of the wind during the storm. It is impossible to express any definite opinion with regard to these theories. I wish only to point out the similarity between the spiral shown here and the spirals seen on my bulb.

Fig. 47. A fulgurite from Mecklenburg, where I have also dug out similar specimens, showing clearly marked signs of spiral worms.

III. Traces Marked in Straight Lines.

Fig. 48. A scalp of a man killed by lightning showing that the hair here has been completely shaved off as though done by a machine, leaving the hair on both sides in two parallel lines running from the forehead to the neck.



Fig. 46.

The symmetrical form reminds us of an observation made by Arago, in which he noted, that on one occasion the lightning dug out a prismatic trench in the earth several metres long, in such a way that the soil was deposited symmetrically on either side.

Fig. 49. Piece of dead fir-tree struck by lightning showing a groove bounded on the right and left sides by parallel straight lines. The fact that no signs of burning are visible is all the more remarkable, as the tree was already dead and the wood quite dry.

Fig. 50. A spherical bulb showing straight lines with double contour.

Fig. 51. The chest of a woman struck by lightning showing burns bounded by a straight line across the waist. Strictly speaking this case does not belong to this group, as the straight line across the waist may be due to the constricting action of the clothes, which were tightly bound round the waist. As I have already noticed similar cases, this seems to prove that the power of lightning is not always unlimited.

IV. Traces in Rhythmical Order.

Fig. 52. The lower part of the back of a woman struck by lightning showing a series of blisters forming an S.

Fig. 53. The back of a man struck by lightning covered by large burns, in the upper part in the region of the scapula are to be seen a great many finely drawn lines as fine as if done with a pen of a compass. These lines are red and absolutely parallel, occurring at regular intervals and resembling the lines drawn with a ruler.

Fig. 54. The thigh of a man, who received an electric shock of 45,000 volts, and died two days later from paralysis of the kidneys. The lower part of the thigh is covered completely with twenty-seven concentric semicircles consisting of bead-like marks.

Fig. 55. The bark of an oak-tree struck by lightning showing two groups of little holes burnt into the bark.

Fig. 56. A horn-shaped lightning conductor struck by lightning showing a series of molten beads running in spiral form throughout the length of the horn of copper.

Fig. 57. The head of the ordinary brass screw struck by lightning showing under the microscope a series of very fine little holes as though indented by a pencil. No signs of melting are visible.

Fig. 58. The apices of two lightning conductors, the larger in copper, the other in platinum, each of these showing groups of finely engraved short parallel lines occurring at regular intervals; in the former the lines are horizontal, in the latter they are vertical. Compare the lines already referred to on the back of a man struck by lightning, which seems to prove that the morphology is the same.

Fig. 59. Fragment of a looking-glass the amalgam of which has completely disappeared except in one corner. In the place of the amalgam we see a dendritic design exactly identical with those seen on the skin of man when struck by lightning. The ramification of the same shows radiative characteristics.

Fig. 60. A looking-glass unbroken, but slightly damaged by lightning. Along the outer edge we see several dendritic designs similar to the former. A closer inspection under the microscope shows that these are crossed by numerous concentric wave lines. The two systems of lines crossing one another remind us of a spider's web.

Fig. 61. *A porcelain insulator short-circuited; to the splinters and the supports no reference is necessary, but along the porcelain ring connecting the two cones we see two groups of erosions branching off to the right and left of the centre-point. If we imagine a perpendicular line representing a mirror placed in this centre-point the two groups show the same relation to one another as an object to its reflection in the mirror.*

EXPERIMENTS.

A. Transient Phenomena of Light Arc Current.

I have removed one or two simple films from my *cinema*, which I am now displaying.

Fig. 62. A photograph of the light-arc current showing a simple wave line.

Fig. 63. A photograph showing a similar wave line splitting up at the meeting point into two or three wavelets.

Figs. 64 and 65. *Stereoscopic* photographs of the same.

These wave lines split up into numerous wave lines gravitating towards one another. The manner in which each single wave unites with its corresponding wave on the other side seems to point to a fixed law.

B. Transient Phenomena in Crookes' Tubes.

Having found the figures shown by the light-arc current similar to those I had already observed in nature and technics, I then resolved to study the phenomena in *Crookes'* tubes and was astonished to find that these well known

phenomena are very similar in shape and appearance to the permanent marks shown on my electric bulbs either after lightning or after short circuit or other disturbance.

In the following eight pictures I show eight successive designs formed in the Crookes' tube. These designs, varying as they do, form a series of evolutions according to the increase of vacuum, which seem to represent nothing less than the evolution of a screw line; also the apparent straight lines, which become visible at the moment when X-rays appear, can be included under this category, when we remember that physicists describe



FIG. 61.

X-rays as *waves*. Also we, as medical men, are accustomed to speak of soft and hard X-rays according to their greater or shorter wave-length. Under these circumstances we should be justified in terming these eight evolutions *an embryology of X-rays*.

PHENOMENA IN CROOKES' TUBES AT VACUUM OF 76 MM., 16 MM., 2 MM., 0·24 MM., 0·1 MM., 0·02 MM., 0·01 MM., 0·001 MM. (FIGS. 66-73.)

Fig. 66 shows a simple spiral line.
Fig. 67 shows an opaque mass of light.

Fig. 68 shows spiral twisted lines.
Fig. 69 sharply defined crescents.
Fig. 70 the whole tube indistinct and blurred.
Fig. 71 similar to 70.
Fig. 72 a repetition of fig. 69 but indistinct.
Fig. 73. Kathode-rays and appearance of X-rays.

The following five pictures (figs. 74-78) illustrate designs found in *deranged bulbs*. N.B.—The predominating feature once more lies in the three recurrent forms: the circular, spiral and straight line. Compare with the *similar designs* in the Crookes' tube.

Fig. 74. An ordinary bulb, the inner rod of which having been struck by lightning shows a wave line extending along the whole rod.
Fig. 75. An ordinary bulb shows several spiral lines extending along the inner surface of the bulb.
Fig. 76. A spherical bulb shows a series of crescents in groups of threes.

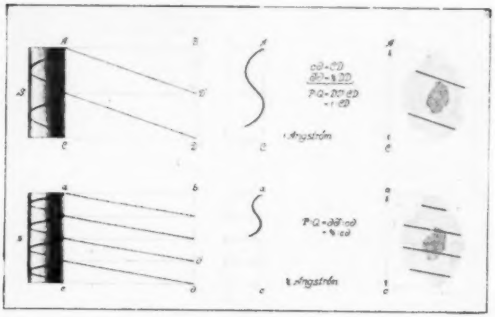


FIG. 79.

Fig. 77. A spherical bulb with deposits covering the whole inner surface, showing however some indistinct wave lines.
Fig. 78. A spherical bulb showing straight lines extending along the whole inner surface.
Fig. 79. *Represents a diagram, which I have constructed as an attempt to explain my hypothesis, that the wave line constitutes nothing else than a projection of a screw.*
According to this supposition the hard waves would have a proportionally greater efficiency of penetration than the soft waves; note the *mathematical equation.*
Let us suppose this diagram to represent the probable action both of a soft wave as well as a hard wave on exactly the same spot in a minute corpuscle of a human cell: in the case of the soft rays the corpuscle is attacked only by two waves leaving the most minute corpuscle untouched; in the case of the hard rays the same spot is attacked by four waves or rather wedges, and in consequence the minute corpuscle cannot possibly escape.

C. Organic Matter Struck by Discharge of High Voltage.

Fig. 80. *A cornea of the larva of a salamander, which was struck by a spark of diathermy apparatus.*[1] *We see here two great holes where the spark has passed through. The essential point lies in the fact that the holes are surrounded not by broken tissues, but by lamina-like formations; the cells have been drawn into long threads.*

[1] Experiment made in the University Institute for Embryology (Professor Fischel and Dr. Politzer).

Fig. 81. *Eggshell struck by discharge of a transformer of 6,000 volts, showing numerous concentric circles; each circle is sharply defined and absolutely regular. These circles, or rather zones, do not merge away into the colour of the shell, but are clearly marked. Note two circles merging into one another and forming an ellipse, three others forming a rosette, &c.*

D. Inorganic Matter Struck by Discharge of High Voltage.

The idea of these experiments occurred to me while busy digging for fulgurites in the forests of Mecklenburg. Natural fulgurites, as we all know, consist of hollow tortuous glass tubes, originating from molten sand. I succeeded in digging out one of these from a depth of $2\frac{1}{2}$ m.; the latter was a thick hollow glass tube, as thick as my little finger.

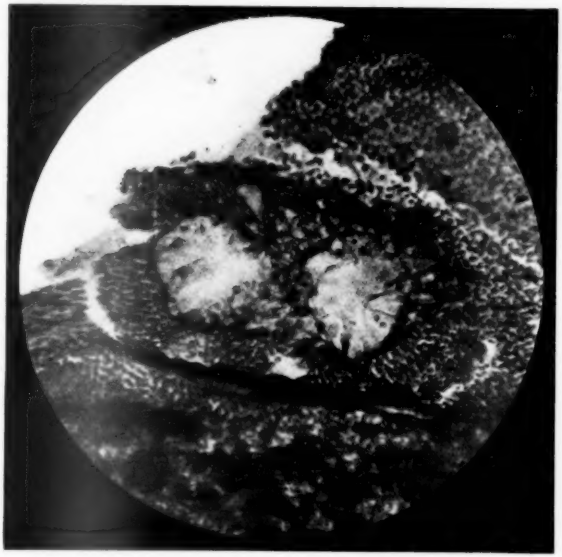


Fig. 80.

Another of these fulgurites was most remarkable on account of the following circumstance: at a distance of about 2 cm. there was no glass tube, but only a hole in the sand, forming a mould of the tube and connecting with the glass tube lower down. As it was impossible to move this without spoiling it I had a plaster of Paris cast made of it, which I am now showing.

Fig. 82. Fulgurite from Mecklenburg, 75 cm. long, hollow, transverse section circular, tube itself tortuous in form.

Fig. 83. Plaster of Paris cast of a fulgurite, in which, however, the sand remained unmolten.

Fig. 84. An *artificial* fulgurite produced in sand by a discharge of high voltage. The photograph was taken immediately after the generation of the fulgurite.

Fig. 85. *Another artificial fulgurite twisted round it in a complete angle of 360 degrees.*

Fig. 86. *The calibre of such an artificial fulgurite, the transverse section of which shows an exact geometrical circle.*

Fig. 87. A tree of fulgurites produced by numerous discharges. We might call it *an electric blossom.*

A COMPARISON BETWEEN SOME ELECTRIC TRACES AND ENANTIOMORPHIC CRYSTALS.

Before we come to any conclusions, we must consider more closely two of our above-mentioned specimens, viz. :—

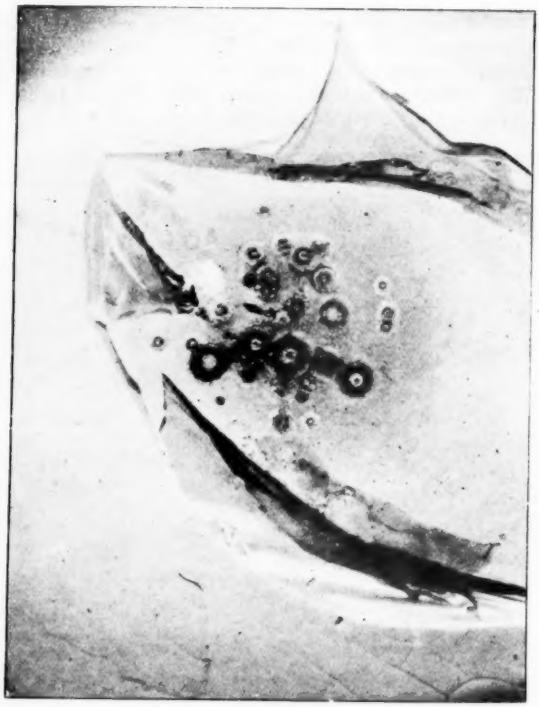


FIG. 81.

Fig. 88. (1) *The inner rod of a bulb with two spirals facing one another.* (2) *The porcelain insulator with its two groups of engravings.*

These specimens are absolutely unique on account of the fact that in each case there are *two systems* or *two halves* visible bearing the same relation to one another as an object to its reflection in a mirror. In seeking for an analogy to this phenomenon our attention was drawn to the enantiomorphic crystals. The latter invariably appear in two different forms, bearing the same relation to one another as an object to its reflection in a mirror. Such crystals are generated, e.g., in tartaric acid, in silicates, &c.

Crystallographers suggest that this enantiomorphic property exists not only in the outer form of the crystals, but also even in the most minute particles or atoms of which the crystal is composed; they furthermore suggest that these minute corpuscles are built up in the form of a screw, and that optica activity, which according to Pasteur is invariably possessed by these crystals



FIG. 85.

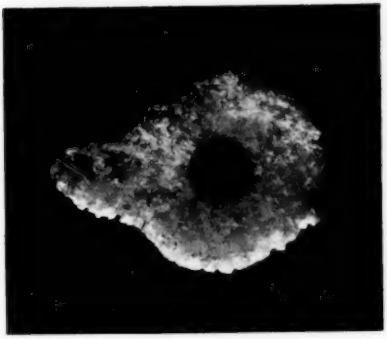


FIG. 86.

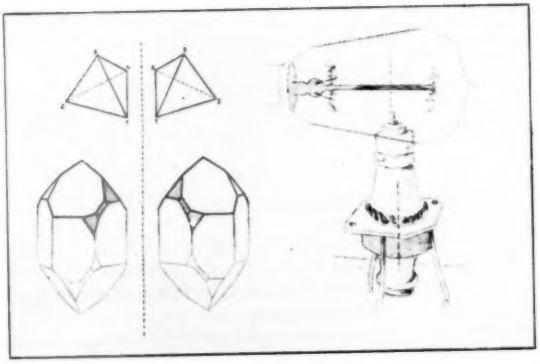


FIG. 88.

varies to the right or left according to the direction in which the atoms are disposed. In connexion with this I may mention that crystallographers regard the crystals as being generated by electric forces.

I need not say I am quite aware of the fact that the case of the enantiomorphic crystals is nothing but pure analogy in respect of the point in question here; in spite of this, however, we are instinctively drawn to ask ourselves the following question: *Does this contrast of appearance or rather this dual system lead us to the obscure field of electric polarity?*

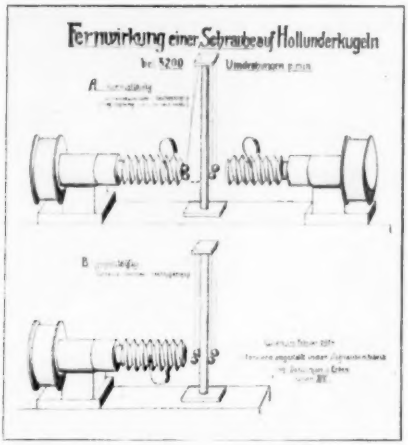


Fig. 89.

With regard to this question of polarity, viz., whether two rotating screws facing one another would exercise any remote action on one another, I placed some elder tree bullets suspended on a silken thread between them and observed that the bullets were drawn towards the screw only when the latter rotated in a certain direction. My experiments seem to indicate that the remote action is not alone due to air currents, and that the efficiency of the screws seems to alter both according to the direction in which its worm is twisted, as well as according to the direction in which the screw itself rotates.

Fig. 89 *illustrates the momentum at which elder bullets are attracted.*

Conclusions.

Starting from the electric marks appearing on the human body, the clinical and morphological characteristics of which are well known nowadays, I proceeded to analyse the marks appearing on inorganic matter, whether produced by atmospheric or technical electricity.

The *first* result of our studies has shown us a *uniformity in designs* which can be classified without exception into three great fundamental groups, viz., the *circular*, the *spiral*, and the *straight line*.

These three groups are followed by a fourth group comprising any single one of the three former groups recurring periodically or *rhythmically*.

The *second* result has shown us a uniformity in *the nature and quality* of these designs, i.e., in decolorization, in deposits of known or unknown origin, in physical or chemical alteration, in plastic mouldings of the matter in erosions and similar losses of substance, as though done by means of a chisel or of a forming tool, and finally in effects or marks, which could only have been produced by heat. It is worth noting, that not all combustible bodies show signs of burning, and not all metals show signs of melting.

This uniformity with regard to the design and nature of the marks is entirely wanting with regard to the *dimension:* the size varies from fractions of millimetres to whole metres; compare the spiral in the amalgam-mirror with the windings shown on the cherry tree.

Although with regard to size and dimension we no longer find the same uniformity, yet we are convinced that this seeming contradiction does not in reality prove any contradiction whatever to the theory in question. Even the traces, which we have observed up till now, although they form a scale in themselves, do not represent the utmost limits either in greatness or in smallness. One thing we do believe, and that is, whatever the size or dimension of the *prime-energy, this primary unit of energy and motion*—this autochthonic movement of electricity—*must revolve in progressive and boring motion*, in other words, in the same way *as a screw does its work*. According to this supposition the various sizes represent nothing more than the various transmissions and transformations from *one primary unit of energy and motion*. Whether this unit be the electron itself is a question which only physicists can decide.

In respect of this problem we may mention that also the white rays of the sun are composed of numerous rays of various wave-lengths.

Before concluding my lecture I will refer once more to the marks which we compared with the enantiomorphic crystals; these marks deserve particular attention, leading, it may be, *to the obscure field of electric polarity*.

I am quite prepared to admit that many of my observations and specimens might be explained in quite different ways, e.g., by the ordinary laws of electrotechnics, or by isotropic or geometric and various other conditions; yet there are certain of these observations which cannot be possibly explained by any of the theories hitherto employed, e.g., the glass rod with the two screw lines facing one another, the absolutely circular calibre of both the natural as well as the artificial fulgurite and other specimens.

In addition to this, we must not forget that, since all traces left by electricity are generated by one and the same force, they must in consequence be regarded from one and the same standpoint; and here we are compelled to note that in *all specimens, without exception, there is the same morphological and the same dynamic uniformity* to be observed, viz., *all designs without exception can be traced back to the motive action of the screw showing the regularity in form* (*morphology*) *and the same regulation in motion* (*genesis*). These designs are so characteristic in their appearance that we are able to *decipher the electricity* by means of these designs; at the present day we can therefore discuss quite freely a new theme, namely, *electrophysiography*.

To draw the attention of the British physicists and scientists to this new problem has been the chief aim and object of my visit to this country; needless to say it has afforded me the greatest satisfaction to be able to deliver my lecture before so attentive an audience.

The Influence of Radiology upon our Conceptions of Disease:

THE MACKENZIE DAVIDSON MEMORIAL LECTURE.[1]

By Sir THOMAS HORDER, Bt., M.D.

THE Mackenzie Davidson Lecture commemorates annually the life and work of a distinguished pioneer in the field of radiology. It was thought by those who founded the lecture that it was an additional and an appropriate tribute to the memory of the man they sought to honour, who combined a true scientific spirit with the exercise of the healing art, that the lecturer should be alternately a pure scientist and a member of some branch of the medical profession. This, rather than any merit of my own, explains why the honour and the obligation of delivering the lecture have fallen on me. I am here to carry on, however unworthily, the tradition which you have established, and I follow, haltingly, in the footsteps of such distinguished men as Professors Rutherford, Halliburton, and Kaye, and Dr. Arial George. This explanation will, I am sure, entitle me to your indulgence.

As a physician it has been my privilege to be very closely associated with the students and exponents of radiology ever since the science had its birth. I have therefore had full opportunities of watching its progress, from the small beginnings of the nineties to the great developments of the present time. I am probably more indebted to my colleagues in this branch of our profession for help and guidance in diagnosis, and for treatment of my patients, than to any other single branch, not excluding the pathologists and the surgeons. I only say this to emphasize my interest in radiology. But this close association and its practical results are not the subject of my lecture. Neither is it a consideration of all those individual diagnostic and therapeutic achievements of which the radiologist may well be proud, and upon which physicians become so dependent in the ordinary pursuit of their daily practice. I wish to deal with a larger aspect of the subject, and to mark down some of the ways in which, as the result of these almost breathless strides that your science has taken, our ideas about certain diseases have changed.

In the first place, let us consider just where radiology stands in relation to other parts of the scheme of medical study. In our conception of the nature and extent of disease processes clinical medicine is capable of contributing admirably clear pictures as the result of careful examination of the surface of the body. And by the exercise of the trained hand and ear we are able to elicit physical signs of lesions lying beneath the surface. But for ocular demonstration of any disease process which affects deeper structures we are dependent upon three methods: there is the experience of the post-mortem room, there is the brief and limited observation afforded by surgical operations, and there is the valuable help given by X-rays. Of the enormous value of the post-mortem room in controlling our imaginations and in teaching us the gross and the minute structural changes produced by disease I need say nothing more. I am, however, tempted to utter an exhortation to radiologists amongst others to remember what a bed-rock of fact is provided by this fundamental institution in the science of medicine, and what a salutary control it exercises over the natural tendency of the human mind to dogmatize. In reference to operations may I, in passing, share the lament of

[1] Delivered at the Conjoint Meeting with the Roentgen Society, at the Radcliffe Infirmary, Oxford, July 5, 1924.

Sir James Mackenzie that hitherto so little has been done by way of systematic study when the living abdomen is opened and when the thoracic or when the cranial contents are exposed to view—proceedings that are becoming more and more common now that the surgery of the chest and skull is receiving more careful attention? But in the post-mortem study of disease the great element of *function* is missing; and observations during operations upon the living are necessarily very limited so long as the procedures are, as needs must, so rapid, and the surgeon, acting in the patient's best interest, guards so jealously the wound which constantly invites infection. Neither of these deterrents applies to the third process; the radiologist is able in many cases to study both structure and function, he has ample time for his observations, and he can submit these to such of his colleagues as are competent to offer suggestions and interpretations resulting from their own special experience of disease in other fields.

Against these obvious advantages we have to set the disadvantage that the radiologist's observations are much more difficult, and therefore his interpretations of what he sees are much more liable to error. He deals with shadows rather than with substances, and this fact alone places a serious limit, if not to the range of his methods, at least to the rate of his progress. Despite this and other obstacles, radiology is probably at the present time the branch of medical science which is most responsible for changing our views in regard to disease processes in a number of directions, confirming some important hypotheses, eliminating others, and suggesting new lines of thought. So far as time and your patience allow I will touch upon a few of these things.

Focal Sepsis.

One of the most helpful conceptions in modern times in connexion with the pathogenesis of disease is that known as *focal sepsis*. We may say with confidence that the doctrine of focal sepsis has passed out of the realm of mere hypothesis and has become, in many instances, an established fact. The various clinical manifestations which we group together under the generic term "fibrositis" form some of the most common expressions of focal sepsis. True it is, there is still a considerable divergence of opinion as to the relative degree to which the microbic and the metabolic factors operate in the production of this condition; but it is demonstrable again and again that in many instances of this morbid process there is close association with a septic focus, drainage of which leads to cure of the disease without any other form of treatment. Radiology has given great help in establishing the truth of this doctrine. In the diagnosis of sinus infection, for example, it has enabled us to visualize the morbid condition of an antrum or of some other accessory nasal cavity; in mastoid infection the same experience often obtains; whereas in connexion with dental sepsis it has become our chief stand-by for exact knowledge wherewith to establish the diagnosis. Bacteriological researches led us some fifteen years ago to the conception of chronic toxic processes set up by subinfection of certain tissues by streptococci of comparatively feeble virulence. We theorized about these processes, and were led to the idea that in some way or other these streptococci colonized in (amongst others) the periodontal tissues very frequently, and thus constituted a form of focal sepsis which gave rise to fibrositis as well as to several other pathological effects. Then came the revelations of dental radiography, and the progressively clearer and clearer views which we were given of the state of the

periodontal membrane. At once our theory was confirmed: we had ocular demonstration of a chronic, painless, sclerosing inflammation, without pus formation, yet capable of causing all manner of remote disturbances, sometimes as trivial as housemaid's knee and sometimes as lethal as ulcerating endocarditis. The specific focal lesion caused by salivary streptococci was exposed, and we found ourselves justified in having sent our patients to their dentists with a statement of our doubts, only to receive too often the familiar assurance that "there was no pyorrhœa." Such assurance nowadays gives us no more comfort of mind than it gives the patient comfort of body, for we know that teeth may be firm and gums dry, yet X-ray examination may reveal a thickened membrane and apical abscesses—adequate causes for toxæmia and a chain of resultant troubles. I may perhaps be forgiven for quoting from a paper which I read before the Section of Odontology in 1914, where I was dealing with this question[1]:—

"The pathological process going on at the root of the dead tooth, leading to sclerosis of the tissues hereabouts, is exactly similar to that which is taking place in the joint structures, the muscular fasciæ, and the sheaths of the nerve-trunks. There may be very little suppuration throughout all this pathological change accompanying the chronic streptococcal infection; quite often there is no suppuration at all. To the naked eye there is nothing to see that indicates infection, unless it be the dense fibrosis. All the same, a sterile platinum loop, applied to the adherent sac as the dental surgeon holds the tooth in his forceps, and bringing away a mere droplet of blood, as it seems, is capable of giving, when smeared over an agar slope, a copious and pure culture of streptococcus. This question of suppuration has been the bugbear of many practitioners for a long time past, and even now underlies many fallacies connected with our ideas of infective processes. 'No suppuration, no infection,' is a rough and ready rule, than which no graver error was ever conceived. There are two conditions in which, as modern bacteriology has taught us, suppuration may be quite absent, though micro-organisms, ordinarily pyogenic, are saturating the tissues with their toxins, and are directly leading to serious diseases: (*a*) The first condition is one in which the infection is a fulminating one, the micro-organism unusually virulent, and the tissues are overpowered before their resistance can show itself. (*b*) The second condition is one in which the micro-organism is one of feeble virulence, of long-standing action, and the tissue response to the prolonged infection is constructive rather than destructive. It is this latter condition which is present in so many cases of dead teeth, the roots of which are infected by *Streptococcus salivarius*."

Even to-day this old notion that infection, and even gross infection, together with severe toxæmia, cannot exist without suppuration, has not died out. The confirmation of the fact given by radiography has been overwhelming, and the influence of such confirmation has been wide spreading.

I need only mention the chronically inflamed appendix and the chronically inflamed gall-bladder as other instances of focal sepsis established frequently by radiography. In the case of the gall-bladder I have little doubt that by improvement in technique we shall soon be able to establish the diagnosis of chronic sepsis in addition to mere concretions.

Radiology of the Hollow Viscera.

Let me turn now to a very different subject. The influence of radiological investigations upon our knowledge of the anatomical relations and upon the motor functions of the *hollow viscera*, both in health and in disease, has been epoch-making. Physiology owes no less than pathology to these investigations; indeed, it is scarcely an exaggeration to say that the whole of our present con-

[1] *Proceedings*, 1914, vii (Sect. Odont.), p. 63.

ception of the neuro-muscular mechanism of the alimentary canal is the result of X-ray revelations. Granted that a study of the shadows cast by the opaque meal give us one aspect only of this complex mechanism, it still remains that this is the only precise and consistent set of visible phenomena at present available to us in this connexion. And the study of these phenomena has advanced and clarified our ideas to an astonishing degree. When we remember the notions we were taught, and were ourselves accustomed to teach, concerning the physiology of the stomach movements, for example, and what we now *know* as the result of studying the X-ray screen, the recollection almost brings the blush to the cheek. Whether we really had any exact knowledge at all on the matter I cannot now remember: no doubt that redoubtable Canadian, Alexis St. Martin, with his exposed stomach and its zealous observer, had much to answer for; the unfortunate incident was probably given a much heavier load than it could be expected reasonably to carry. To-day we stand upon much surer ground. The "lie" of the stomach and transverse colon, the position of the cæcum, the appendix, and the hepatic and splenic flexures are now matters of common knowledge. The variations, too, in the positions of these organs, variations which are not incompatible with good health, despite all the gloomy forebodings of the advocates of colo- and cæco-pexy—we are familiar with these, and they find us for the most part unconcerned, though the knowledge of them is apt to leave their owners much perturbed. We are passing through the stage of incrimination on the score of disease to that of tolerance on the score of variability in individuals, and we may yet arrive at the stage of indifference on the score of a changed conception of the standard of health—all as the result of greater familiarity with the facts as they steadily accumulate from large numbers of radiographic records. In other words, the same thing is happening with regard to our ideas about the hollow viscera as has happened with regard to our ideas about the kidneys. Even moderate degrees of nephroptosis were at one time regarded as signs of a morbid state, capable of explaining most of the functional disabilities met with in the neurasthenic, and not excluding, in the opinion of some, even grave forms of psychosis. The dropped organ was supported by pads, was "stitched up," was even removed. To-day none of these treatments is considered necessary; the most that we do is to advise some sort of general abdominal support. Enteroptosis, its significance and its treatment, would seem to be passing through the same phases. But just as a kidney may be fixed in an aberrant and in an awkward position, justifying surgical treatment, so, no doubt, a cæcum may be tied down in the pelvis, be incapable of emptying itself and may justify a surgical procedure to restore its function. But this difference—between mere ptosis from lack of tone and support on the one hand, and mechanical handicap from fixation on the other hand—constitutes the very essence of the matter, and it is a difference which radiography, and that alone, is able to distinguish in any given case.

But it is not only in the matter of the mere "lie" of the hollow viscera, and the presence or absence of fixation, that radiography has given us such clear ideas; a good many points connected with the motor function of these organs have also been established. I do not propose to deal in detail with the series of events that is now recognized as constituting the normal diorama of the passage of the opaque meal through the alimentary canal, though this is probably the most illuminating discovery of all. But I wish to refer to a few points in which radiography is still giving great assistance to our ideas and enabling us to understand more clearly the nature of some of the problems

presented to us by the patient. To put my conclusions first and discuss them briefly afterwards, I would say that X-ray investigations of the alimentary tract seem to me to indicate that although there is a general average of results which may conveniently be taken as representing normal functions, there are many variations within this general average to which it is very important not to attach too much pathological significance.

(1) These variations are as between individual and individual and they are probably part of the personal equations of the particular subjects examined, as much as are their facial expression, their gait, and their speech. But our experience of the posture and movements of the hollow viscera is a new experience—it is little more than half a generation old—whereas our experience of face and gait and speech is very old. No wonder that we lack unanimity in our views as to what is normal and what is abnormal. There being no rigid standard of health in this particular respect, it is not to be wondered at that our interpretations of what we see differ so much. Up till the present I doubt if we can say much more of large numbers of opaque meal skiagrams than that they are all of them representations of the alimentary tract of genus Homo, and that they do not reveal any evidence of gross structural defects.

How can we, by means of radiology, get further along this particular line? I suggest that one hopeful way is by departing somewhat from the fixed routine at present employed. There is surely room for some play of the imagination and some experimentation with the method. It was a great advance when the radiologist began to supplement his stereotyped methods of examination by varying the posture of his patient; it was a still greater advance when he began to combine palpation of the abdomen with the use of the fluorescent screen. Such combined methods were, I believe, almost resented by certain clinicians, who thought their own sphere of activity was being thereby invaded; but such resentment, though inevitable, was justly disregarded. I am not one of those who regard the radiologist as a mechanic, or as a photographer, though I am aware that some radiologists aim at little else. There is no reason why the radiologist should not be a clinician in spirit, indeed there is every reason why he should be, for to divorce clinical medicine and radiology is to impoverish both. This leads me to say that I think a closer co-operation between the clinician and the radiologist, and more exchange of views, are desirable factors in the advance of knowledge of the diagnosis and treatment of those diseases which have points of interest common to them both. Such co-operation might easily result in new methods of combined radiographic and clinical investigation as well as in more consistent results from radio-active treatment.

I have said that I think the ideal radiologist should be a clinician in spirit, and I have inferred at the beginning of my remarks that he is a pathologist in spirit also. But I think it very important that he should know the limits of his method, or rather that he should not strain his method past its possibilities in his interpretation of results. If he does this he is no longer giving us the valuable help of his technique and of his powers of observation —he is theorizing. Let me illustrate this by reading the conclusions of a report upon the opaque meal examination of a patient whom I saw a week ago. They read as follows:—

"The elongation of the pelvic colon and the kink of the iliac colon constitute the primary abnormalities in the alimentary tract. There is pronounced catarrh shown by increased sacculation of the cæcum, ascending and transverse colon, and by persistent narrowing from tonic spasm of the latter portions of the transverse colon and of the

descending, iliac, and pelvic colon. The appendix is also catarrhal. The ileo-cæcal valve is incompetent. The terminal coil of the ileum is hypertrophied. There is spasm at the ileo-cæcal entrance. There is delay in the stomach, but no organic disease or mechanical abnormalities of the stomach or duodenum.

"The severe catarrh of the large intestine and the iliac stasis due to spasm at the ileo-cæcal entrance lead to profound toxæmia."

Now I should be the last to put a limit to the radioscopic method of demonstrating anything, whether it be "catarrh" or "toxæmia," but I am not aware that, up to date, either of these processes is amenable to the opaque meal method. Though I say it with less assurance, I am doubtful if "hypertrophy" is any more demonstrable than "catarrh." No doubt the argument is sound enough if we add to the radioscopic evidence the collateral facts elicited by clinical and clinico-pathological methods. But not otherwise. If forbearance be not exercised in these things and the radiologist really "lets himself go" he is apt to bring his science into disrepute. I have received reports of a much more committal kind than the one I have just read. It is by no means uncommon to be told that a child has active and extensive pulmonary tuberculosis because the hilum shadows are conspicuous, and the glands obviously enlarged, after some febrile illness. I have even been rung up on the telephone, so intent was the radiographist to let me know the seriousness of such a situation. I was able to assure him that the boy was already convalescing rapidly at the seaside. I am, of course, alive to the fallacy in these remarks: that the child might be suffering from pulmonary tuberculosis after all. But experience must be the final guide in such cases, and experience cautions us very definitely not to make exact diagnoses in the absence of clinical data. Why need we? There is no necessary parallelism between change in structure and change in function, and the essence of disease is a functional rather than an anatomical change. Radioscopy itself has helped us greatly in this conception. How many instances we see in which there are quite extensive bony changes in the hip-joints and in the spine, but little or no pain or other disability. On the other hand, how severe may be the symptoms of hip-joint disease, without any demonstrable change seen in the radiograms. One patient may have a large collection of gall-stones and be in excellent health: another may have no actual concretions and yet be the subject of severe gall-bladder dyspepsia. These examples might easily be multiplied.

(2) To return to the question of the opaque meal and its help in establishing variations in the position and in the functions of the hollow viscera. Not only are these variations seen in skiagrams resulting from the examination of different healthy individuals, there are also variations, of no great moment considered pathologically, and yet of great physiological significance, in the same individual at different times and under different conditions. I have proof of this fact, and no doubt not a few others present have proof also. But as "repeat" investigations are not often undertaken the evidence is not so abundant as in the case of different individuals. Certain fortuitous observations, however, are pertinent. A radiological colleague of mine recently told me that he was on one occasion screening a nervous patient just after the opaque meal had been swallowed. It was before the time of the fixed screen. In moving this upwards to get a view of the cardiac orifice, the top inadvertently struck the patient under the chin. It was immediately noticed that the stomach, which had previously been observed to lie in a good position, and to be actively motile, dropped suddenly a distance of several inches, and

at the same time all active peristalsis ceased. This excellent demonstration of the sensitive response made by this organ to a sudden shock points a moral to the story that I am, by comparison, labouring in the telling. It is sometimes inevitable that, though no such physically painful stimulus as this takes place, the novel and somewhat alarming noises which occur during a radioscopic examination must have a somewhat similar effect. There is little doubt that when a patient says he has a sinking sensation in the abdomen the sensation is accompanied by a relative and temporary degree of gastroptosis; and I think it quite likely that, if we were able to screen a person during the play of various emotions, we should find that when his "heart was lifted up," and when his "heart sank within him," it is in reality not his heart but his stomach that changes its position in this manner, as the result of increased and of decreased tone respectively. I do not know if the cinematograph method has ever been applied to the transit of the opaque meal, but it is not at all unlikely that a series of pictures taken in this way would reveal many facts at present only guessed at, especially if the observations were made to cover some of the emotional states just referred to. If the stomach is so sensitive to emotions, to pain, and to reflex action through the special senses, it is not likely to be less sensitive to general fatigue. I think this may perhaps explain some of the discrepancies between the clinical examination of the stomach and the radiographic results. It was formerly not at all an uncommon experience of mine to conclude, as the result of my examination of a patient in the afternoon, that he had a dilated and atonic stomach, and to feel somewhat mystified when my radiological colleague demonstrated to me next morning that the organ was of a natural size and possessed of good motility. I now feel confident that it is the difference between afternoon and morning that accounts for the apparent disparity of the results, though I think that there are other factors which enter into the causation of the "splashing stomach" that is so common on the couch of our consulting rooms, and yet which so often finds no equivalent expression in the observations of the radiologist.

There are two or three further points in connexion with the important subject of enteroptosis where radiology has been illuminating in two senses of the word. One of these is the difference in level of the stomach, and especially of the colon, with posture. Radioscopic investigations certainly confirm the advice which we usually give patients suffering from this trouble to interrupt the erect posture of the greater part of the day by lying for a certain time in the semi-Trendelenburg position, for the difference of levels of the viscera in the two positions can be shown to be well worth the while. Again, examination during the wearing of a properly fitting support, such as special corsets which take their purchase from the hips and from the spine, proves that, in some cases at all events, the support really does assist in keeping the organs in better position. Lastly, it can be demonstrated that the effect of a walk which is not too fatiguing is to raise, and not to lower, the levels of the hollow viscera. For these demonstrations I am indebted to my friend, Dr. Dudley Stone, whose skiagrams I hope to show you at the close of these remarks.

Before leaving this matter of enteroptosis I wish to refer to one other point, partly because of its pathological interest and partly as a warning. It is common knowledge that we are often able, by a carefully devised programme of treatment properly carried out, to improve the health of the subjects of this condition very considerably, even to the extent of hearing them declare that they are "cured" of their symptoms. If now we examine them again radio-

scopically by the opaque meal method we find, in quite a number of cases, that the "lie" of the stomach and colon is practically unchanged. The moral is obvious: to the patients whose intelligence is able to hear the news we explain that it was not the mere ptosis of their organs that caused their discomforts and their general inefficiency, but the associated nervous asthenia, and even more (as I myself think probable) the congestive state of these same organs. To the majority of such patients, however, it is wiser to avoid the repetition of the examination altogether—not a very difficult situation to negotiate in actual practice in view of the present cost of a complete opaque meal investigation. The experience that I have just recorded serves well to confirm the principle that many diseased conditions are complexes made up of anatomical defects, together with nervous, vascular, metabolic, and infective associations. Radiology shows us one part of the picture, and a very important part—still, only one part.

In the history of its progress radiology must, of necessity, have first concerned itself with gross structural points, both normal and abnormal. It was essential to get certain rough criteria established. The method has now passed this stage in most regions of the body, though not in all. It is now contributing help of a more important, though more subtle, kind: it is helping us to study function, again both normal and abnormal. It is helping us to realize more vividly the fact that for a long time the patient does not suffer from diseases but from a tendency to disease; that later, when disease is established, the disfunction is not a fixed state, but an ebb and flow. I should like to make it clear that to me this is the kind of help that seems of vital importance. I doubt very much if those radiologists who are striving after "standards" may not be missing some more important things—I do not speak of physical standards, but of health standards. One such radiologist friend of mine with whom I was discussing a case of mutual interest said recently, "I wish we had a standard measurement for the breadth of the mediastinal shadow." Comment is unnecessary, but that is the outlook which I regard as likely to stultify some of the great possibilities of this science in the near future.

Radiology in Diseases of the Lymphatic Glands.

Upon our knowledge of diseases of the lymphatic glands radiology has thrown a flood of light. The discovery of enlarged nodes by X-ray methods was for a time such a revelation that it not only gave confirmation to the view that tuberculosis of those glands was a very common disease, it led many radiologists to give quite unwarranted and pessimistic opinions concerning the patients examined by them—an error of judgment that even to-day is by no means rare. If I may add that it is also by no means rare for patients to be given possession of their own or their children's radiograms, it is readily seen how easy it is to construct, quite unwittingly, situations full of apprehension and alarm, but which can be avoided by a little care and tact on the part of the radiologist. With improvement in technique, the frequency with which shadows are demonstrated in the regions where glands are known to enlarge—such as the roots of the lungs—becomes greater year by year. It is no longer necessary that a node shall be calcified, or even caseous, to throw a shadow by modern radiological methods. The standards of health are therefore, in this respect, changing. On the one hand we realize more clearly than we formerly did that there is an ebb and flow in the size of lymph glands in the thorax as there is in superficial parts of the body that are open to palpation; to some extent, therefore, the significance of demonstrating enlarged glands in the

former situation is not so serious as was once supposed. On the other hand radiology has opened our eyes to the fact that, in certain diseases of the lymph glands where we thought the lesions were confined to the superficial nodes, in many cases the intrathoracic nodes are enlarged also. An important example of this is lymphadenoma, where such a discovery at one and the same time increases the seriousness of our prognosis yet enables us to treat the disease much more promptly and effectively. In the not uncommon cases of this disease in which periodic pyrexia occurs, we were often quite in the dark as to where the specifically active process which led to the fever was going on. We watched the glands in the neck, axillæ, groins—nothing occurred. We suspected the visceral lymphoid tissue, and we therefore kept the spleen and liver under close observation—still we had no clue. A good X-ray picture solved the mystery by its revelation of the rise and fall in the breadth of the shadows in the hila of the lungs. No doubt in some of these cases we shall in the future, with improved methods, get similar demonstrations in regard to the abdominal lymph nodes in lymphadenoma. In the case of carcinoma of the breast a similar demonstration of the fact that the zone of lymphatic infiltration is more extensive than would otherwise be supposed is often given by radiological examination. And the significance of this finding is much the same as was seen to be the case in lymphadenoma: on the one hand our conception of carcinoma mammæ as a disease has changed in that we now know it to be often a more extensive affair than was formerly supposed, and, on the other hand, we are able to set against this serious consideration knowledge that leads to much earlier and therefore more effective treatment.

It is not only in relation to lymph nodes that our conception of carcinoma has changed as the result of radiological findings: the same remarks are applicable to the bones. We now know that osseous metastases are much more common than was thought to be the case before the era of radiology. The clinical picture presented by a woman who has undergone excision of the breast for cancer two, four, six, or even ten years ago, and who now complains of severe pain in the back, hips, or legs, due to cancerous infiltration of the bodies of the vertebræ, or of the pelvic bones, is all too familiar. Such cases were usually thought to be severe examples of fibrositis until one or more of the lesions led to local prominence or to spontaneous fracture. It is possible that such cases are more common to-day than formerly; it is even possible that they are relatively more common in patients who have had radical operations done for the primary growth; but it is certain that as the result of X-ray investigation we are able to diagnose them much earlier, and it is highly probable that many are brought to light to-day which would never be discovered at all. Further, it is not a very uncommon experience to find these tell-tale lesions overlooked in a good radiogram, even by the radiologist; and I think the reason is, either that he is not warned of the possibility of neoplastic disease, or because his attention is distracted from the infiltrations by some osteo-arthritic changes at the periphery of the vertebræ. I have recently dealt more fully with this subject of carcinomatosis in the Purvis Oration. I pointed out there that in not a few of the cases the appearances may be equivocal. For there is a condition now and then met with in carcinoma in which good radiograms reveal a state of diffuse rarefaction of several of the bones, the nature of which is difficult to determine, and which is almost certainly different from the more localized deposits that I have just referred to. I regard this change as part of the general toxic or nutritional disturbance that occurs in carcinoma, a disturbance which leads eventually to the so-called

malignant cachexia. It is sometimes asserted that malignant cachexia is due to secondary microbic infection of the growth; but this is probably only so in part; these bony changes go to show that there is something specific in the carcinomatous cachexia, for we have no knowledge of them in chronic pyogenic infections. I was interested to hear from my colleague, Dr. Gilbert Scott, that he had met with this same appearance, and that he felt sure it was not due to actual cancerous infiltration, however widespread this undoubtedly is at times. I purpose showing lantern slides, taken from skiagrams kindly lent to me by Dr. Gilbert Scott and by Dr. Robert Knox, which illustrate this condition. I find that this peculiar form of mollities has been noticed by one or two observers before, particularly by Roger Williams, but I have not come across any radiographic examples of it.

Radiotherapy.

Thus far I have dealt only with some of the ways in which radiology has influenced our ideas of disease by revealing to us changes in structure and function that we previously knew nothing about or merely guessed at. I turn now to a few examples of modifications in our views of disease resulting from the experience of radiotherapy. And it is a natural transition from the radiographic aspects of carcinoma to which I have just referred to the radiotherapeutics of malignant disease. The surgical treatment of malignant tumours has advanced very little during the past generation, and such advance as it has made is to be attributed almost entirely to advance in the means of diagnosis whereby operative procedures offer better hope of radical cure. The practice of aseptic methods once established and the importance of the anæsthetist's contribution to the success of the operation once fully recognized, the limits of the surgeon's possibilities in this direction seemed to be reached. Handley's important work upon the lymphatic spread of cancer of the breast is now just twenty years old, Stiles's and Halstead's classical papers advocating widespread removal of infected tissues are ten years older. The work of these men has not been materially altered up till to-day. Perhaps the only important advance in the operative treatment of carcinoma during the past two decades is the abdomino-perineal operation for cancer of the rectum, for the present improved technique of which credit is to be given in this country to W. E. Miles. If we turn to the medical side of the question, and ask what advances the physician has made towards the treatment of malignant disease, we are met almost with silence. The evidence that the injection of the heavy metals materially benefits the subject of malignant disease is, I think, very slight, although I consider that they have a definite value when used in conjunction with radio-active treatment. The fact that various sorts of serums and antigens, even including Coley's fluid, have completely fallen into disuse, goes to show that in this field nothing is being accomplished. Certain ancillary helps, it is true, the physician can give, both to the surgeon and to the radiologist, but I know of nothing he does, any more than the surgeon, in the direction of therapeutics which affects our conception of the neoplastic process.

If we now turn to the sphere of the radiologist, we find an entirely different state of things. I am not to-day concerned with the results, still less with the methods, of treatment of malignant growths by X-ray or by radium. But the bearing of such treatment upon our ideas of the course and even the pathogenesis of such growths is very close. It is fairly safe to predict that the many observations now in actual progress will very shortly yield important facts the knowledge of which will not only help to crystallize our ideas upon

some of the causative factors, but will also enable us to control our remedies more hopefully. One great change in our ideas about cancer, or so it seems to me, as the result of radio-active treatment, is the abolition of the notion that there is anything necessarily and inevitably progressive about the disease. Once we have seen—as which of us here has not?—a patient's general condition of health restored, and his growth greatly reduced in size, by radio-active treatment, we can never again look upon cancer in quite the same light. It is true that, with our present limited knowledge, working in the dark to a large extent as we are, such restoration to health, and such resorption of the tumour, are but of temporary duration. But my point is this—that in such cases we have a demonstration of the fact that *cancer is controllable*, using a word that I regard as more applicable to diseases in general and more scientific in its connotation than the more popular word "cure." If we consider the matter for a moment, do we *cure* tuberculosis? We do not, but we are perhaps getting the disease slowly under control. We have certainly added nothing specific to our means of "cure" in this disease; we are even using specific methods less and less. Syphilis is more definitely controlled to-day than twenty years ago, but there are many cases that still resist curative treatment; syphilis of the central nervous system we are still almost powerless to deal with by any of our present methods. Ulcerating endocarditis we neither cure nor even control, and of certain forms of rheumatic infection of the heart unfortunately the same thing may be said.

The senior surgeon of a large London hospital recently said publicly that he regarded the exploitation of radio-therapeutic methods as a menace to the effective treatment of cancer, asserting that he considered such efforts prevented patients from receiving the early benefits of radical surgical measures. Such an extraordinary indictment calls for some comment. If radiotherapy really did this I should agree with him. But of course it does not. No reputable radiologist dreams of denying a patient with an operable malignant tumour the advantages of surgery; in every case the surgeon has the "first call": and a firm conviction that in the treatment of malignant disease to remove the diseased organ cannot be more than a method *faute de mieux* does not, in the present state of our knowledge, cause us to swerve for an instant from this invariable rule of practice. Unfortunately there are all too many cases in which, even at the earliest moment of the diagnosis, modern surgical technique is is unavailable; the growth is so situated anatomically that it cannot be reached or is already disseminate. Appropriate material for the radio-therapist is therefore not lacking, though he is, by virtue of the rule already mentioned, denied the very sort of material with which he could probably do the best work. But even with this handicap it is possible to mark a steady change in the course of cancerous diseases as the result of the radiologist's efforts, and the outlook for sufferers from the dreaded scourge was never so hopeful as now. Many cancer patients live longer to-day, and live less painfully, both in body and mind, than ever before, and this is very largely on account of radio-active therapeutics.

Do these facts throw any clear light upon the ætiology of cancer? Perhaps not. But the more successful application of radiotherapy to lymphadenoma is to me significant in this connexion. There are many similarities between these two diseases. If, as I think is not unlikely, the lymphadenomas are proved to be caused by a group of protozoal parasites, then the increasing success that we find attending the combined treatment of these affections by arsenic and X-rays does seem to suggest that we should be premature in con-

sidering that cancer is not an infective disease as has been recently asserted, and asserted semi-officially, by some workers. But this is too large a question for the purpose of this lecture. Like many other questions connected with cancer, the answer can only come from concentrated labour and constructive thought. I am optimistic about combined methods of research—radiologist, clinician, and pathologist working in team fashion. Research combined with treatment is not popular with scientific advisory committees, but we must do our best to make it popular, and no doubt the more contributions it makes to knowledge the less unpopular will it become. "Why," said a colleague to me recently, "why do they call it research when *they* put radium into a mouse, but not when *we* put it into a man?" No answer seems possible unless it be that the radiologist, the clinician, and the histologist are not considered to be capable of observation or experiment. Let us demonstrate that this is not true, and let those who disburse public funds help us to explore this fertile ground of research combined with treatment. I have often remarked that every piece of treatment properly devised and carried out *is* a piece of research. I am no prophet, but I hazard a prediction that the effective control of cancer will come by other means than by a direct attack upon the essential *materies morbi*, even if it does not come before we know what that *materies morbi* is. And I think that research combined with radiotherapy will have been a conspicuous factor in bringing the control about.

I must not stay to deal with other diseases in which our conceptions have been influenced by the experience of radiotherapy. In the leukæmias, in exophthalmic goitre, in various skin diseases, and to a less degree in several other fields, our ideas about pathology have been modified. I think the modifications would have been even more striking if the work had been more thoroughly controlled by combined clinical and histological methods. But there are, as I have said, hopeful signs that such control will be sought and will be given in the future.

Conclusion.

I am all too well aware that in this address I have brought coals to Newcastle. For a physician to talk to radiologists about a subject so special as theirs is must lead of necessity to not a few bizarre expressions and demonstrations of his own ignorance. But the point of view of one who is an outsider in all matters of radiological technique, and yet an insider in all matters of pathological interest, may not be without its suggestiveness to your minds. And surely it is pathology (I use the word in its old and truest sense) that links together all our diverse interests. We must never lose sight of the essential *unity* of pathological processes. Though clinical medicine sees one aspect of a disease, and radiology another, and morbid anatomy a third, and parasitology a fourth, the disease is one and the same, and our best conception of it is obtained, not by undue insistence upon any one of these aspects, but by a proportionate mingling of the pictures presented by all. He who achieves the most complete conception of a disease in his mind is the most likely to envisage the appropriate treatment and get the best result. None of us can afford to neglect the facts elicited by the methods of others; indeed, our own methods will yield us more facts, and a clearer vision, in exact proportion as we allow them to be guided and controlled by the methods of others. It is of the utmost importance that we keep our minds alert and free from stereotyped ideas, yet bound always, fettered even, by the principles of science and reason, with a healthy scepticism withal—that saving grace so essential to the

scientific mind. Let us remember that our present notions of diseases, even when we think them very complete, are probably extremely crude. We have already ample experience that this is so. Not only are they crude in respect of the extent and the complexity of the disease process, as instanced earlier in this address, but in respect of the course diseases run and their response to our efforts at treatment. The problems of pathology become more elusive and yet more fascinating the closer we study them. It is a glorious contribution to such study that you are making to-day by means of radiology, both in its diagnostic and in its therapeutic values. The memory of him in whose name we meet together to-day is, I feel sure, being well served by you who follow him. I wish to congratulate you upon the fine work with which you are supplementing his and other pioneer achievements, and I shall have justified myself if anything I have said serves to stimulate you to further efforts and yet greater results.

At the Conjoint Meeting with the Roentgen Society held at the Radcliffe Infirmary, Oxford, July 5, 1924, Professor SIDNEY RUSS, D.Sc., read a paper on "Experimental Studies on the Lethal Doses of X-rays and Radium on Animal Tissues."

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The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

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Section of Epidemiology and State Medicine.

President—Dr. RICHARD J. REECE, C.B.

"Pestilence and Leechcraft in Ancient China."

By W. PERCEVAL YETTS, O.B.E., M.R.C.S., L.R.C.P.
(Ministry of Health).

(ABSTRACT.)

THE beginnings of Chinese medicine go back far beyond the range of authentic history. In fact, tradition connects them with the earliest legends of the race. The Chinese themselves have never supposed that their first forefathers migrated from abroad; nor is there, despite contrary theories, sufficient evidence to discredit the immemorial belief that the cradle of the nation was a territory represented by the modern provinces of Shensi and Kansu in the north-west of China. So we have to turn to China itself for the birth of the Chinese art of healing. Yet the literature of the subject is permeated with myth and legend, and the task of sifting fact from fancy is impossible. Accordingly the term "Ancient China" in the title of this paper is meant to include the shadowy past long before strictly historical times, and the period goes down to the fall of the Han Dynasty; in other words, the survey begins some 5,000 years ago and ends at the third century of our era.

The earliest mythical rulers of China figure in popular esteem as the culture-heroes of the race. As the first of these pioneers of civilization, the Emperor Fu Hsi naturally takes his place as one of the patron saints of medicine. His successor on the throne, Shên Nung, is more closely associated with the beginnings of the art of healing. He is chiefly noted as the originator of agriculture, a tradition indicated by his name, which may be rendered "Divine Husbandman." One of his activities was an investigation into the medicinal properties of plants, and there are legends of him trying them all on his own person till he had proved which were poisonous and which were useful for the treatment of disease. The results of his experiments are supposed to be recorded in a Herbal which is called after him. The third of the great culture-heroes is the Yellow Emperor, and most of the fundamental achievements of civilized man which are not attributed to the other two are assigned to him. Probably the oldest medical treatise extant is one connected with the Yellow Emperor, as its title denotes: *Huang Ti nei ching*. It may have taken written form several centuries B.C., after having passed in oral currency for unknown ages.

From the fabulous reigns of Fu Hsi, Shên Nung and the Yellow Emperor there is a stretch of some fifteen centuries before historical times are reached. During this long period the primitive patriarchal structure of society gradually

became organized into a feudal system. Men of sufficient worth were enfeoffed with domains, and in return they owed allegiance and rendered aid, when required, to the Son of Heaven who ruled over the dominant State. Thus China consisted of a large number of principalities, and, though each was independent except for its feudal obligations, the social fabric of the larger ones was based on that of the suzerain State.

At the end of the twelfth century B.C., the ducal house of Chou rose to imperial dignity. It is an important point in Chinese history. For one reason it marks the beginnings of a truly wonderful code regulating the machinery of Government. This book still exists under the title of "The Chou Ritual" (*Chou li*), and it enables us to study most minutely Chinese civilization during the last eight and a half centuries of the Feudal Period, i.e., till 246 B.C. More than that, it gives us insight into the national institutions even down to the establishment of the Republic, eleven years ago; for the standards laid down in it have always been venerated by the race as providing perfect models for guidance in official and social life. Though the authorship of the book is traditionally ascribed to the actual founder of the dynasty of Chou, who died in 1231, it is unlikely that he himself drew up more than its outline. Doubtless it received many additions and modifications during the long rule of the house of Chou.

The Chou Ritual contains a detailed account of the State medical service, comprising five departments, under the control of the Prime Minister or "Officer of Heaven." The first of these five was what we may perhaps term the Ministry of Health, presided over by a chief medical officer (*i-shih*) whose staff numbered thirty. His functions were to supervise medical practice and pharmacy throughout the State, and himself to treat the Emperor and high officials. At the end of each year he put his medical officers through an examination as to their success or failure in treating their patients, and he adjusted their rank (and presumably also their pay) accordingly.

Then there was a department, with a personnel of eight, charged with treating the maladies of the common people. Their functions seem to have been mainly clinical, but one of their duties was to send a certificate to the chief medical officer showing the cause whenever a death occurred.

Another separate medical department was that concerned with abscesses, ulcers and other septic conditions. Among the medicaments used by its members was a group called Five Poisons (*wu tu*) which appear to have been antiseptics of a rather drastic kind. They cannot be identified with certainty; but probably they included cinnabar, realgar, green vitriol, and loadstone. According to a commentator, they were heated together in an earthenware crucible for three days and nights. Fumes arising therefrom were caught on a bunch of feathers with which the lesion was brushed. Probably among the compounds deposited on the feathers were the sulphides of mercury and arsenic. Evidently the mixture was corrosive: for the commentator states that the septic tissues came away, leaving a healthy wound which afterwards was suitably dressed.

The fourth department was a small but important one. It was composed of two medical officers charged with supervision of the imperial dietary.

Last there comes the Veterinary Department, and it interests us chiefly because its members are recorded to have practised surgery to the extent of opening abscesses.

This elaborate State medical service, which existed during the Chou period from about 1100 to 300 B.C., had in its establishment no less than twenty-

eight fully qualified medical officers, if we include the four doctors of the Veterinary Department. It is to be noted that the organization belonged only to one of the many States composing China in those days. It was the dominant State over which the Son of Heaven reigned, and it served as model for the constitution of the other States. Hence some of them probably had medical services imitating that of the Chou. A computation of the entire population of China during the period is difficult; but a rough estimate puts it at two or three millions for 1000 B.C., and approximately at fourteen millions towards the end of the Feudal Period. A guess may therefore plausibly be made that the personnel of the official medical services of ancient China, in proportion to the population, far outnumbered the medical staffs of our Local Government Board or Ministry of Health.

In any account of the State medical organization of the Chou, mention of two other departments should not be omitted. They comprised the court wizards and the court exorcists. The former represented the world-wide primitive religion called Shamanism. The latter were the outcome of a general belief, not confined to China, attributing disease to the malevolent agency of demons. Official exorcists were provided to fight, destroy or scare away the evil spirits. Naturally they came under the control of the Minister for War. Though pestilence-hunting processions died out about the end of the ninth century as a State ceremonial, they still take place in some parts of the country, or at any rate they did so until recently.

Animistic beliefs often resulted in medical practice being mixed up with exorcism in some form. To give a few typical examples: therapeutic treatment was carried out by means of fire, swords, mirrors, charms and spells, or with medicines made from tigers, cocks, and peach trees.

What was the standard of qualification and status of medical men in ancient China? There can be little doubt that the healing art was learnt by apprenticeship and by the acquisition of approved prescriptions—methods of obtaining proficiency generally recognized in China up to the present day. One is forced to the conclusion that the status of a doctor in ancient China was a poor one; in fact, no better than it has been in less remote times. Confucius himself is reported to have spoken of doctors in a disdainful way. And there is an illuminating passage in the *Li chi* (a similar book to the *Chou li*) which classes doctors with a number of others "who professed particular arts for the service of their superiors." The list is as follows: "Prayer-makers, writers, archers, carriage-drivers, doctors, diviners, and artisans."

Allusions to doctors in the ancient classics are scanty, but scantier still are the records of epidemics and prevailing diseases. The earliest undoubted mention of an epidemic refers to one which occurred in 243 B.C. It does not specify the nature of the disease.

Some of the famous doctors of antiquity are next to be considered. A generally accepted tradition credits the Yellow Emperor with solicitude for the health of his subjects, and a great part of the most ancient medical treatise (*Huang Ti nei ching*) is concerned with the advice he sought of his medical advisers. Chief among these was Ch'i Po, who occupies in the Far East much the same position as Asclepius does in the West. At this point the close relationship between leechcraft and the cult of Tao should be emphasized. Speaking generally of Taoist adepts, it may be stated that they were alchemists, physicians and philosophers combined. A miraculous event, frequently recorded in the lives of noted physicians, is the acquisition from supernatural sources of magic drugs or books of secret remedies. In this way

Pien Ch'iao began his career as the great physician whose fame still flourishes after twenty-four centuries. The science of the pulse is generally said to have originated with him. It is in connexion with his achievements that we first hear of an operation performed under an anæsthetic. The instance is not historical, but the interesting fact remains that several centuries B.C. an anæsthetic was written about.

Some seven and a half centuries passed before another physician, equal in eminence to Pien Ch'iao, arose in China. Hua T'o was one of sixteen magicians and doctors who were attracted to the court of the rebel general Ts'ao Ts'ao at the end of the second century A.D. It is difficult to decide whether he or Pien Ch'iao better deserves to be called the Hippocrates of China. There is a circumstantial account in the dynastic annals of his time describing the use of an anæsthetic by Hua T'o in his surgical operations, and there seems no reason to doubt its trustworthiness. Probably hemp was the drug. The fame that came to Hua T'o following his cure of the general Kuan Yü's arrow wound, brought about his undoing. Ts'ao Ts'ao summoned him to treat his violent headaches. Hua T'o advised trephining under an anæsthetic. "What!" shouted the irascible old soldier, "you wish to kill me." "Your Highness will recall that I successfully scraped Kuan Yü's arm bone," replied the scared surgeon. "The brain is not the arm," retorted Ts'ao Ts'ao, and threw him into prison, where he died. The general himself died soon afterwards from what was probably a cerebral tumour.

The Chinese sciences of anatomy and physiology are not based on actual and accurate observations, but owe their existence chiefly to the ingenuity and imagination of theorists. Oriental training and tradition do not lead the mind into channels of scientific investigation such as we follow. Another factor is the firmly-rooted dislike of mutilating human bodies, and in fairness it should be stated that not till ten years ago did anatomical dissection receive governmental sanction.

On the subject of diagnosis it may be said that physical examination beyond that of the pulse has seldom been carried out.

The various forms of medical treatment employed in ancient times may be classed under the following four headings: drugs, diet, acupuncture and physical exercises. Many close parallels are to be found between Chinese pharmacology and ours. There is evidence that acupuncture and the cautery are very ancient arts. For the great bulk of the medical profession surgical treatment began and ended with acupuncture. An extensive literature is devoted to acupuncture, and no less than 368 spots on the body are recognized as suitable places for performing the operation.

Physical exercises or medical gymnastics go back to a very early date. Their origin has been traced to the legendary Emperor Yü who saved China from a great flood some 4,000 years ago. The story is that moisture and stagnant water were recognized as favouring endemic and epidemic maladies, and therefore, gymnastics and dances were instituted for the maintenance of health. At any rate a writer of the fourth century B.C. speaks of them as ancient practices.

A bibliography of Chinese medical literature would fill several large volumes. It is enough to mention that treatises on medicine were among those exempted from destruction at the burning of the books by imperial decree in 212 B.C. We find in the dynastic annals (*Ch'ien Han shu*), published about the beginning of our era, the titles of thirty-six works then

extant, and some of them were doubtless very ancient then. These facts are significant, because from them we gather that traditions of leechcraft have been handed down without interruption from the earliest times.

DISCUSSION.

Dr. R. J. REECE (President) said that all present must realize the great difficulty that faced Dr. Yetts when he attempted to condense into the confines of a paper that could be read before this Section an account of pestilence and leechcraft in Ancient China. The labour must have been very great and it would not be lessened by the fact that he had a wide and comprehensive knowledge of ancient Chinese literature which had enabled him to obtain his information directly from the original Chinese writings. Dr. Yetts had devoted much time and trouble to the task and his effort had been successful. Of course Dr. Yetts had only been able to place before them prominent points in the history of medicine in ancient China, and he (the President) hoped that on some future occasion Dr. Yetts would tell something more of what could be learnt of epidemics from ancient writings in that country.

The account given by Dr. Yetts showed that Fu Hsi and his sister Nü Wa occupied places in the mythology of Ancient China that were filled by well-known deities in the mythology of other countries; but in these, three deities were united. In the ancient Egyptian mythology we found Osiris, the creator, the father; Isis, the preserver, the mother, and Horus, the destroyer of Set, the son. In India these were represented by Brahma, the creator, the father; Vishnu, the preserver, the mother, and Shiva, the transformer, the son. One wondered whether in ancient China there was a third deity associated with the other two.

The comprehensive division of medical work laid down by the State in the Chou Ritual was interesting in showing the importance attached to medical matters. The first division could be compared with State medicine or public health as it was at present known. The notification of the causes of death would signify that information as to where and why deaths occurred was required probably with a view to ascertaining the influences that led to any undue mortality.

The gradation of medical practice was also of interest. Apparently in the lowest grade were those who were allowed to open abscesses. This differentiation was seen to be carried through the ages. It appeared in the history of ancient Greek medicine, whence it has been probably handed on to our country and it was in evidence until quite recent years. In 1633 when William Harvey, who first described the circulation of the blood, was physician to St. Bartholomew's Hospital, there were sixteen regulations as to the admission and treatment of patients and six of these defined the relation of the physician or doctor to the surgeon. The surgeon was not to give inward physic, to trepan the head, pierce the body, dismember or do any great operation on the body of any, but with the approbation and by the direction of the doctor, and he was required to declare to the doctor in the presence of the patient, "what he findeth and what he useth to every external malady." There was a note added: "The chirurgeons protest against this."

That in Ancient China doctors of the third generation were considered to have had transmitted to them from their ancestors special qualities which entitled them to a superior position in their profession, was a practical application of the doctrine of heredity which might appeal to several of us.

In many parts of the world there was evidence that from time immemorial building tools and implements had been used to point a moral, and ancient China furnished no exception to this. In the "Book of History" (*Shu ching*) there was the sentence: "Ye officers of the Government apply the compasses." In the "Great Learning," 500 B.C., it was written: "A man should abstain from doing unto others what he would not they should do unto him and this is called the principle of acting on the square." In the works of Mencius, a latinized form of Mêng Tzŭ, who lived 372-289 B.C., and who studied under the grandson of Confucius, it was written "that men should apply the square and compasses morally to their lives, and the level and marking line besides, if they would walk in the straight and even path of wisdom

and keep themselves within the bounds of honour and virtue." Also in the sixth book of his philosophy he wrote: "A master mason in teaching his apprentices makes use of the compasses and square. We who are engaged in the pursuit of wisdom must also make use of the compasses and square." It was of interest to know that in Ancient China there were in use implements that were commonly employed to-day in building operations; and that among the educated, an allegorical or symbolic meaning was attached to them.

Dr. Yetts had said that the goblin who was specially connected with pestilence had his abode in the water of rivers; this raised the question whether the ancient Chinese associated the pollution of river-water with pestilence. The fact that 2,600 years B.C. the decadence of society was to be deplored might encourage us not to be too pessimistic concerning the society of the present day.

His Excellency CHAO-HSIN CHU (Chinese Minister) said that he was very pleased not only at being present that night on the occasion of such an instructive lecture as that to which they had just listened, but also at being able to avail himself of the opportunity of taking part in the discussion on such an interesting subject pertaining to a country which he had the honour to represent. It was, indeed, very interesting to hear that the lecturer traced back his investigations to the medicinal history of China 5,000 years ago. Chinese medicine was an art rather than a science in the old days. The ancient doctors as named by the lecturer, particularly Pien Ch'iao and Hua T'o, succeeded in curing their patients in an extraordinary way. The former was a doctor for every disease. History stated that when he travelled to Han-tan, where women were considered superior, he advertised himself as a specialist in the treatment of severe maladies affecting women. He afterwards went to Lo-yang, where aged folk were particularly respected; he made himself known there as an ophthalmic and aural doctor. Finally he moved to Hsien-yang, where children received special attention, he devoted himself to specializing in respect of children's ailments. An Imperial doctor was jealous of him and arranged for someone to assassinate him.

With regard to Hua T'o, he (Mr. Chao-Hsin Chu) would tell the story a little differently from that as told by Dr. Yetts. According to the dynastic annals of Hou-Han, Hua T'o was engaged by Ts'ao Ts'ao to attend to him, to treat him for violent headaches. Hua T'o could not get away from such attendance for a long time. One day he was granted leave to go home for a prescription. On his arrival at home he found his wife indisposed and he had to attend to her, and was detained several days. Ts'ao Ts'ao summoned him by several letters, but he stubbornly delayed his return. This made Ts'ao Ts'ao very angry, and Hua T'o was sent to jail and executed. Before he died he handed a medical book called "Green-Pocket Classic" as a present to the jailer, who dared not accept it and it was put into the fire.

From the incidents just related it would be seen that difficulties were indeed experienced in the old days in carrying on continuously the art of healing and of medical prescription. The Chinese learnt to be doctors by apprenticeship or trained themselves by reading books. However, the methods of curing various diseases had been developed in China and used for thousands of years, and hence there must be something good and worth while studying. In that connexion he would call attention to a few special points. In the first place, Chinese medicine was mainly made up of herbs. Experiments with hundreds of herbs had proved useful and a Herbal written by Shên Nung, and the subsequent discoveries from century to century in connexion therewith and on other lines, should be expounded and made known to the medical world of the West.

In the second place, acupuncture and cautery were very ancient arts in China, as Dr. Yetts had recognized. Under the old régime, the Imperial doctors were trained to locate the 368 spots on the body, each spot bearing a particular name. A piece of copper was prepared so as to contain all the little spots existing on the body, which was completely covered by paper. The student was trained to find out each spot, and he threw a needle into it from a little distance. He was not allowed to make a single mistake, and yet the space between the spots varied according to the proportions of different bodies. When a student had finished the course he was qualified to be admitted to the Imperial surgery.

Some diseases required the treatment by cautery on certain spots of the body, just as effectively as by acupuncture. Both these ancient arts were still largely practised in China, particularly in the northern provinces. He (Mr. Chao-Hsin Chu) was once personally cured by acupuncture carried out by a doctor, and he had witnessed hundreds of cases which had been successfully treated by acupuncture and cautery.

In the third place, the Chinese method of distinguishing various diseases developed internally by examining the pulse was an ancient practice, and this was still carried out by the native doctors. A distinguished medical scholar, Chang Chung-ching, of the Han Dynasty, wrote a thesis known as "Maladies from Cold," which contained thirty-two chapters. The precepts enjoined in this masterpiece were still carefully and widely observed by the Chinese medical students, who believed in pulse examination being practical and effective.

The Chinese were very cautious about taking drugs as to the effects of which they were not very sure. Confucius taught: "Do not attempt to take medicine which is prescribed by a doctor who was not trained on the experience of at least three generations." He (Mr. Chao-Hsin Chu) had, from personal experience, great confidence in Chinese medicine. What the lecturer had told them, therefore, met with his whole-hearted approval, and he would recommend Western doctors to take an interest in the medical development of the East.

Dr. MILLAIS CULPIN (speaking with a superficial knowledge of China and Chinese medicine) said that Dr. Yetts had made an excellent use of his opportunities. It was not easy to tell how much clinical observation was embodied in a form at first sight fantastic. It was possible that the demonology of ancient Chinese medicine veiled the results of clinical observation. He (Dr. Culpin) had suspected an educated Chinese of expressing bacteriological knowledge, acquired from foreign books, in terms of "devils of sickness." In spite of seemingly absurd theories the Chinese practitioner was able to recognize the pulse of high tension and give a prognosis accordingly. In his (the speaker's) experience the case reports of a native practitioner, though couched in strange terms, sometimes enabled one to reach a probable diagnosis of the nature and course of the disease. Acupuncture was employed in cases where benefit could not be expected—the veins of the arm were needled for cholera, and a strangulated hernia was made the object of multiple punctures. On the other hand, the vigorous needling of an enlarged liver, due to a failing heart, seemed a reasonable measure; he (Dr. Culpin) had carried it out, with at least no harmful results, in the case of a patient who suggested that it should be done.

Sir E. DENISON ROSS (Director of the School of Oriental Studies, London Institution) emphasized the importance attaching to the study of Oriental literatures at first hand, notably those in Chinese and Arabic, by members of the medical profession. He said that a valuable contribution to such study had been made by Professor Browne, of Cambridge, who in addition to being Professor of Arabic, had also taken his medical degree. He also pointed out that Arabic writers on medical subjects, like the Chinese, had arrived at conclusions which were of great interest to modern men of science, and that a debt of gratitude was owing to the Arabs for having kept alive the best products of Greek medical science during the long interval which separated the epoch of Greek learning from the Renaissance.

Section of Epidemiology and State Medicine.

President—Dr. RICHARD J. REECE, C.B.

Some Considerations on the Use of Hydrogen Cyanide and Allied Substances with Special Reference to International Preventive Medicine.

By P. G. STOCK, C.B., M.B., Hon. F.R.C.S.(Ed.)

SINCE the recognition of the fact that the infection of plague is propagated and carried more by rats than by man, the efforts of sanitarians all over the world have been more and more directed to the prevention of this rodent-borne infection.

The danger from rats to international public health and the guiding principle for dealing with it were aptly expressed by Dr. Calmette when he submitted the conclusions of a Sub-Committee, who considered the question, to the Office International d'Hygiène Publique.

He then stated:—

"The embarkation on board a ship of plague-infected rats, coming either from a port infected with plague or from a port where there exists an unsuspected rat epidemic, constitutes the principal danger in the propagation of plague.

"All measures tending to reduce in a permanent fashion the rat population on board ship and in ports either infected with or free from plague and also in land areas exposed to epidemics of plague, should be considered as of a nature to form the most efficacious obstacle of the diffusion of the disease."

Up to a point these principles were recognized by the International Sanitary Convention of 1903, and the destruction of rats on board "infected" ships was made compulsory. The actual wording of Article 21 (6) of this Convention is as follows:—

"In the case of *plague infected* ships . . . the rats on board *must* be destroyed either before or after discharge of cargo, as quickly as possible, and in any case, within a maximum time of forty-eight hours and so as to avoid damage to merchandise and to ship's plating and engines. . . ."

In the case of plague "*suspected*" ships the destruction of rats on board was merely recommended; but in the case of "*healthy*" ships, while they had to be given free pratique immediately, the sanitary authority might subject such ships, *if from an infected port*, to a process intended to secure destruction of rats on board, either before or after the discharge of cargo, although this measure could not be resorted to as a general rule and in no case was it to take longer than twenty-four hours.

In order to estimate the value of these requirements, the definitions adopted in the Convention of 1903 in regard to the classification of ships have to be borne in mind. Under Article 20:—

"A ship shall be regarded as '*infected*' if there is plague . . . on board or if there have been one or more cases of plague . . . on board within seven days.

"A ship shall be regarded as '*suspected*' if there have been cases of plague . . . on board at the time of departure or during the voyage, but no fresh case within seven days.

"A ship shall be regarded as '*healthy*,' *notwithstanding its having come from an infected port*, if there has been no death from nor case of plague . . . on board either before departure or during the voyage or on arrival."

When the International Sanitary Convention was revised in 1911, the classification of vessels adopted in 1903 was maintained. Proposals, however, were made to impose a compulsory system of periodic rat destruction on all ships and even to extend the same system to ports. These proposals were not accepted by the Conference assembled to consider the revision of the Convention. But, in the new Convention which was then drawn up and which is the Convention now in force, rat destruction on "suspected" ships was made compulsory instead of being merely recommended. A new clause was also added in the Convention of 1911 *recommending* that all ships should be periodically subjected to a process of rat destruction at least once in every six months and that preferential treatment should be accorded in ports of arrival to ships which had undergone the process.

Before the War the periodic fumigation of ships for rat destruction was becoming a usual practice throughout the world, but under war conditions it fell into abeyance. As a result, and aided also no doubt by the conditions prevailing during the latter years of the War, most observers were agreed that by the time of the Armistice it was usual to find the ordinary mercantile vessel heavily infested with rats. Outbreaks of plague amongst the rats and humans on board ships also appeared to be on the increase. It is not surprising, therefore, that when, in 1921 and 1922, the Office International d'Hygiène Publique considered the draft revision of the International Sanitary Convention of 1912, a clause should have been agreed to, making it *compulsory* for *all* ships —except those employed in coastwise service—to be periodically de-ratized at least once in six months.

A new article was also drafted under which governments will undertake to make use of every possible means to diminish the danger from rats, and to keep themselves as well as the Office International d'Hygiène Publique regularly informed of the condition of the rats in their ports as regards plague infection.

For various reasons, however, it has not yet been possible to assemble the formal international conference which will have to adopt or amend the draft revisions now ready.

In the meantime the United States, by their Quarantine Regulations of 1920, had imposed as a definite requirement that all vessels engaged in trade with foreign ports should be fumigated not less than once every six months for the purpose of destroying rats. The regulations also stipulated that vessels should be fumigated *simultaneously* in all parts, and laid down the gaseous agents which were authorized. The regulations recognized that the oxides of carbon are efficient for the destruction of rats, but as they do not kill fleas or insects, these gases are practically excluded, and reliance is placed on sulphur dioxide or hydrogen cyanide—more popularly spoken of as hydrocyanic acid

gas. An amendment (No. 6) published last January, adds cyanogen chloride gas mixture to the list of authorized agents, and its use will be referred to later.

In thus defining how the de-ratization was to be carried out, the United States went beyond the strict, though perhaps not the logical, interpretation of the convention. When they insisted, however, early in 1922, that even the first and second class passenger accommodation of the largest liners should be included in the fumigation, shipowners were faced with the alternatives of

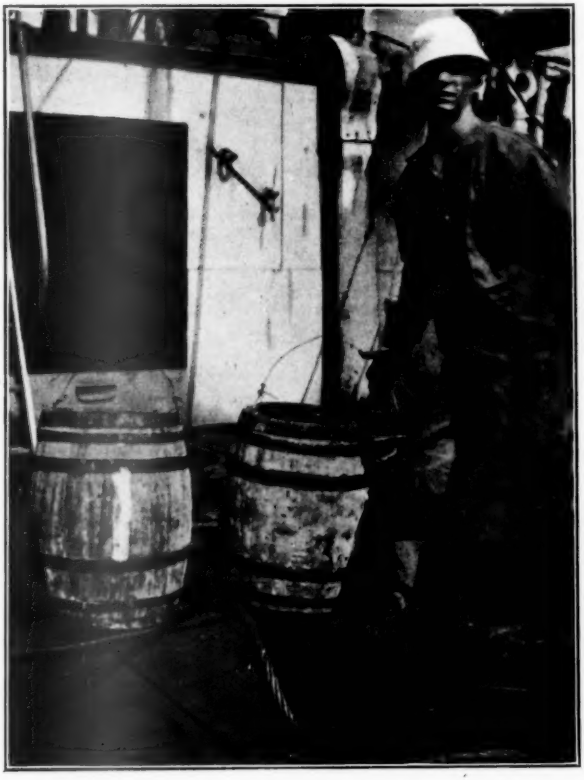


FIG. 1.

complying with the regulations or of having their vessels quarantined on arrival in American ports—with the consequent enormous loss on "demurrage." To fumigate the passenger accommodation with sulphur dioxide gas was held to be out of the question.

Apart from the *cost* of making good the damage which might ensue, the time which reconditioning would take was considered to render the use of this gas impracticable. The practical alternative was to use hydrogen cyanide,

which does not damage metal work, paint or fittings. Now the use of hydrogen cyanide for fumigation purposes is not a new process. For the last twenty-five years it has been used for the destruction of vermin in railway carriages in South Africa, and even in 1910 it was authorized as a fumigant in the United States quarantine regulations.

The great disadvantages of course are the deadly nature of the gas and the difficulty of enforcing safety regulations. Owing to its efficacy and quickness, however, the method was introduced into Germany during the war to fumigate flour mills which had become infested with weevil, and owing to the presence



FIG. 2.

of plague in the Mediterranean, it was used in Italy to fumigate vessels which had to be treated as rapidly as possible on account of the shortage of tonnage.

It will now be convenient to refer to the main points in connexion with the generation and use of this gas. If required on a large scale it is usually generated by adding sodium cyanide, either in solid form or in solution, to a mixture of sulphuric acid and water held in a container placed either in the compartment to be fumigated or, as in the case of the Glen Liston apparatus, in a machine placed in the open air from which the gas passes through a pipe into the space to be dealt with.

If *solid* sodium cyanide is used the correct amount may be placed in bags, which slightly retard the commencement of the reaction, and the bags are either dropped by hand, or lowered by a string into the mixture of acid and water. Another method is to place the cyanide in some form of tipping device which can be worked from a distance.

In the United States the fumigation of vessels is carried out by the Federal Quarantine Service and the method of "dumping" or lowering the solid cyanide into a container holding the sulphuric acid and water is the one most usually followed.

Wooden barrels are used to contain the sulphuric acid and water and a type of barrel commonly used is shown in the photograph (fig. 1, p. 11).



FIG. 3.

Such barrels are fitted with wire loops to which a rope can be attached if they have to be lowered into the holds.

In the next photograph (fig. 2) a member of the fumigating staff is seen pouring sulphuric acid from a jar into a barrel. The barrel itself cannot be seen but is suspended just below the hatchway combing by the rope shown in the photograph.

In the next photograph (fig. 3) a fumigator is shown ready to lower into a barrel in the hold a charge of cyanide contained in the bag.

If the vessel is lying alongside the quay the fumigating staff bring their apparatus alongside on one of their motor lorries (figs. 4 and 5, p. 14). If lying in the stream one of the launches, specially fitted for fumigating work, belonging to the Quarantine Service, is employed



FIG. 4.



FIG. 5.

Creel, in a paper published in 1915, described a device for tipping the cyanide into the acid, where the escape of the operator after introducing the cyanide by hand was considered impracticable. This method will be more readily followed from the sketch now shown.

A is an ordinary wooden cask which serves as a container for the acid and water. B and C are five-gallon tins with the tops removed, and fitted with pin hinges which fit similar hinges on the sides of D. The tins hold the cyanide. D is a funnel of galvanized iron and was introduced chiefly to prevent splashing.

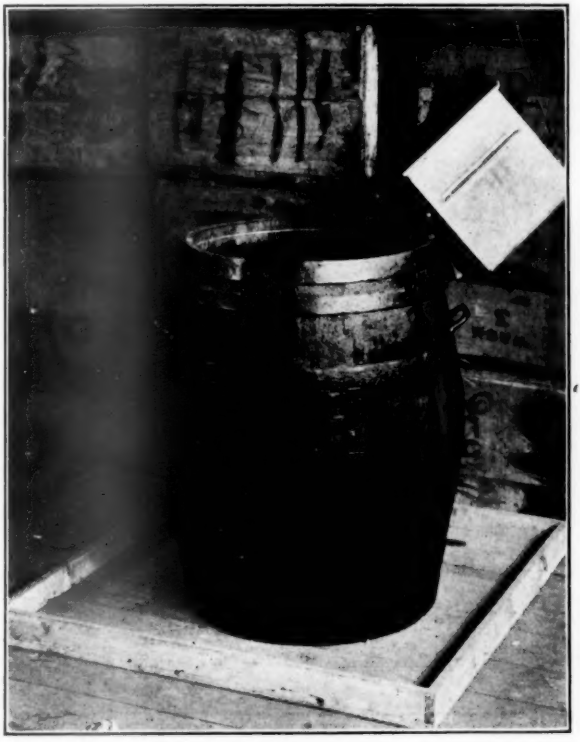


FIG. 6.

The plan is to place the water and acid in the barrel and then lower the barrel into the hold of the ship. When the barrels are in position the funnel is placed on the top of the barrel and the tins containing the cyanide attached. Ropes are then fixed to the bottom of the tins and passed into the open air. By means of pulling these ropes the cyanide is dumped into the barrel. The next photographs (figs. 6 and 7) which have been kindly supplied by Messrs. Siebe, Gorman and Co., show the type of barrel which has been used in this country. It is larger and heavier than the barrels used in America, but most of the

vessels fumigated in England have been vastly larger than the average vessel fumigated, say in New York. The depth of the holds has also usually been so great that wires in place of ropes have had to be used to tip the containers.

In the *solution* method, instead of adding the solid sodium cyanide to the mixture of acid and water, a solution of the salt is run in. The barrels containing the acid and water are arranged in the same way, but, in place of a "tipper," a rubber tube is provided one end of which discharges into the barrel, the other end being carried into the open air and fitted with a funnel down

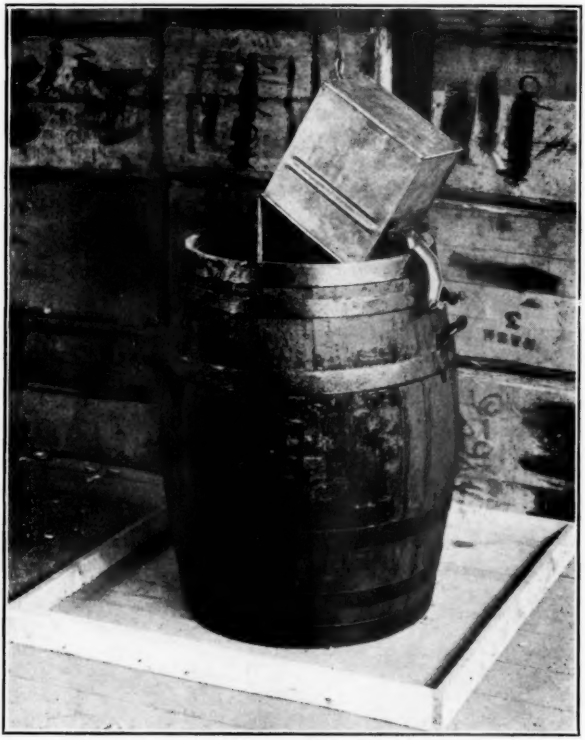


FIG. 7.

which the solution of cyanide is poured. The arrangement of the lower end of the tubes in the barrels is shown in the slide.

The next six photographs were taken on the deck of the s.s. *Aquitania* a vessel of some 46,000 odd tons gross register. They show the upper end of the tubes, the type of funnel employed and the actual operation of pouring the solution of cyanide.

In the photograph (fig. 8), an operator is seen pouring the solution into a single funnel. In the photograph (fig. 9) a battery of funnels on for'ard "well"

deck is shown. The operation of pouring is in progress and the pipes from funnels can be seen passing under tarpaulin at the corner of the hatchway.

Both methods have the disadvantage of requiring a considerable amount of time and labour to place the apparatus in position and to remove it after the fumigation is over. As far as ships are concerned, several marine superintendents with whom the question has been discussed would prefer to have the gas generated outside the ship and then passed, by means of a pipe, into the space to be dealt with. This principle is followed in the Glen Liston apparatus and in the generator designed by Dr. Grubbs, the medical officer in charge of the Quarantine Station at New York. Glen Liston's method is so



FIG. 8.

well known that any detailed description is unnecessary. The principle is to generate the gas in a machine placed in the open air and to circulate it by means of pipes through the compartment to be fumigated. The mixture of air and gas in the compartment is continuously circulated through the machine and gas is added to the air until the desired concentration has been obtained. In the next slide the apparatus is seen at work, first, from a distance looking down the side of the ship, and secondly, close up.

Each machine may be regarded as a unit capable of fumigating at one time an air space of about 60,000 cubic feet.

The Grubbs generator (fig. 10) consists of two iron chambers, one above the other, lined with lead and fixed on a metal tray. Owing to the weight of

the apparatus and the difficulties in handling, it can only be transported on the deck of a boat, adapted to the purpose. The slide shows the generator in position on the aft deck of one of the New York quarantine tugs. To operate this generator the correct amount of acid and water is first poured into the lower chambers. The valve between the upper and lower chambers is closed. The requisite amount of cyanide is then placed in the upper chamber and the lid clamped down. When everything is ready the charge is fired by opening the valve and letting the cyanide fall into the acid. The gas is forced by the pressure in the generator along a 1-in. armoured rubber hose-pipe into the

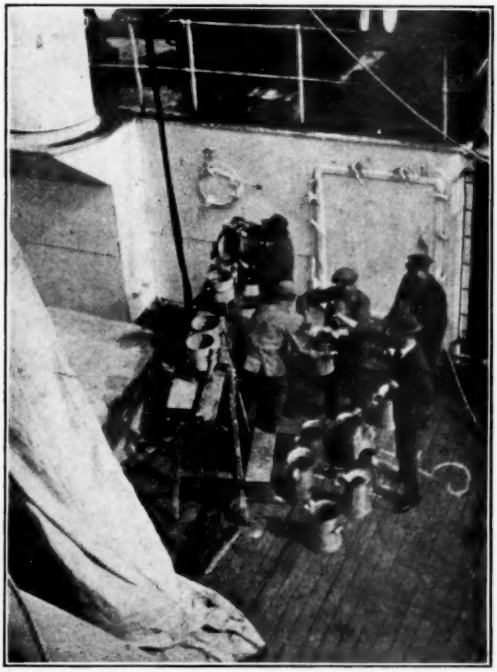


FIG. 9.

compartment to be fumigated. It is doubtful whether this form of generator has proved as satisfactory in practice as was hoped. Unfortunately there appears to be a limit to the size of the delivery-pipes, for if they are made too large the American experience is that difficulties in handling are so great as to render the use of barrels preferable.

Glen Liston in his machine aims at controlling the percentage of gas in the compartment under treatment, and his machines have been used most successfully in India for dealing with houses. He contends that when the ordinary dumping method is used the gas is evolved with almost explosive rapidity,

that high, and therefore dangerous, concentrations are developed and that the gas is completely out of control. These contentions require further investigation, but there is no doubt that this type of apparatus offers many advantages. The disadvantages attaching to their use—say, on a big ship—would resolve themselves largely into questions of transport and the difficulties of handling the pipes.

Liquid Hydrogen Cyanide.

Many of the troubles which arise in connexion with the handling of barrels, tipping devices, pipes, number of personnel required, &c., hardly occur when liquid hydrogen cyanide is used. This method is now being employed in this

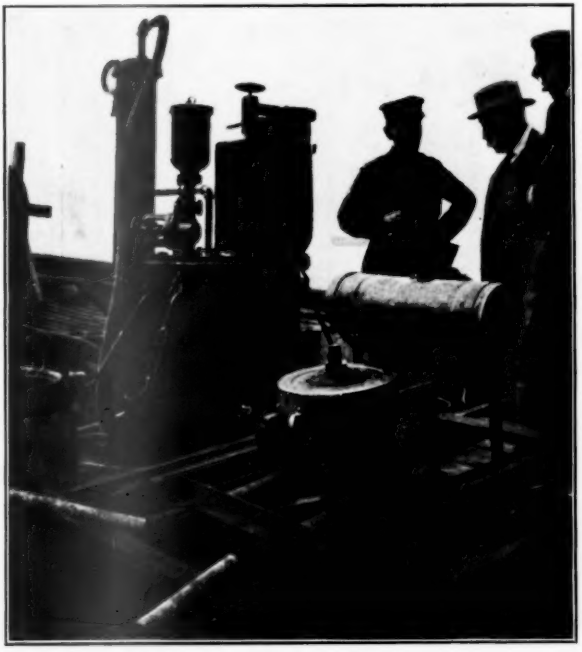


Fig. 10.

country by the Mining Engineering Company of Sheffield, a firm undertaking the fumigation of ships.

The liquid hydrogen cyanide is obtained from America in steel cylinders, and the next slide shows one of the cylinders being hoisted on board a ship in the London docks. The cylinder is placed in some convenient position and portable metal containers which are used for distributing the liquid are then filled from the cylinder. The next slide (fig. 11, p. 20) shows the operation of filling a container in progress. In the next slide (fig. 12, p. 21) the container is seen with the distributing spray attached, filled and ready for use. In the photograph (fig. 13, p. 21) the actual progress of fumigation is seen taking place. The

procedure is extremely simple. A corner of the tarpaulin covering a hatch is turned back, the spray is inserted and the liquid HCN is pumped out of the container with what is equivalent to an ordinary tyre pump. Another view is shown in the next slide.

If sufficient of the tarpaulin is turned back to enable the whole process to be watched, the fluid is seen issuing from the nozzles of the spraying pipe and then disappearing as a cloud of bluish-grey smoke which appears to sink to the bottom of the hold. As soon as the correct amount of liquid has been distributed the spray is removed, and there is neither any cumbersome



FIG. 11.

apparatus nor the residue left in "containers" to be dealt with after the fumigation is completed. The great drawback to the use of the method consists in the difficulties in connexion with the transportation of the big iron cylinders. In this connexion it may be of interest to mention that should a cylinder crack and the liquid be spilt in any position where it cannot immediately be flushed away with large quantities of water, it can be rendered innocuous by setting it alight. It burns with a bluish flame, and apart from the possible danger of fire the products of combustion are harmless.

The hazard in connexion with the use of hydrogen cyanide is due chiefly to



FIG. 12.



FIG. 13.

the very faint odour which the gas possesses and the absence of warning that a poisonous gas is present. From time to time consideration has been given to the use of some mixture, which, by reason of its odour or other physical property, would give adequate warning to persons entering a space which still retained cyanide gas in lethal strength.

Two such substances are now in use. It is claimed that they possess all the advantages of hydrogen cyanide, and are highly lachrymatory even in non-lethal concentrations. The first is known under the trade name of cyklon. It is a liquid which somewhat resembles liquid hydrogen cyanide, but is believed

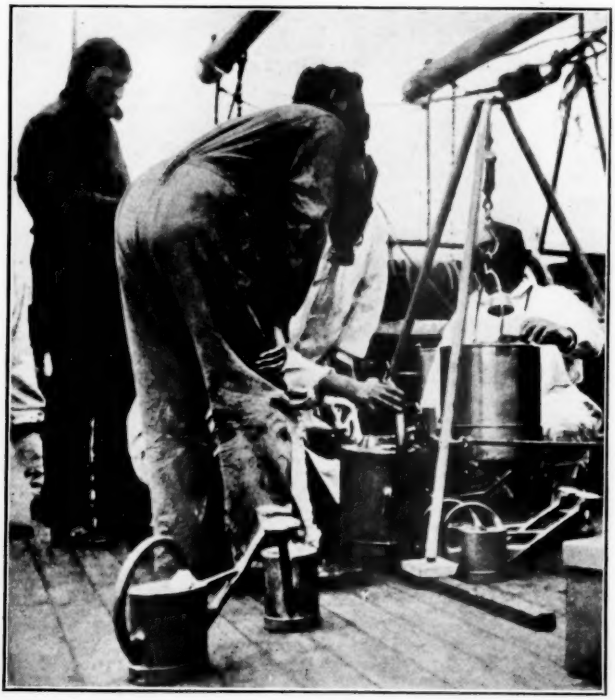


FIG. 14.

to consist of methyl cyano-formate 90 per cent. and methyl chloro-formate 10 per cent. It volatilizes rapidly if sprayed into the air, the methyl chloro-formate turning into an intensely irritating vapour which is said to give effective warning before the concentration of methyl cyano-formate has reached dangerous proportions. For fumigation purposes it may be distributed, as liquid hydrogen cyanide is, by means of a spray, or it may be sprinkled, in the compartment to be treated, by means of specially constructed "watering" cans. The next slide shows a sketch of one of these cans, and the next (fig. 14) shows the cans being filled on the deck of a vessel which is about

to be fumigated. Cyklon is very highly spoken of by the fumigating authorities at Rotterdam, but the question of cost and availability of supplies will have much to do with its wider use.

The other compound—cyanogen chloride gas mixture—is claimed to be one of the most penetrating and toxic of all fumigants. It is generated on similar lines to the preparation of ordinary hydrogen cyanide by adding sodium cyanide, sodium chloride and talc to a mixture of hydrochloric acid and water. This fumigant is highly commended in the United States, and there is no question as to the effectiveness of the warning. It is intensely lachrymatory,

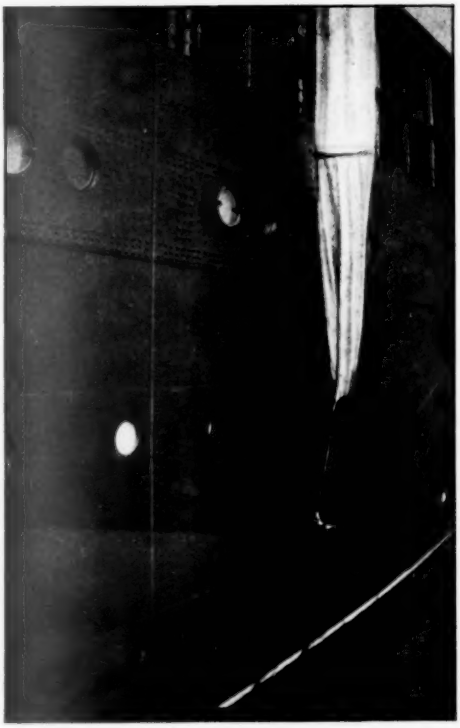


FIG. 15.

and it would be impossible for any human being, unless adequately protected with a mask, to penetrate any distance into an atmosphere charged with the gas. While this gas is stated to have no effect on paint work, it is believed, unfortunately, to have an effect on nickel, but sufficient experience is not available as to whether this action will negative its use for the treatment of first-class accommodation on passenger vessels.

It should also be remembered that one of the advantages attributed to hydrogen cyanide has been the absence of warning afforded to rats and other

vermin, and fears have been expressed that a lacrymal gas will lose much of this value. Whether under conditions of practical fumigation there is any real justification for these fears remains to be seen.

The next photograph illustrates an early stage in the process of ventilation. It was taken on the upper deck of a big transatlantic liner, whilst the temporary covering on one of the ventilators was being broken away in order to facilitate the escape of the gas from the deck below.

In the next (fig. 15, p. 23) a canvas wind-chute is seen rigged in position on the s.s. *Mauretania* before the fumigation has been begun. The tube for pouring the solution passing down the side of the vessel should also be noted.

In the United States great reliance is placed on artificial means of ventilation and in the next slide a Claridge fan on the deck of a steamer is seen at work driving air along a two-foot canvas pipe into one of the holds.



FIG. 16.

In the next (fig. 16) an aërothrust is seen at work. The aërothrust is a two-blade propeller mounted on a portable frame and driven, at the rate of about 1,600 revolutions a minute, by a small air-cooled petrol engine. The whole apparatus weighs less than 100 lb. and is capable of delivering 22,000 cubic feet of air per minute.

The use of this form of aërothrust is highly spoken of in the United States and it can be used to shorten the time required for ventilation after fumigation with sulphur as well as after hydrogen cyanide. Perhaps a better idea of an aërothrust will be obtained from the next photograph in which one is seen on the deck of one of the fumigating tugs in New York Harbour.

Whatever methods, however, are adopted, it must be admitted that the fumigation of a really big ship is a matter of considerable trouble, expense and even anxiety to those responsible for the undertaking.

A better idea of what is meant will perhaps be obtained by inspection of the

next slide which is taken from a blue print of the s.s. *Majestic* and shows the position of the barrels in two of the thirteen sections of the ship into which she had to be divided up in order to facilitate the fumigation.

Whilst fumigation, with hydrogen cyanide, of the ordinary cargo-carrying vessel with its big holds and few cabins is a comparatively simple matter, it is not so with the big transatlantic liner. To make fumigation compulsory every six months, quarantine authorities must be satisfied that there is complete justification on public health grounds. There must be no unnecessary delays to shipping and the charges must not be regarded as a source of additional revenue by the port authorities.

The public health authorities of the United States and of South Africa have criticized the classification of ships into "infected," "suspected" and "healthy" adopted in the present International Sanitary Convention. Cases are quoted to show that it is the "healthy" vessels from the plague-infected port and not necessarily the "infected" or "suspected" vessels which constitute a menace to the public health of the United States. The human case is stated to be largely of academic concern only, as the Quarantine Authorities attach relatively slight importance to such cases as a means of spreading plague, except in so far as they may indicate the source of rodent infection. According to these views the human case does not necessarily indicate an "infected" ship. On the other hand the classification of a ship, from a plague-infected port which has taken no precautions for preventing access to rats, as "*healthy*" instead of "*suspected*," merely because there are no human cases on board or the rats have not been proved to be infected with plague, is held to be a defect.

Objection may also be raised to the absence of provision in the Convention requiring "lighters" in plague-infected ports to be periodically fumigated. This is a matter of considerable practical importance to shipping in the East, where vessels in harbour frequently do not lie alongside a quay, and the work of loading or unloading is done with lighters. Possibly it is a matter better left to the discretion of the local authorities, but it will probably be one of increasing importance to shipping.

As long as rats on ships are under suspicion Quarantine Authorities will press for their destruction, and it must be admitted that the use of hydrogen cyanide is a most effective means to this end.

Unfortunately fumigation with hydrogen cyanide is not free from hazard to human life, and it remains a question for consideration whether the periodic fumigation of ships is the most practical means and the least irksome to shipping and commerce to attain the end in view.

It is therefore of interest to note that in January last the Quarantine Regulations of the United States were revised, to allow the extension of the period of six months for the compulsory fumigation of vessels in the case of ships complying with the following conditions:—

(1) Vessels constructed so as not to favour or encourage the harbourage of rats.

(2) Vessels plying regularly between ports not infected with plague.

(3) Vessels regularly carrying no cargo or cargo of such a nature or so packed or stowed to serve as rat food or rat refuge.

(4) Vessels which have been regularly certified as loading in stream from rat-free lighters or as complying with the regulations relative to fending off from docks, proper use of guards on lines and hawsers, raising or guarding of gangways and ladders and docking at rat-free docks or wharves.

The future policy of Quarantine Authorities will probably move on these

lines, and it is submitted that the interest of the naval architects should be secured on the general question of rendering vessels free from rat harbourage.

The use of hydrogen cyanide as a fumigant is not, of course, confined to ships, and I cannot conclude these remarks without some reference to its employment on land frontiers.

The method has been used to a large extent, and with successful results, for the destruction of lice in clothing, in baggage and in railway carriages on the Russo-Polish border. In 1920 the local authorities in Warsaw constructed

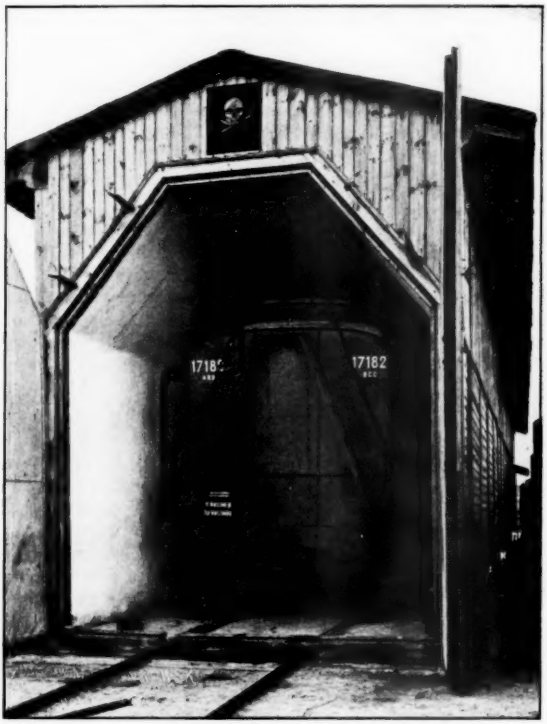


FIG. 17.

a chamber or tunnel which could be hermetically closed, large enough to contain several railroad cars (fig. 17). The cyanide for the fumigation is obtained from poison gas shells which were left in Poland in 1915. After fumigation the gas is withdrawn from the chamber by means of fans and passed through a stove where it is burnt.

At nearly all quarantine stations on the Russo-Polish frontier there is a cyanide chamber which, it is claimed, has given more satisfactory results than the other methods formerly in use. The process is not considered to be

expensive, and all baggage, clothing, bedding, &c., of refugees has been treated in this way.

Cyanide chambers are also used at the Quarantine Station, New York.

Cyklon has been used in a similar way at the frontier stations in Holland, and it is said to be particularly effective in destroying the eggs as well as the lice themselves.

In the time at my disposal it has only been possible to refer to some of the main points which are of interest to this Section, but I hope the photographs of the various methods in actual operation will be of greater interest than details which for the most part are available elsewhere.

DISCUSSION.

Dr. REECE (President) said that whatever chemical might be used the success of the fumigation would depend to a great extent on the care with which the vessel had been prepared for fumigation. It was necessary that persons engaged in the work of fumigation should have a comprehensive knowledge of ship construction, and when large vessels were dealt with the arrangements should be supervised by someone who had an intimate knowledge of the particular ship. It was quite an easy matter to lose one's way in a large ship which had six or seven decks one above the other. It had to be remembered that when once a ship had been battened down for fumigation any work that had then to be carried out in the interior must be done in darkness in narrow gangways and with the possibility of falling through an open hatchway. The liberation of large quantities of lethal gas after fumigation was another matter that needed consideration. He (the President) believed that the quantity of gas required for the complete fumigation of their largest liners was greater than the volume of gas used in any single gas attack on the Western Front during the war.

The reduction of rats on board ship was a public health problem of international importance, as risk of transference of plague from one country to another was lessened as the rats on ships were reduced in number. Fending off vessels from the sides of docks and quays and placing rat guards on mooring hawsers would not prevent rats gaining access to a ship. In all probability the greater number of rats found on ships had been carried on board in cargoes. He imagined that a certain number of rats might always be introduced to ships in this manner. What was required was that ships should be so constructed that no harbourage nor breeding places for rats were afforded. At the present time ships were being built which were in effect rat-free ships. This had not resulted from any consideration of public health problems, but had followed from the evolution in ship construction. It was possible to "rat proof" vessels very much on the same lines as buildings could be made rat proof on shore. He thought the solution of the problem of rat-carried plague infection would be found in improved ship construction, and if ship owners and those who were responsible for designing ships would give effect to such matters the repeated fumigation of ships for rat destruction, the associated danger to human life, the expense of the fumigation and the delay to ships with the consequent interference with trade, would all disappear proportionately and the risk of transference of ship-borne plague from one country to another would be reduced to a vanishing point.

Dr. A. K. CHALMERS (Glasgow) said that Members of the Section had just heard a most comprehensive statement from Dr. Stock as to the questions which arose whenever the use of hydrocyanic acid gas as a means of fumigation came to be considered.

Attention had most naturally been directed mainly to the methods of applying the gas. At this stage its distribution was under reasonable control, but on the completion of the process this control ceased and the gas was usually discharged into the atmosphere to be dispersed by the wind. It thus became of importance to consider, especially in crowded harbours, whether this discharge entailed any risk to adjacent shipping or to persons employed on the wharves. So far as his (Dr. Chalmers') limited

experience had gone, there were two conditions which were capable of contrast, namely, the rapid discharge of the gas in considerable volume, which was possible when a large cargo steamer was under treatment; and the more limited and intermittent discharge which occurred when the ship in question was a passenger liner. In the latter case the discharge was through portholes, companion-ways, ventilators, &c. It occupied a longer time and, of course, a smaller volume of gas was discharged in each unit of time than when, as in a cargo steamer, large holds were uncovered by simply pulling a tarpaulin off them.

So far as he had seen, in the passenger liner, only those actually engaged on board in unsealing the ventilators and companion-ways were exposed to risk, but he was by no means sure that unsealing the holds of large traffic steamers would be equally innocuous in the conditions of a crowded harbour such as he had suggested. He considered it imperative that a certain distance between the ship under treatment and the adjacent ships as well as the wharf itself should intervene, although he was not at all clear as to what that distance should be. On one occasion a large steamer, berthed thirty feet behind a still larger steamer undergoing fumigation, experienced no inconvenience, although it was only right to say that the wind was in the opposite direction to that in which the steamer in question lay.

Dr. W. M. WILLOUGHBY (Port of London) considered that all the methods hitherto devised for cyanide fumigation involved the use of so much apparatus as to put the operation out of perspective with the object aimed at.

Cyanide methods raised a class of "high priests" in the matter of rat killing, whereas he had hoped that a domestic matter such as this would have been taken up by the shipping companies under the sulphur method with their own staffs, when the work of a Port Sanitary Authority would have been only supervision. A maximum effect in freeing ships of rats would be obtained by the enrolment of a maximum number of agencies.

Dr. Wade had shown that 0·5 per cent. sulphur dioxide killed rats: why, therefore, were we now using 3 lb. of sulphur for 1,000 cubic feet, an atmosphere of 3·3 per cent. sulphur dioxide? Such an atmosphere damaged paint work and left an aromatic odour. Moreover it was unnecessary to disinfest where paint work and decoration was costly; rats did not usually frequent such parts of the ship. He (Dr. Willoughby) had carried out complete disinfestations with smaller percentages which had not damaged paintwork.

He thought that when the subject was worked out thoroughly, where rats only were the object of gas attack, the uses of cyanide would be very limited in ship disinfestation. It would seem that the introduction of cyanide methods into England was due to the allowance made for a margin for imperfect procedure in the use of sulphur—and constituted an invitation to such imperfect procedure.

The application of cyanide did not permit of a leisurely survey of the ship which was under fumigation and of the deck search necessary on plague-infected ships.

Dr. C. F. WHITE (Assistant Port Medical Officer, Liverpool) said that the fumigation of ships with hydrogen cyanide in this country was first started because certain medical officers of the United States Public Health Service were unwilling to exempt from fumigation the first and second-class passenger accommodation of the large Atlantic liners, though the American Quarantine Regulations provided for such exemption. Dr. King, the United States medical officer in London, had given the lead in granting this exemption, and as his example had now been followed by the other American medical officers the use of hydrogen cyanide was no longer forced upon them, as all the sections of the ships now required to be fumigated could be exposed to sulphur dioxide gas without damage to paintwork, fittings, &c.

The prevention of the passage of rats between ships and the shore presented many difficulties. The breasting off of ships six feet from the quay added greatly to the cost of discharging and loading cargo, the standard rat-guard was effective only when accurately applied and undamaged, and the parcelling and tarring of ropes was unsatisfactory. Further, rats could jump ashore from ships with a low freeboard and were also carried between ship and shore in merchandise. Consequently adequate protection from the introduction of rodent plague depended, first, on the reduction of the

numbers of rats in dock sheds and on quays by the elimination of rat harbourage and readily available food supplies, and, secondly, on the periodic fumigation of ships.

But he (Dr. White) did not consider it to be the duty of port medical officers rigidly to insist on one particular method of fumigation. Both sulphur dioxide and hydrogen cyanide were accepted as effective fumigants throughout the world and it was immaterial to the port medical officer which method was used provided that he was satisfied as to the efficiency of the fumigation. If a shipowner elected to use hydrogen cyanide it was the duty of the medical officer to render all possible assistance to secure efficiency and to safeguard human life. This entailed a great deal of work on the part of the staffs of port sanitary authorities. In Liverpool ten ships had been fumigated throughout with hydrogen cyanide, which had also been employed in partial fumigations on three other ships. In no case had there been either loss of life or even complaint of ill-effects from any person. This had only been achieved by the most careful supervision of every detail of the procedure by the medical officer and by trained inspectors of the port sanitary staff. Further, even after testing with animals had shown that no dangerous concentration of the gas remained, the medical officer did not sign the ship clear until he was satisfied by personal inspection that the hydrogen cyanide present was not sufficient to produce the slightest disagreeable sensation. This usually meant devoting the whole day to the fumigation, and as Saturdays and Sundays were usually chosen by the shipowners for fumigating, he felt his original enthusiasm for the cyanide method had somewhat diminished.

After a period during which cyanide fumigation appeared to be in abeyance in Liverpool certain shipping companies had again taken up the method, and contrary to the opinion of Dr. Willoughby, he felt confident that in the future hydrogen cyanide would gradually supplant sulphur dioxide as the routine fumigant in this country.

He agreed with Dr. Stock that the liquid hydrogen cyanide method appeared likely to prove the most efficient and the easiest in application; and in view of the probable extension of the use of hydrogen cyanide as a fumigant in this country, he thought that both the paper read by Dr. Stock and the very full report by Dr. Stock and Dr. Monier-Williams recently issued by the Ministry of Health were of the very greatest value.

Dr. ARNOLD CHAPLIN, in the course of his remarks, emphasized the great value of Dr. Stock's paper. From the shipowner's point of view, however, he was of opinion that the method of fumigation by hydrocyanic acid gas was not yet sufficiently freed from its dangers to permit of its being used with impunity. Until that position could be attained, he preferred the method of sulphur dioxide gas as a fumigant. No doubt in time some way would be found by which the method of fumigation by hydrocyanic acid gas could be rendered harmless to human beings, but he felt that that stage had not yet been reached.

Sir GEORGE BUCHANAN said that hydrocyanic gas fumigation on vessels must necessarily be a dangerous occupation, though no doubt the intrinsic danger differed according to the processes used. The fact that it was a dangerous occupation, however, did not in any way imply that it should not be utilized. Many other necessary trades and occupations had their special dangers. Cyanide fumigation ought to be applied by responsible persons acquainted with the risks and all the methods necessary to guard against them. If further study was necessary regarding the prevention of danger, physiologists, chemists, ships' architects and other specialists, who could contribute to the investigations, ought to be consulted. He did not think it was a drawback that cyanide fumigation of ships should be undertaken for profit by one or two responsible commercial firms. Those firms, from the nature of the case, would be on the look-out for all possible improvements, economies and methods of preventing danger, and they would have a trained staff.

It seemed sometimes to be thought that use of hydrocyanic gas for rat destruction on ships was the result of a sort of whim of the United States public health authorities. This was far from being the case. The extensive use of cyanide processes in the Italian port health service, in India and many other places should be realized, as also the large scale on which cyanide had been employed in connexion with Eastern

European epidemics. The efficiency of the gas as a means of destroying rats and, specially, insects did not need any further proof at this stage, and advantage ought to be taken of this fact in public health work wherever cyanide processes possessed practical advantages.

The International Sanitary Convention of 1912, which recommended periodic de-ratization of ships, had not insisted on any one process. In point of fact, at the date of that Convention the use of cyanide processes for rat destruction was very little developed. The intention of those who made the Convention was to deal with the continued spread of plague infection from port to port by means of the ship rat population, and to do this by means of a periodic overhaul of every ship in order to see whether it was infested with rats, and, if so, to get rid of them by any appropriate means. This, he (Sir George Buchanan) believed, was still the position taken by those who had been preparing the revision of the 1912 Convention. It was not intended that every ship, regardless of its character, structure, or condition in regard to rats, should be submitted for six months to a ritual of cyanide fumigation. The principle was that the ship should be overhauled at regular intervals, and that if there was evidence of rats, the most appropriate method should be used, whether burning sulphur or sulphur dioxide in cylinders, using rat catchers or fox terriers, or applying one or another form of cyanide treatment.

The application of cyanide fumigation on a huge scale in the case of some of the passenger ships going to America had perhaps been determined upon without full balancing of the necessities of the case. In this matter, however, they were learning by experience, and he thought the United States authorities had done a good service to public health by making all concerned give their serious attention to this process, which, in appropriate instances, seemed to offer such considerable advantages.

Dr. W. W. KING (U.S.A. Public Health Service) said that the statement had been made that the interpretation of the U.S. Quarantine Regulations, in carrying out fumigation of vessels bound for the United States, had greatly influenced the adoption of hydrocyanide gas fumigations in England. It was to be regretted if anyone had been forced to use this agent unwillingly. Certainly the American authorities had no such intention. At the same time, the British authorities would scarcely regret their experience with it, even if it did not become the method they would use by preference.

The question of relative values between sulphur and cyanide gas was one not yet settled, from the fact that the last word had not been said in regard to either agent, and more particularly as to the newer one, cyanide gas. The latter was undoubtedly the more dangerous, and therein lay the chief criticism urged against it. However, with the help of the investigations which were being carried out on both sides of the Atlantic it was to be expected that considerable improvement would be made in its method of use and in protection against its dangers. At present a most valuable protection consisted in a well-trained personnel, both officers and men, who were familiar with the dangers and the way to avoid them. Even this in itself constituted an element of danger, because after many successful fumigations without any unhappy incident there would be a tendency towards contempt of the dangers and carelessness in avoidance of them. Constant and extreme vigilance was the price of safety.

The periodic fumigation of ships had done good service in the prevention of the spread of bubonic plague by reducing the rat population on the ships, thus lowering the opportunities for transmission of the disease. Heavy infestation on ships was usually the result of rats living and breeding on board for a considerable time. When the flourishing community of rats was broken up by fumigation, re-infestation would take place, of course, but generally it was comparatively slow except when there was on board cargo or something especially attractive to rats. A certain number of the rats might get on board by passing over mooring lines, gangways, &c., but most of them were probably carried on board in cargo and stores.

Periodic fumigations, however, were not the only means which should be used to discourage the rodent pest on ships. In modern construction of iron and steel vessels many old-time rat harbours had been eliminated but there was still room for improvement in this respect. The construction of rat-proof wharves and warehouses reduced

the opportunities for rat infestation of vessels, and it was quite possible that with the development of these and similar phases of the general fight against rats, the frequency of the de-ratization of ships might be lessened.

Dr. STOCK (in reply) said that in view of the lateness of the hour he must reply as shortly as possible.

He agreed with the remarks of the President. If foreign ports were kept free from rats, and vessels were designed and constructed so as neither to favour nor encourage the harbourage of rats, the need for fumigation would only occasionally arise. The question of rendering vessels "rat-proof," however, was one in which the interest and co-operation of naval architects must be secured. When fumigation was necessary the best method to adopt could only be decided upon after all the various factors had been taken into account. Most port health officers were agreed that the best time to fumigate a vessel was after discharge of cargo, and he thought that Dr. Willoughby had, perhaps, the empty cargo-carrying vessel especially in mind when stressing the sulphur method. If this was so, he agreed with Dr. Willoughby that fumigation with sulphur might be a good routine measure to adopt, but on the other hand, the empty cargo vessel was the class of ship on which the fewest difficulties were encountered if cyanide were used. Leaving aside the question of the large passenger vessel, the situation might arise where it was desirable, or even essential, to fumigate a loaded vessel. Under such circumstances the use of sulphur might result in most serious damage to cargo, damage which could be avoided by the use of hydrogen cyanide. He thought Sir George Buchanan had summarized very clearly what one might term the "international opinion." It must also be remembered that the use of cyanide was by no means confined to the destruction of rats, and in quarantine work was widely used for other purposes, e.g., for the fumigation of plants, for which sulphur was quite unsuitable.

The point raised by Dr. Chalmers was important, and had caused him (the speaker) some little anxiety on the first occasion that a huge vessel like the S.S. *Majestic* was fumigated. He thought that it was always a wise precaution to keep the dockside clear of people, and the American practice was not to fumigate a vessel with cyanide if she was lying alongside another that had anybody on board. On the other hand he had been unable to trace any record of any accident being occasioned in this way. Owing to the rapidity with which the gas diffused in the open air, hydrogen cyanide had been found useless for the purposes of gas warfare, and he doubted whether the gas, escaping from a vessel under ventilation, could be a serious danger. A question had been asked as to the best form of gas-mask but he did not think he could add anything to what Dr. Monier-Williams and himself had already stated in their preliminary report. Dr. Woolff of Amsterdam, however, had published the results of some tests he had carried out on the box form of respirator. The "box" was filled with the special filtering material for cyklon and by connecting one side of the box to a flask containing cyklon and the other to a flask in which a mouse and strips of paper moistened with copper benzidine acetate solution had been placed and then driving a current of air through, Dr. Woolff had come to the conclusion that even with the oldest filling a man using a "box" respirator could breathe for over an hour without noticing the presence of cyklon. In regard to rat guards he agreed with the remarks that had been made, but confessed that he was disappointed that the "electric" rat guard had not yet been tried in this country. The whole question was full of interest, and many points required further investigation. Gases did not always diffuse as one might expect them to do, but he hoped that Dr. Monier-Williams and himself would have an opportunity later on of carrying out some of the investigations they had planned, more particularly in regard to the manner in which gases diffused in the interior of a ship and the question of "pocketing."

Section of Epidemiology and State Medicine.

President—Dr. Richard J. Reece, C.B.

Refuse Disposal in Relation to the Enteric Group of Diseases.

By Harold Kerr, M.D.

(Medical Officer of Health, Newcastle-upon-Tyne.)

Up till a dozen years or so ago it was regarded almost as a postulate by medical officers of health that its mortality rate from the bowel infections gave a very fair indication of the sanitary state of an area, and particularly that the incidence of typhoid and diarrhœa were in more or less direct ratio to the prevalence of the conservancy method of excreta disposal. Other influences there were, of course, such as direct personal infection, or infection by way of food (milk, shellfish, watercress and so forth); but an unsatisfactory closet system, and irregular or otherwise inefficient scavenging, with consequent fly-prevalence, were generally accepted as the chief factors in the perpetuation of at least the steady endemic incidence of these diseases, as distinguished from the fulminating outbreaks associated usually with water or milk contaminations.

Forty-one Great Towns, 1906-1910.

In 1911 the present writer obtained particulars over a sequence of five years (1906-1910) from forty-one great towns, and found that there appeared to be little or no correlation between the proportion of conservancy closets and the incidence of bowel infections as between different towns. This, after all, was not to be wondered at, since individual places all differed in numerous respects from one another—in age, occupation, habits, and wealth of population, in situation and natural amenities, climatic influences, water supply, drainage, hospital accommodation, and so forth, besides the variation in their methods of disposing of their effete products.

The conclusion arrived at, however, was that—

"The fairest method of appraising the influence of the conservancy system is by watching the effect on the enteric fever and diarrhœa rates of the gradual elimination of the dry closets in individual towns, that is to say, comparing each town with itself alone."

This showed, in the towns examined, that conversion was invariably accompanied by a fall in the enteric death-rate, though the rate of decline varied very greatly in different individual towns. Unfortunately the necessary data as to the rate of conversion were only available for a few of the towns in question, consequently the value of the deduction was somewhat limited.

Durham County, 1910.

The subject matter of the L.G.B. Report by Dr. S. W. Wheaton, on "Enteric Fever in the County of Durham," was then two years old, and subsequent history had already begun to qualify slightly the reliability of the natural laws that its facts seemed to demonstrate. Already Dr. T. Eustace Hill was able to point to inconsistencies among the numerous sanitary districts of the county, although the correlation still applied to the county as a whole. And in the course of time it became more and more evident that the mere existence of the conservancy system was not the determining factor, but that the quality of the scavenging was probably at least as important.

Liverpool, 1896-1911.

In 1912, Dr. C. O. Stallybrass, of Liverpool, endeavoured to define the causes at work in the great decline in typhoid in Liverpool from 1896-1911. He discovered little direct correspondence between privy conversion and disease decline, for indeed the latter had anticipated the former in Liverpool by about ten years. Ultimately he found that the evidence pointed to hospital isolation, and particularly to the clearance of the congested areas of old courts and alleys which used to characterize Liverpool, and which served as the breeding-grounds of massive infection. The reason he gave for the last assertion was that the common closets (w.c., trough, or otherwise) were usually grossly misused and foul, and that the courts themselves were the scene of much promiscuous defæcation by children and others, and the chosen Eden of swarms of flies.

Present Inquiry.

Fifty Great Towns and Durham County, 1906-1910 *and* 1918-1922.

Recent reference by Dr. Hill to the subject, in reports and in his Presidential Address last session to the Society of Medical Officers of Health, suggested to the present writer that it would be worth while to take up the inquiry again, but on a more extended scale, with a view to the assessment in various areas of the outstanding factors in the universal great decline in incidence of the diseases in question, since the five-year period previously examined.

Since water no longer plays more than a very occasional part, in the towns at least, in enteric causation, and food stuffs (milk, ice-cream, shellfish, watercress, &c.), also figure rather as occasional and accidental than as constant vehicles of infection, information was sought only upon methods of excreta and domestic refuse disposal, and of street cleansing, and an endeavour was also made to gauge the extent to which dried milk has come into use in each area. Particulars were asked for relating to each year in the two quinquennia 1906-1910 and 1918-1922 from the medical officers of health and borough engineers or cleansing superintendents of about fifty great towns, and most grateful thanks are due to the officers of the forty-nine towns from whom full

replies have been received, as also to Dr. T. Eustace Hill, who supplied exhaustive details in respect of each of the forty-three sanitary districts in the County of Durham.

So great is the mass of valuable data thus obtained that it is quite impossible to deal fully or adequately with it in the scope of this paper, but an attempt has been made to synopsize, and to extract a few at least of the outstanding facts which emerge.

Climatology.

An official statement of the general climatology of each of the years was obtained from the Meteorological Office, from which it appeared that the summers of 1906 and 1908, in the first of the two quinquennia, and of 1921 in the second quinquennium, alone were hot and dry, and of the type that appears to favour an autumn prevalence of diarrhœa, usually followed by excess of enteric.

Generally speaking, the mortality rates for each of these three years was relatively high, though those for 1921 in all the towns were much below the corresponding figures in 1906 and 1908.

For greater convenience, the rates in each town have been averaged for each quinquennium, thus giving a fairer measure of progress than by comparing individual years, and the results are as set out in the large table (see pp. 36, 37).

At first sight it would appear almost hopeless to evolve any kind of sequence or mutual relation out of this, beyond the fact that there is a general immense improvement common to all units.

Water Carriage versus Conservancy.

Water Carriage Towns in both Quinquennia.

One thing strikes us, viz., that an entirely water-carriage system does not connote, *per se*, a low enteric and diarrhœa rate. If the figures for Bristol, Leicester, Cardiff, Plymouth, and Southampton (all w.c. towns for many years) be examined, it will be seen that while all had, compared with the rest, fairly low typhoid and diarrhœa mortality in the first quinquennium they made much less improvement in the next dozen years, and in the second quinquennium actually showed death-rates which compared less favourably with those of other towns. West Ham, Portsmouth, and Preston, have improved their relative positions, as has also Liverpool (which had a low place in the first quinquennium). Brighton, Hornsey and Nelson, all high in the first period, remain almost, but not quite, as high in the second.

Towns with High Proportion of Conservancy Closets in both Quinquennia.

Taking the towns with continued outstandingly high proportion of conservancy closets we find some striking contradictions. Thus in Hull, Middlesbrough, Wigan, Warrington and Darlington, both rates are high relatively to other towns in each of the two periods. In Bolton, Derby, Huddersfield, Halifax and Rochdale, the typhoid rates are high, while the diarrhœa rates are low. And in Gateshead, South Shields, St. Helens and Wolverhampton, the enteric rates are low, and the diarrhœa rates high!

ENTERIC AND DIARRHŒA MORTALITY IN RELATION TO EXCRETA DISPOSAL AND USE OF DRIED MILK IN THE TWO QUINQUENNIA, 1906-10, and 1918-22.

Town	Population	Average Annual Death Rates per 1,000 Population						Total Dried Milk Distributed in 5 Years	Conservancy Closets Percentage		Remarks
		Typhoid			Diarrhœa and Enteritis (all ages)						
		1906-10	1918-22	Per cent. fall	1906-10	1918-22	Per cent. fall	lb. per 1,000 population (Approx. estimate)	1906-10	1918-22	
Birmingham ...	945,100	0·07 17	0·01 15	*86* 22	1·14 40	0·37 29	*68* 14	0·3 30	?	W.C.'s	
Liverpool ...	826,400	0·10 30	0·011 24	*89* 14	1·05 36	0·64 47	*39* 43	0·6 16	W.C.'s	W.C.'s	
Manchester ...	748,500	0·11 33	0·02 34	*82* 30	0·95 30	0·30 20	*68* 26	0·1 38	35	0·76	
Sheffield	522,600	0·08 20	0·006 7	*92* 11	1·27 41	0·42 37	*67* 16	1·8 2	25	12	
Leeds	466,700	0·07 17	0·01 15	*86* 22	0·80 24	0·36 28	*55* 28	? —	2	1	
Bristol	383,900	0·05 6	0·01 15	*80* 34	0·51 8	0·26 15	*49* 36	? —	W.C.'s	W.C.'s	
West Ham ...	310,200	0·08 20	0 01 15	*87* 19	0·96 31	0·38 32	*60* 21	0·7 14	W.C.'s	W.C.'s	
Hull	294,000	0·08 20	0·02 34	*75* 39	1·05 36	0·47 42	*55* 28	0·6 16	65	49	
Bradford	291,300	0·10 30	0·02 34	*80* 34	0·61 13	0·25 13	*59* 24	* —	46 ?	9 ?	* Sterilized bottled milk used
Newcastle-upon-Tyne	281,600	0·05 6	0·01 15	*80* 34	0·78 20	0·44 40	*44* 40	1·4 7	17	8·5	
Nottingham ...	267,900	0·11 33	0·008 11	*93* 6	1·00 35	0·32 23	*68* 14	0·9 12	58	1	
Salford	240,700	0·16 43	0·04 48	*75* 39	0·97 33	0·46 41	*52* 33	0·2 34	20	2	
Leicester	238,800	0·03 1	0·017 32	*43* 48	0·73 17	0·26 15	*64* 18	1·3 8	W.C.'s	W.C.'s	
Portsmouth ...	236,630	0·13 38	0·00 ? 1	*100?* ? 1	0·50 5	0·24 11	*52* 33	0·2 34	W.C.'s	W.C.'s	
Cardiff	223,830	0·06 9	0·02 34	*67* 43	0·73 17	0 37 29	*49* 36	0·8 13	W.C.'s	W.C.'s	
Plymouth ...	209,857	0·06 9	0·01 15	*83* 29	0·60 12	0·28 19	*53* 31	0·5 21	W.C.'s	W.C.'s	
Bolton	182,700	0·16 43	0·02 34	*88* 17	0·79 22	0·30 20	*62* 20	? —	52	36	
Rhondda	167,500	0·12 36	0·016 29	*87* 19	1·62 46	0·196 8	*88* 2	0·6 16	0·74	0·47	
Southampton ...	163,700	0·06 9	0·025 42	*58* 46	0·56 10	0 34 26	*39* 43	0·3 30	W.C.'s	W.C.'s	
Sunderland ...	162,900	0·09 25	0·006 7	*93* 6	0·79 22	0·50 44	*37* 46	1·5 5	29	9	
Birkenhead ...	149,200	0·09 25	0·009 14	*90* 13	0·99 34	0·47 42	*53* 31	0·5 21	0·4	W.C.'s	
Oldham	148,300	0·06 9	0·027 43	*55* 48	0·93 29	0·23 10A	*75* 6A	0·1 38	29	6	
Brighton	135,279	0·04 3	0·006 7	*85* 26	0·51 8	0·21 10	*59* 24	0·2 34	W.C.'s	W.C.'s	
Middlesbrough ...	134,800	0·18 45	0·025 42	*86* 22	1·56 45	0·81 49	*48* 38	0·6 16	80	51	
Derby	132,400	0·07 17	0 017 32	*76* 38	0·50 5	0·14 6	*72* 10	0·4 28	27	14	

ENTERIC AND DIARRHŒA MORTALITY IN RELATION TO EXCRETA DISPOSAL AND USE OF DRIED MILK IN THE TWO QUINQUENNIA, 1906-10, and 1918-22.

Town	Population	Average Annual Death Rates per 1,000 Population						Total Dried Milk Distributed in 5 Years	Conservancy Closets Percentage		Remarks
		Typhoid			Diarrhœa and Enteritis (all ages)						
		1906-10	1918-22	Per cent. fall	1906-10	1918-22	Per cent. fall	lb. per 1,000 population (Approx. estimate)	1906-10	1918-22	
Blackburn ...	129,600	0·10 **30**	0·00 ? **1**	*100 ?* **? 1**	0·78 **20**	0·26 **15**	*67* **16**	0·5 **21**	33	18	
Gateshead ...	127,300	0·04 **3**	0 005 **6**	*88* **17**	0·90 **26**	0·59 **46**	*34* **47**	0·7 **14**	77	76	
Stockport ...	125,800	0·06 **9**	0·015 **26**	*75* **39**	1·10 **39**	0·12 **5**	*89* **1**	0·2 **34**	33	9	
Norwich	122,900	0·11 **33**	0·016 **29**	*85* **26**	0·65 **14**	0·20 **9**	*69* **11**	0·3 **30**	21	2·5	
South Shields ...	122,400	0·06 **9**	0·015 **26**	*75* **39**	0·72 **16**	0·65 **48**	*10* **48**	1·5 **5**	80	79	
Preston	120,900	0·16 **43**	0·01 **15**	*94* **9**	0·96 **31**	0·35 **27**	*64* **18**	0·5 **21**	W.C.'s	W.C.'s	
Huddersfield ...	111,600	0·09 **25**	0·03 **44**	*67* **43**	0·48 **4**	0·07 **1**	*85* **3**	0·1 **38**	70	50	
St. Helens ...	106,400	0·14 **39**	0·008 **11**	*94* **9**	1·06 **38**	0·42 **37**	*60* **21**	1·6 **4**	66	25	
Wolverhampton	105,700	0·06 **9**	0·00 **? 1**	*100 ?* **? 1**	0·92 **28**	0·37 **29**	*60* **21**	0·4 **28**	52	23	
Burnley	105,100	0·09 **25**	0·019 **34**	*79* **37**	1·46 **43**	0·33 **24**	*77* **6**	? —	1·8	0·8	
Halifax	100,500	0·09 **25**	0·01 **15**	*89* **14**	0·21 **1**	0·19 **7**	*10* **48**	? —	75	66	
Rochdale	92,510	0·06 **9**	0·03 **44**	*50* **47**	0·58 **11**	0·25 **13**	*57* **27**	1·1 **9**	79	42	
Wigan	91,750	0·26 **49**	0·04 **48**	*85* **26**	*0·84 ?	0·40 **33**	*52* **33**	? —	64	53	* Diarrhœa only
Hornsey	87,310	0·04 **3**	0·007 **10**	*82* **30**	*0·19 ?	0·106 **4**	*44* **40**	? —	W.C.'s	W.C.'s	* ,, ,,
Grimsby	82,329	0·21 **47**	0·008 **11**	*96* **4**	0·91 **27**	0·41 **35**	*55* **28**	0·5 **21**	?	17	
Smethwick ...	78,140	0·08 **20**	0·004 **4**	*95* **5**	0·77 **19**	0·24 **11**	*69* **11**	1·1 **9**	4	W.C.'s	
Warrington ...	79,150	0·12 **36**	0·01 **15**	*92* **11**	1·5 **44**	0·41 **35**	*73* **7**	0·5 **21**	95	76	
Rotherham ...	69,840	0·15 **41**	0·016 **29**	*89* **14**	0·60 **12**	0·33 **24**	*45* **39**	3·1 **1**	21	1·8	
Darlington ...	66,710	0·08 **20**	0·015 **26**	*81* **33**	0·68 **15**	0·40 **33**	*41* **42**	0·5 **21**	71	43	
Widnes	38,879	0·20 **46**	0·014 **25**	*93* **6**	2·16 **47**	0·42 **37**	*81* **4**	? —	68	11	
Stretford	47,030	0·05 **6**	0·03 **44**	*40* **49**	0·29 **2**	0·09 **2**	*69* **11**	1·0 **11**	85	1	
Nelson	39,839	0·03 **1**	0·004 **4**	*87* **19**	0·39 **3**	0·104 **3**	*73* **7**	0·3 **30**	W.C.'s	W.C.'s	
Colne	26,000	0·059 **8**	0·02 **34**	*66* **45**	0·95 **30**	0·26 **15**	*73* **7**	0·6 **16**	13	9	
Farnworth ...	27,901	0·21 **47**	0·037 **47**	*82* **30**	1·44 **42**	0·306 **21**	*79* **5**	1·7 **3**	?	33	
Durham County (43 districts)	943,356	0·14 **39**	0·02 **34**	*86* **22**	0·82 **25**	0·50 **44**	*39* **43**	? —	75 (probably)	75	

The figures in black type indicate relative order of towns to one another.

Towns recently "converted."

And what is the result where great numbers of conversions have been carried out? Manchester has simply moved with the rest, remaining pretty low down in the list for enteric (despite an 82 per cent. improvement) and about the middle of the list for diarrhœa (with a 68 per cent. improvement). Bradford, converting perhaps four-fifths of its 50 per cent. dry closets, makes an 80 per cent. improvement—less than the average—in enteric, and with a 59 per cent. decrease in diarrhœa remains relatively to other towns *in statu quo*. Salford converts all but 2 per cent., from 20 per cent. privies, and actually, with 75 per cent. and 52 per cent. improvement in enteric and diarrhœa respectively, loses ground!

Special Comparisons.

Nottingham and Leicester.

Comparison of those old protagonists, Nottingham and Leicester, is particularly interesting, for Nottingham has been very hard at work converting its 58 per cent. privies, and has now only 1 per cent. left, while Leicester has had none at all for many years past. Nottingham, with 93 per cent. improvement in enteric rate, now stands far above Leicester, with 43 per cent. (0·008 as against 0·017), though Leicester still takes the lead in regard to diarrhœa (0·26 as against 0·32).

Newcastle and Gateshead.

Here we have two neighbouring towns in which the larger, Newcastle, has got rid of all but 8½ per cent. of its privies (from 17 per cent.), while the smaller, Gateshead, still has 76 per cent. conservancy closets, only 1 per cent. less than in the first quinquennium. Newcastle, with 80 per cent. decrease in enteric has dropped in relative position, and now stands lower in this respect than Gateshead, which in the same period has improved by 88 per cent. In regard to diarrhœa, Newcastle still retains a somewhat precarious lead.

Sunderland and Stockport.

Sunderland and Stockport, with somewhat similar proportionate conversions, show contrasting results, since Sunderland's enteric rate has improved by 93 per cent., and Stockport's by 75 per cent. to very slightly above the general average, while Sunderland, with only 37 per cent. improvement in diarrhœa, falls nearly to the bottom of the list, and Stockport, with 89 per cent. gain, is now nearly at the top.

Widnes and Stretford.

Widnes and Stretford similarly show a contrast. Widnes improves in regard to enteric by 93 per cent. and so rises from a low place to a high one, while Stretford, with 40 per cent. gain in enteric, experiences the reverse. While Widnes, with 81 per cent. improvement in diarrhœa, still remains relatively low on the list, Stretford, with 69 per cent. improvement, remains the highest but one.

Scavenging.

On paper, scavenging administration would appear to be almost equally perfect in all the towns. The usual practice seems to be to empty ordinary domestic refuse containers weekly, and ashpits monthly; in a few instances collection is stated to be daily, or thrice or twice weekly. Closet pails are mostly emptied weekly, and in one or two instances are actually washed at a depot; middens are emptied as a rule monthly.

In some of the Yorkshire and Lancashire privy towns the container tubs are wooden. In Nottingham and in Manchester a history is given of substantial reduction of enteric mortality following substitution of metal containers for wood. In Nottingham the death-rate to 1900 was 0·29, and after the change to metal it fell, by 1910, to 0·157. In Manchester in 1870, with cesspools and privy middens, the rate was 0·6; in 1881 with cesspools abolished, and most of the privy middens converted to pails the rate was 0·22; in 1891, with continuance of pails, and gradual neglect in scavenging, the rate was stationary, or on the rise again: in 1915, with conversion to water carriage practically complete, the enteric death-rate was 0·06, and, by 1921, 0·02.

Cleansing.

Street cleansing practice (in theory at least) appears to be uniform, with daily sweeping of principal streets, and of secondary streets and back streets and courts thrice, twice and once weekly, as a general rule. The water flushing of back streets and courts is not usual, though a few engineers practise it, weekly in summer, or on request by the medical officer of health. Nearly all, however, indicate considerable improvement in method during recent years, and it is evident that the better surfaces, from substitution of asphalt and tar spraying for the former granite setts and macadam, with substitution of motor cleaners and sprinklers and the general disappearance of horses and stables, has enabled the work to be done much more frequently, with better result, and with much less dust-raising than formerly.

On the whole, enteric incidence seems to be less influenced by circumstances of scavenging and cleansing than is that of diarrhœa, at any rate in recent years. Formerly it was usual for some medical officers to classify their enterics annually under "privy" and "w.c." houses, and to show a distinct disproportionate incidence upon the former. Thus Dr. Wright Mason states that 85 per cent. of his enterics occur in privy houses (Hull is still half conservancy) and 15 per cent. in water-closet houses. In Newcastle we find the distribution as follows:—

Incidence of Enteric Fever in Newcastle.

Year	Cases per 1,000 houses with Privies	Water-closets
1913 (11½ per cent. privies)	3·80	1·74
1914	1·50	1·26
1915	1·63	1·03
1916	1·69	1·06
1917	0·37	0·39
1918	0·37	0·39
1919	—	0·15
1920	0·41	0·15
1921	0·21	0·12
1922 (8½ per cent. privies)	—	0·36
Ten years	12	7·3

Altogether 378 w.c. houses and sixty privy houses were involved. But quite a number of the former, sixty in all, had privies in their near neighbourhood, and infection may have come from them.

It has been possible to get out particulars of diarrhœa (deaths only) during the last eight years, and in that time there were 1,085 deaths in w.c. houses or 20·9 per 1,000 houses, and forty-seven in privy houses or 9·4 per 1,000 houses. But here again 272 of the w.c. houses involved were in the vicinity of privies.

This factor is referred to also in the Second Report on Infantile Mortality of the L.G.B., 1912-13, where it is stated that a few scattered privies may be as dangerous as massed multitudes.

Durham County, 1906-10 and 1918-22.

From Dr. Hill's very complete figures for each of his forty-three sanitary districts in County Durham one finds, on comparing the two quinquennia, that while very little progress has been made in effecting conversions, there has been a diminution of over 85 per cent. in the enteric death-rate (nearly 88 per cent. in incidence), and 40 per cent. in the diarrhœa death-rate. Thus County Durham, which included some of the blackest spots in the country in regard to bowel infections, with an enteric rate rather above the average for the great towns, has actually improved its relative position, and now has an enteric rate below the average. While sharing in the national decline in diarrhœa, progress has been relatively less, and position has been lost, but the place of Durham County is nevertheless superior to that of Liverpool, Gateshead, Middlesbrough, and South Shields, and equal to that of Sunderland.

The Durham pit villages are not remarkable for *furore* in their worship of Hygeia, and therefore this improvement in common with towns which have made immense efforts, by conversions and otherwise, is really remarkable (compare also Gateshead, above). Of course there is a considerable range of difference between individual districts, and Dr. Hill states that the quality of the scavenging and cleansing appears to be a chief determining factor.

The municipal boroughs, with their scavenging and cleansing in municipal hands, as a general rule show lower disease rates than urban districts, or rural districts that are really collections of small townships, where these functions are either in the hands of contractors or are much neglected otherwise.

Dr. Hill lays great stress, as do many of the other medical officers in their replies, upon the value of the great amount of educational work being carried on now through school and maternity and child welfare medical services.

Dried Milk.

To what extent the use of dried milk has affected the diarrhœa rate is not clear from the data available. Some towns can show figures for both free and retailed powder, others can only venture an estimate. The amounts so stated are totalled for the five years (none was used in the first quinquennium) and set out as so much per 1,000 population to allow of some sort of comparison. From the large table it will be seen how greatly the quantities varied in different places, and how little correspondence appears between these and the diarrhœa death-rate.

Other Influences.

Dr. Niven ascribed 9 per cent. to 30 per cent. of his total cases of enteric to shellfish, and 20 per cent. to direct personal infection (annual report for Manchester, 1920), and in Newcastle in the last ten years we suspected shellfish in 8 per cent., and direct personal infection in at least 20 per cent. of our cases. In each instance this leaves a very large proportion unexplained. We know the causative organisms, and we know something of the mode of conveyance of infection. And all of this is intimately associated with human and animal excrement, and less directly with other waste organic matters, and such as favour the breeding of flies. And flies, according to the observations of Sir William Hamer, have become distinctly less prevalent in recent years.

Inoculation, and control of immigration, played a considerable part no doubt in preventing enteric-infection in a proportion (only) of the most susceptible sex-age groups of the population during the war period, and for a year or so after, and consequently in extension of the disease to others. Carriers doubtless account in part for the continuance of the infections to-day, as they have always done, and little more is being effected in their detection now than hitherto.

Other Countries.

(*a*) *Denmark.*

Experience in other countries of progressive character is similar to our own. Dr. Max Christiansen, of Copenhagen, has been good enough to obtain the Danish figures for enteric and diarrhœa from 1890 to 1922, for urban and rural populations separately. These are strictly comparable with those of this country. With a general safeguarding of water supplies there has been active conversion of dry closets, improved scavenging and cleansing, and great progress in dairy hygiene. Popular education, through the schools and infant welfare centres, has been pressed on actively. And in fact sanitary policy in Denmark is on the same lines as our own.

Dr. Christiansen expresses the opinion that no one factor, such as privy conversions, is alone responsible for the steady disappearance of bowel diseases, and he lays stress on the results of the educational policy in regard to mothers and children.

(*b*) *United States of America.*

Dr. Charles V. Chapin, of Providence, one of the most eminent and clear-sighted hygienists in America, has also written at some length upon his experience. After ensuring a safe water supply chief attention was given to conversion from conservancy, and coincident with the progress of this work in Providence, during the nineties, there was a 40 per cent. drop in enteric incidence.

Dr. Chapin quotes Dr. McCullough, of Ontario, as saying that "the reason we have less typhoid is because we have less typhoid," and that unless existent in mass, it tends to die out, as in the case of malaria (Ronald Ross).

In regard to diarrhœa Providence has experienced a phenomenal reduction, and Dr. Chapin says that apart from education of mothers by the visiting nurses, little direct action has been taken. But he offers the interesting

theory that "resistance is of more importance than infection in diarrhœa, and that modern methods of feeding which keep up the nutrition of the infants and children are probably an important factor."

Conclusion.

Such, then, is a very rough and incomplete digest of the information that has come to hand in the preparation of this paper. There is very much more that might have been done with it, had time and space permitted. For example, a detailed study, year by year, of each town's history might have thrown more light upon a quest that is exceedingly involved. The figures for County Durham alone would provide pabulum for a valuable analysis. As it is, the conclusions to be derived are very general, and not too definite.

In the first place, while there is strong and repeated evidence from many individual towns, and from reliable observers, that privy conversion has played a notable if not a chief part in the past in the reduction of enteric and diarrhœa incidence, it is not nearly so clear that conversion is to-day, *per se*, even essential to a low enteric rate (although it would seem to have rather greater influence upon diarrhœa) provided that scavenging is efficient. This, of course, is altogether apart from the consideration that the water carriage system is an æsthetic necessity and imperative for ordinary decency and comfort.

Scavenging and cleansing are interdependent. Modern road-making, and road cleansing, with the replacement of the horse by the motor in general service, has done very much to lessen dirt, dust, and flies, all of them important factors in the propagation and dissemination of the bowel infections.

The substitution of dried milk for the usually highly contaminated fresh commodity is all to the good in that it cuts out another link in the chain of communication between midden and man.

With inoculation we are hardly concerned in the present study, although its protective value must not be forgotten. Nor are we considering whether the germ stock is declining in vigour or whether we ourselves are acquiring immunity. That there is no general diminution of virulence of the *Bacillus typhosus* is evident from the case mortalities.

This, however, *is* relevant, that as the result of the very considerable amount of education systematically carried out through our public health and education departments, to say nothing of innumerable other agencies, each successive generation shows a higher appreciation of the common-sense laws of health, and adopts an ever-rising standard of cleanliness and wholesome living, which is not compatible with the class of disease we are now discussing. In fact, as stated by one correspondent, "we are getting more civilized."

DISCUSSION.

Dr. R. J. Reece (President) said that Dr. Kerr had taken great trouble in preparing his paper, and he made it clear that, although the change from the conservancy method of excreta disposal to the water carriage system had played an important part in the reduction of the enteric group of disease, it was not the sole factor concerned. Although it might be said that the change in the method of excreta disposal had led to an all-round reduction in the incidence of the enteric group of disease, the decline had been unequal in different towns; and it was manifest that further investigation was essential in the elucidation of the special factors that were operative in maintaining or reducing the incidence in one or another town. It might be found that in regard to

certain towns the fall in the incidence of enteric fever might be due not solely to the method of excrement disposal in operation but to the less frequent introduction of infection from the outside. The attention now paid to the pollution of beds from which shellfish were gathered, the closure of polluted beds and the cleansing of shellfish before being placed on the market possibly had played some part in the fall of the enteric fever attack rate in certain towns in which shellfish were habitually consumed, and this appeared to have been the case in the City of Manchester. The period of the year in which shellfish were in season was the autumn and winter, i.e., the time of the maximum seasonal prevalence of enteric fever, and the lower curve of seasonal prevalence now observed might in some measure be accounted for by the greater attention now directed to secure that shellfish when brought to the market should be free from pollution. The consumption of flat fish from polluted estuaries which went on fairly regularly throughout the year would not effect the autumnal rise in the enteric fever curve. During last year several outbreaks of paratyphoid B had occurred, and it was a question how far this particular disease prevailed at the present time in England. In the inquiry made by Dr. Kerr it had not been possible to subdivide the enteric group of disease into typhoid fever, paratyphoid B, &c., but it would be interesting to ascertain how far this differentiation could be made, and whether paratyphoid B was prevalent in those towns in which the fall in the incidence of diseases in the enteric group had not been commensurate with that of other towns in which the conservancy method had been replaced by the water-carriage system of excrement disposal.

Sir WILLIAM HAMER said it was interesting to compare data from different towns; and Newcastle-upon-Tyne and London presented some noteworthy contrasts. In London conservancy methods almost ceased to exist half a century ago, so that any influence they might have had upon typhoid practically vanished at that time. As to scavenging, London by-laws came into operation in 1894 and were straightway enforced, yet typhoid showed actual increase in the closing years of the nineteenth century. London observations upon flies gave no support to the fly-typhoid theory. Water outbreaks had been conspicuous by their absence, and the few milk outbreaks recorded would not stand close criticism. As to healthy carriers, Dr. Kerr's abstract said that "carriers are probably just as much a factor to-day as previously." This was a formula most of them could perhaps almost ungrudgingly accept. So far as he (the speaker) was concerned, he had felt much difficulty with regard to healthy carriers at any rate since 1906, when a fish outbreak in London simultaneously involved thirty-three different retail supplies, and suspicion as to origin was thus necessarily transferred from the shops and stalls affected, at least as far back as the wholesale market, and indeed presumably to a distant estuarial source. The London typhoid he believed was a fish and shellfish typhoid. It had been well said that a working hypothesis should not only account for the facts it was invented to explain but also for other facts not under review when the hypothesis was formulated. "Shellfish typhoid" was invented round about thirty years ago; "fish typhoid" a few years later. So far as London was concerned the latter hypothesis was now seen to explain broadly the time correlation between decline of typhoid and increasing disuse of the A_3 area as a main source of supply of small plaice to London. Moreover, the almost entire abandonment of this area, from about 1909, and its complete disuse since 1911, corresponded with disappearance of the autumnal waves of prevalence, a phenomenon first discernible in 1909, and sustainedly so from 1912 onwards.

Dr. STOCK said that he thought they were too often inclined to be satisfied with the conclusions arrived at as a result of former investigations, and the value of reviewing old problems from time to time, in the light of more accurate data and with the aid of improved methods, could not be overrated. Dr. Kerr's excellent paper was one of great interest. The facts and figures, however, which he had brought to notice, required to be carefully studied before it was really possible to add anything very helpful to the discussion. If it was his (Dr. Kerr's) intention to confine the question to the behaviour of enteric fever in this country, his investigations and conclusions must be of the greatest value, but if, as epidemiologists, it was permissible to consider the problem from the point of view that enteric fever was by no means confined to this

country, but was almost a world-wide disease, he (Dr. Stock) thought that some of his conclusions required qualification. Dr. Kerr's statement that "it is not nearly so clear that conversion is to-day *per se* even essential to a low enteric rate" should not be taken too generally. He (Dr. Stock) quite agreed that in a comparison of the enteric rates in different localities, the wide variations in regard to the conditions of the people and climatic influences, must be taken into account. The difficulty he had always found had been to reduce these conditions to any numerical factor.

A mere statement that a certain proportion of conversions had been carried out gave no idea of the actual conditions existing in a given area. Thus, a number of conversions in a crowded area was much more likely to be followed by improvement in the enteric or diarrhœal rates than if the same number of conversions were carried out in a thinly-populated district. In any case he (Dr. Stock) thought it essential to have a clear idea as to what the local conditions were. With all respect to such an authority as Sir William Hamer, he preferred to rely on the "legend" that he (Sir William) was the author of the fly theory, than on the statement he had just made that he did not believe in it. He (Dr. Stock) thought that Sir William must have been referring only to the spread of enteric in London. Anybody who had experienced a real plague of flies and had seen them, with their feet smeared with chloride of lime from the latrine buckets, walking over the plates or food at meal times could not be sceptical as to the part they might play. In a town with few privies but which suffered much from flies, the chances of enteric being spread might be considerably greater than in, say, a similar town with more privies but fewer flies. Where so many factors might be concerned in the propagation of enteric fever, it might be difficult to state definitely that any one factor had been of most effect in reducing the incidence. Dr. Kerr had relied chiefly for his data on experiences in this country, and he (Dr. Stock) would suggest that if he could find time to continue his inquiry, he could obtain most valuable data from some of the rapidly growing towns in the Dominions.

When he (Dr. Stock) went to Johannesburg as Assistant Medical Officer of Health in 1904, there were annually over 1,000 cases of enteric fever among the white population alone. At that time the town depended entirely on the pail-closet system, but by 1907 the main outfall sewer had been completed, and by 1908-9 a considerable portion of the more crowded area had been converted and the following figures, showing the *mortality* rates from enteric fever per 1,000 for the white population alone, might be of interest:—

In the year	1903-4	the mortality rate was		...	...	...	1·3
,,	1904-5	,,	,,	...	...	...	0·4
,,	1905-6	,,	,,	...	...	...	0·7
,,	1906-7	,,	,,	...	...	...	0·4
,,	1907-8	,,	,,	...	...	...	0·3
,,	1908-9	,,	,,	...	...	...	0·3
,,	1909-10	,,	,,	...	...	...	0·15

He did not contend that this reduction was entirely due to conversion of pail closets into w.c.'s, but the opinion formed was that the conversions had undoubtedly contributed enormously to the reduction. There were many other factors which might have, and which no doubt did contribute. Chief amongst them he should place an enormously improved service for emptying and dealing with the night soil pails on those premises which had not been converted. The common form of pail, such as was only too frequently used in this country, was an iron pail with the bottom and sides riveted together. This form of pail was difficult to handle, and with the rough use incidental

to service the joints opened, leakages occurred, and the ground became soiled with fæcal matter. Whilst this was probably not of great importance in a country such as England with its humid atmosphere, it did become of very great importance in a country where the atmosphere was persistently dry and dust storms prevailed. To deal with this possible factor in Johannesburg, an improved form of sanitary bucket, made out of seamless pressed steel, was evolved, and the use of this pail went a very long way to remove the nuisance occasioned by leakage. This form of pail, moreover, had the great advantage that it was easy to clean, and after cleaning the pails could be stacked one inside the other, which rendered their transport easier and reduced the cost of the service. After the pails had been emptied, they were taken to a cleansing station where they were washed and dipped in hot creosote. They were then placed on racks to drain, and when required for service, stacked in the service carts. He (Dr. Stock) thought that this system, of which he had only given a brief outline, was as perfect as any he had seen anywhere. The service aimed at emptying the pails on alternate nights, and it could not be denied that the improvements which were effected must also have contributed largely to the reduction in the enteric rate. There was also the example of the town of Bloemfontein, which many of those present would remember bore a most unenviable reputation during the South African War on account of the terrible outbreak of enteric fever which occurred in the vicinity amongst the British Troops. Enteric fever persisted in Bloemfontein for some years after peace was restored, but practically disappeared directly a water-carriage system was installed in the town. No doubt with an ideal scavenging service and properly constructed pail closets, their conversion to the water-carriage system might not be such an important factor; but he certainly thought that under the ordinary conditions which existed in big towns, and particularly if the abominable privy midden system was in vogue, conversion to the water-carriage system had a very great effect in lessening the enteric rate. The good effects of conversion might in themselves, of course, be vitiated by other factors.

He would certainly agree with Dr. Kerr's conclusion that improved methods of street cleansing had little influence on the enteric rate. Such improvement could only influence the spread of the disease very indirectly. In regard to the question of carriers, he would ask Dr. Kerr whether he did not think that as the enteric rate fell the carrier did not assume a relatively greater importance? No doubt as we reduced our number of cases of enteric, so we eventually reduced the number of carriers, and what was of almost equal importance in countries where enteric fever was really endemic, we reduced the number of our ambulatory cases. In South Africa, certainly, the carrier had frequently been a cause of outbreaks of enteric, and instances could be quoted in which a milk supply had been infected in this way.

Dr. G. CLARK TROTTER (Islington) said that the inquiries, the sifting, and the tabulation necessary for the preparation of Dr. Kerr's paper had involved an enormous amount of work on the part of the author, and, as was the case with research work, results, as Dr. Kerr had said, were disappointing from the fact that they were inconclusive. These results, however, were of value, for until such investigations were done we were, so to speak, in the dark.

Referring to enteric fever in Paisley, and the records from 1865, Dr. Trotter stated that main sewage was commenced in 1879, completed 1894; from 1889 to 1911 there had been the total abolition gradually of privy middens, and apart from a probably water-borne outbreak, 1893-98, one might ascribe the main diminution of enteric to the introduction of the water-closet system. There was a difficulty, arising from the

circumstance that the drainage supply went into the tidal river, and if the spread by flies was as common as some believed, the enteric should have continued.

He (Dr. Trotter) was strongly of opinion that although one often failed, on inquiry into the occasional cases of enteric now occurring, to get at any probable source of infection, *every* persistence in this respect should be continued, and he gave examples of several obscure cases in which hidden sources of infection had been found. It was by means of persistent and painstaking investigations, such as Dr. Kerr's paper showed, that our knowledge might be increased.

Dr. ALLAN C. PARSONS remarked that he had no experience of enteric fever in England. His only possible claim to notoriety lay in the fact that, long ago, he had dared to diagnose and report this disease in a tropical Crown Colony—only to be told that enteric fever did not exist in those parts.

The patient in this instance was an English explorer, not long out from home, who had been trekking through the Colony in easy stages and with the usual retinue of native carriers and servants. Within a year or so of his death two further cases of enteric fever were reported by other medical officers of the colony, and the interesting point was that in both instances these patients were attacked while travelling along the route that had previously been followed by the speaker's patient.

Section of Epidemiology and State Medicine.

President—Dr. RICHARD J. REECE, C.B.

Analogies between Influenza of Horses and Influenza of Man.

By Lieut.-Colonel A. J. WILLIAMS, D.S.O., F.R.C.V.S.

(Royal Army Veterinary Corps.)

(Introduced by the PRESIDENT).

I APPRECIATE the honour you have accorded me in inviting me to address your Section and I feel a special pleasure in doing so, being keenly interested in the question of collaboration between the two professions.

During my service in the various parts of the world, occasions have arisen in which such co-operation has been beneficial on both sides, and judging by recent events it will be more in evidence in the future to the mutual benefit of our professions.

In the time at my disposal I can only deal briefly with the subject and will endeavour to bring forward the salient features from the comparative aspect.

Equine influenza may be defined as an acute febrile, contagious disease, which manifests itself as a general blood infection, especially by catarrhal inflammation of the mucous membranes, inflammatory swellings of the subcutis and tendons, and a tendency to localization on the respiratory or gastro-intestinal mucosa.

Although historical records point to equine epizootics of this nature having occurred in various parts of the world from ancient times, the nature of the primary infective agent of equine influenza remains to be discovered.

Titus Livius gives hints of a Sicilian equine epizootic of this kind in 412 B.C., and accounts of similar outbreaks are given by Virgil, Columella, Absyrtus, and Vegetius.

From 1648, when horses of the French Army in Germany suffered severely, up to recent times, frequent outbreaks have occurred in various parts of the world with serious losses.

The affection existed in the large cities in a mild form in ordinary years, breaking out without obvious cause in certain years into an advancing epizootic which swept the whole continent. Such were the great outbreaks in Europe in 1881 to 1883, and in America in 1872 to 1873 and 1900 to 1901.

In the great American epizootic of 1872 to 1873 which prostrated 1,000,000 equines in the United States for one to two weeks and paralysed the agricultural and commercial activities of the continent for that period, the affection advanced gradually from Toronto over the whole continent of North America, where horses were kept, taking ten months to accomplish this.

The condition spread with great rapidity along the principal lines of communication, namely the railways, canals, and stage coach routes. The spread was more rapid towards the east than the west, the former being in the direction of the main equine traffic. The route of a travelling circus was traced by the trail of centres infected.

The island of Vancouver was not infected, as a strict quarantine on imported horses and mules was imposed. Prince Edward Island also escaped invasion as it was ice-bound and shut off from all traffic with the mainland. Cuba was infected by American horses landed at Havana, but no other West Indian island was attacked.

The southerly spread of the disease was finally arrested at Central America, where horses were scarce and horse traffic practically unknown. The affection failed to traverse any serious gap over which there was no movement of equine animals.

It is interesting to compare this historical record and the history of an outbreak in India, with which I especially wish to deal this evening, with the historical records of epidemics of human influenza.

With regard to the relationship between influenza in horses and influenza in man :—

During epidemics of human influenza in 1758 and 1775 medical men noted that many horses were affected with colds and coughs. Again, preceding the great epidemic in England 1889-1890 influenza prevailed among horses and Dr. E. Symes Thompson was so strongly impressed with the intimate connexion between equine and human influenza that in December, 1889, he wrote to the *British Medical Journal* calling attention to the prevalence of the equine epizootic and suggested that it would not improbably prove the forerunner of an outbreak in man ; but no such relationship has been established.

During the American epizootic in 1872-1873 there was no corresponding epidemic among human beings, and in 1889-1890 persons dealing with horses were not specially affected, whilst in certain places where influenza prevailed epidemically, no similar disease among horses occurred. My experience has been that no cases of influenza have developed among men attending cases of equine influenza and there appears to be little doubt that they are essentially distinct diseases. The difficulty of producing human influenza in animals by inoculation supports this view.

Causation.

Available evidence is in favour of a filter-passing organism being the prime factor responsible for the condition, and that its presence favours the invasion and infection of the body with other bacteria such as streptococci, diplococci, bipolar bacilli, &c., which give rise to the various types of the disease and set up the numerous complications observed. As far as I am aware Pfeiffer's bacillus has not been found in cases of equine influenza.

The most recent evidence appears to be in support of the view that human influenza is due primarily to a filtrable virus, and announcements that the germ has been isolated have been made from time to time, but it has not yet been fully established that the disease is due to an organism of this type. Until the primary infective agent has been discovered the prophylaxis and cure of such diseases cannot be carried out on a satisfactory basis.

I will now pass on to the outbreak in India which I had special facilities of observing.

This outbreak first appeared in Calcutta in the beginning of April, 1915, when there was an enormous influx of remounts from Australia. Large numbers were constantly arriving, and were dispatched as quickly as possible to various depots and mounted units all over India, as it was impossible to accommodate them in Calcutta. The disease spread with great rapidity wherever horses from Calcutta had been sent, from Calcutta in the east to Peshawar

in the north, about 1,500 miles, to Loralai in the north-west, about 1,250 miles, and to Bangalore in the south, about 1,000 miles. From July onwards the epizootic gradually died down and was practically over by the end of September. During these six months 16,921 cases were recorded, with 893 deaths.

At this time I was responsible for the veterinary administration of an area extending from headquarters at Poona to Loralai in the north-west, Madras in the south, and Secunderabad in the Deccan, including the ports of embarkation, Bombay and Karachi. The distance from Loralai in the north-west to Madras in the south is about 1,500 miles.

Of the seven principal stations in this area, animals from Calcutta were found to be affected on arrival at five, at one the disease appeared a week after their arrival and animals at the remaining station were infected through the medium of tonga ponies travelling from an infected centre fifty-five miles distant.

The first cases observed at Calcutta were of a mild type, in which the distinctive features of the disease were not exhibited, and they were diagnosed as ordinary cases of ship's catarrh. Owing to the extremely susceptible condition of the majority of the animals, due to their lowered vitality after completing a long sea voyage, the disease spread like wild-fire, and in so doing acquired a distinctly increased degree of virulence.

The disease was first suspected at Calcutta on April 3, but was not definitely diagnosed until April 7. By this time cases had been found in a train-load of horses from Calcutta on arrival at a remount depot in the Punjab.

The effect of a long and trying train journey on animals already enfeebled after a long sea voyage is well known, and it was therefore not surprising to find that deaths occurred during the journey. Some horses died immediately on detraining, while others, suffering from a very virulent form of the disease, tottered along almost pulseless and in a dying state.

Spread of the Disease.

The following examples of how the disease was spread are of interest:—

One hundred horses from Calcutta arrived at a certain station on April 14, and were immediately isolated three miles away from all other animals. A few cases of influenza developed within the course of a few days; these were of a mild type and the horses soon recovered. The whole batch was, however, kept in isolation for two months after the last case had occurred, and was then ordered to move to the regimental lines. Up to this time the regiment had been quite free of the disease. When the batch was being marched to the lines, one horse broke loose and ran into an isolation camp containing influenza cases. The Indian N.C.O. in charge of the party sent for the horse, but did not report the incident, with the result that the whole batch, including the runaway horse, was taken into the lines, where these animals were picketed apart from the regimental horses. Two or three days later this horse developed the disease, also the in-contacts. The whole batch was immediately moved out again to a camp six miles away; but the damage was done, and shortly after this cases appeared among the horses of the regiment and the disease gradually spread throughout them.

In another station the disease spread to various units, probably through the medium of orderlies' horses meeting at some centre such as the headquarter staff offices; and it is interesting to note that it spread along the main road from one end of the station to another; but one battery, whose lines were in an isolated position away from the other batteries and off the main road, escaped.

Direct transmission appears to be the chief method of spread in human beings and equines, and the usual mode of infection is by direct contact with those ill of the disease.

Infection by Coition.

Many observers have recorded instances in which the disease was transmitted from a stallion to a mare by coition, the stallion having apparently recovered from an attack. Cases are reported in which a stallion under the above conditions has proved a source of infection to mares even for months after convalescence.

In Wallis Hoare's "Veterinary Medicine" there is the following interesting note by Major Todd, A.V.C., in connexion with this mode of infection:—

> In the spring of 1907, the Netherlands Government bought a stallion in France for stud purposes. He was examined and passed sound, and arrived at his destination in the best of health. When put to the stud every mare mated with him developed influenza three or four days after coition. As he was a good-looking horse, with an excellent pedigree, it was decided to retain him till the following season. The result in this and the succeeding year was the same.
>
> Poels collected some of this horse's semen and inoculated several susceptible horses with it. A few days afterwards they exhibited the same symptoms as the mares. He then passed the semen through a Chamberland filter, and found the filtrate to be equally virulent. The blood of the infected stallion was now tested by inoculation and gave negative results, proving that the virus was confined to some part of the generative organs. This was also supported by the fact that susceptible animals placed in the same stable as the infected stallion did not develop the disease.

Types of Horses Affected, their Recent History, Condition, Age, Sex and Breed.

All types and breeds suffered, the initial outbreak being among the newly-landed, which were unacclimatized, many in poor condition, susceptible to disease, and with no power of resistance to an attack. Animals of all ages were attacked; in some cases very old animals were severely affected; aged horses exposed to infection when unacclimatized and in soft condition showed the same susceptibility to the disease as the young animals.

Condition and acclimatization were the chief factors determining the course of the disease; acclimatized horses in good hard condition developed the disease in a much milder form than young unfit horses. Country bred horses were not so severely affected as the imported horses.

Mules showed a considerable resistance to the disease; the percentage affected was low, the type of the disease mild, and the mortality almost *nil*—only one death occurred in 302 cases.

Sex did not appear to have any effect on the condition. Male and female animals were affected to the same extent, but in very severe cases with great loss of condition; mares took longer to recover.

Types of the Disease Observed, Symptoms, Course, Incubation Period, Mortality, and Post-mortem Appearances.

In every batch of remounts the disease was a typical epizootic cellulitis of varying degree, and the condition spread rapidly throughout the batch. The first few cases were often extremely acute, death occurring sometimes in a few hours, due to syncope from toxæmia. During its spread the virulence appeared to decrease, affecting horses in varying stages of condition and

acclimatization in varying degrees, the fit, acclimatized animal suffering least.

Catarrhal symptoms were present in all outbreaks, and in the case of seasoned horses they were often the principal symptoms observed.

In horses which were in good condition, and hard work, the disease was often so mild that the only symptoms were a rise of temperature for one to three days, slight œdema of the eyelids with lachrymation and anorexia for two or three days. In such cases there was no loss of condition.

In severe cases of the epizootic, cellulitic form in recently-landed horses the symptoms were typical of the disease. There was an initial temperature of 105° to 106° F., marked prostration, extensive effusion causing great swelling of the dependent parts, and in some cases the head was so badly affected as to interfere with respiration. There was swelling of the eyelids, with lachrymation and effusion into the anterior chamber.

In favourable cases there was a marked improvement in four or five days, the temperature falling gradually and the normal being reached in about ten days. In complicated cases, pyrexia existed for fourteen to twenty-one days. As a rule, an early improvement was followed by a rapid return to the normal state, except that in cases where extensive effusion occurred, œdema of the legs persisted for some considerable time, especially in the fetlock region of the hind legs.

The aged regimental horses were severely attacked, but the temperature dropped to normal in five to six days; the effusions were not so extensive, disappeared quicker than in remounts, and the period of convalescence was shorter.

Pneumonic complications in very bad cases of epizootic cellulitis were so extensive and severe that the condition was hopeless and the termination rapidly fatal.

Hydrothorax with hydropericardium caused sudden deaths from syncope. In cases not so rapidly fatal extensive pleuro-pneumonic lesions with gangrene of the lungs reduced the animals to living skeletons in a few days, necessitating their destruction. In horses dying in a few hours from toxæmia the most striking lesion was the enormous effusion into the tissues, which were bathed in serum. Among one batch of horses the first case was one of this kind; on removal of the ear for bacteriological examination as a precautionary measure owing to the symptoms simulating acute anthrax, serum poured from the cut surface.

Compared with human influenza there is the same sudden onset, short period of incubation, many symptomatic phases, prevalence of catarrhal symptoms, similar effects on the heart, blood, respiratory tract, and digestive system.

There is no comparative immunity from complications among young animals as I understand is the case in influenza in children.

Mortality.—In one batch of eighty-eight remounts there were nineteen deaths and destructions, a mortality of over 20 per cent., but this was exceptional. The average was from about 1 per cent. in acclimatized horses to 10 per cent. in remounts.

The mortality amongst horses, mules and donkeys respectively was as follows:—

	Cases	Deaths	Percentage
Horses ...	16,045	886	5·52
Mules ...	804	5	0·62
Donkeys ...	72	2	2·08
Totals	16,921	893	5·28

Period of Incubation.—The period of incubation was from three to five or seven days, but occasionally it extended to ten days.

Hygienic Measures.—Difficulties were experienced in checking the spread of infection, even when immediate isolation was enforced and all preventive measures were adopted. The only satisfactory method of control was complete isolation, in the earliest stage of the outbreak, of the sick and in-contact animals with their attendants, and when this was carried out in a thorough manner the number of cases was limited, but where half-measures were instituted the rapid spread of the disease was checked, but not completely controlled. The best results were obtained by moving the unit out of the permanent lines and treating the cases in the open, utilizing any shade available.

Infection spreads far less readily in the open than in stables, and evacuation of the lines allowed of thorough measures of disinfection being carried out, so that they were ready for occupation when the disease had subsided. In one infected regiment the whole of the animals were sent into a segregation camp three miles from cantonments. After four days in camp no fresh cases occurred. In another regiment so situated that it was not possible to move then into a segregation camp, affected animals were removed from the lines and isolated, but the disease did not disappear from the unit for over three months.

Treatment.

The chief factor in the successful treatment of this disease is good nursing, and in this respect the provision of a suitable diet is one of the most important features. Fresh air, shade, and a plentiful supply of water are essential.

With regard to the administration of drugs. The general opinion was that the simplest were the best and those capable of being given in the drinking water were most suitable. In the majority of cases salines were given in this way but animals affected with the catarrhal form were also given electuary and medicated inhalations. Drenching was never practised. In cases with hyperpyrexia a combination of quinine sulphate, sodium salicylate and ammonium carbonate, gave excellent results. This treatment was usually only necessary for a few days.

In severe cases with extensive effusion potassium iodide in the early stages, to assist absorption and prevent permanent thickening of the limbs, was found beneficial.

In cases showing signs of collapse hypodermic injections of strychnine proved very useful. The intravenous injection of normal saline solution was tried in some cases without much effect. Atoxyl treatment was fairly successful but not sufficiently so to warrant its general adoption. At one remount depot the intravenous injection of quinine acid-hydrobromide (30 gr.) gave excellent results.

The importance of a suitable diet has already been mentioned. In mild cases, where the appetite was retained, little change was made in the diet, but in severe cases where the appetite was capricious and animals refused all grain, an unlimited supply of green lucerne was provided where possible, and was readily eaten. In twenty severe cases in which the horses showed great prostration, refused all food, and were some of them too weak to stand, they were given milk, eggs and alcohol. Three of these animals died but the remainder recovered and were undoubtedly saved by this treatment.

The grouping of cases into four classes, viz.: severe, mild, convalescent,

and in-contact, was found to be extremely helpful. This procedure allowed thorough supervision of treatment, nursing, exercise, &c., the cases being moved into various groups according to their condition.

Nothing short of the strictest isolation prevented the spread of the disease, and it appeared that, to be effective, segregation camps should be at least a mile from any unit. There are so many loopholes in connexion with prevention of spread in this disease that it was unusual to find it confined to any one area once the disease had obtained a footing. In order to guard against spread of the disease by recovered animals acting as carriers, no movements were allowed until six to eight weeks had elapsed from the time animals became convalescent.

As in human influenza the treatment is expectant, palliative and symptomatic. The importance of nursing and good general hygiene is the same in dealing with the disease in equines as in human beings. The dietetics which apply to any febrile disease apply equally to human influenza, and the same holds good in equine influenza. There is no specific for the condition, nor up to the present is there any means of securing immunity against infection.

With regard to vaccine treatment. It is stated that little can be said regarding its efficacy either as a prophylactic or curative agent in human influenza; it is principally of use in controlling the secondary infections, and the same applies in equine influenza.

Immunization.

Poels made further observations on the blood of recently infected animals, and found that the virus was present during, and for a short time after, an attack of the disease, and that susceptible horses inoculated with small doses of virulent blood passed through a mild form of the disease. They were afterwards immune to large doses and to natural infection. Although the virus soon disappears from the blood stream, it is apparent from the above that it may remain in a state of virulency in certain glands in the body with no ill-effects on the general health. Such animals may become "carriers" of the disease for an indefinite period, but it is only when the secretion of the infected glands comes in contact with susceptible horses that the latter become infected. Poels afterwards inoculated large numbers of horses against the disease without fatal result. A reaction is noticeable two or three days after the inoculation. It is accompanied by a mild fever, inappetence, and swelling of the eyes and limbs in varying degrees of severity.

Colonel Todd gives the following note as to the results of immunization:—

In summer of 1906, 400 remounts from Ireland sent to the Dutch Remount Depot were given 5 c.c. of virulent blood from a case of influenza as soon as they had recovered from the voyage. All developed typical symptoms of influenza in varying degrees of severity in from two to five days after the inoculation. No deaths. No treatment. All loose in paddocks. Only untoward result of the inoculation was loss of condition in 10 per cent. Animals afterwards transferred to army units, only two died from pneumonia compared with severe losses from pneumonia in previous years.

Prophylactic measures are similar: strict isolation, the prevention of spread of infection through infected discharges, and disinfection.

Law states that

"during the great outbreak of 1872-1873 when germ-potency and all but universal susceptibility were so remarkable, effective quarantine showed the most signal success, in the resulting immunity of Vancouver Island, Prince Edward Island, the whole of the West Indies except Cuba, Central and South America, and an isolated district in Mexico."

Disinfection.

In paddocks and open standings the action of the powerful Indian sun undoubtedly had a far-reaching effect. Fire was used wherever possible. Standings, mangers, troughs, buckets, &c., were all thoroughly disinfected in this manner, i.e., by burning litter or by the use of a blow lamp. In segregation camps burning litter over the standings and incineration of the manure were preventive measures adopted for the destruction of the contagium.

Disinfection by fire was found to be both effective and inexpensive; there was no necessity to dig up the standings and to carry out a more expensive method of disinfection. When horses were being constantly railed over the country, the importance of thorough disinfection of the railway trucks and watering troughs at halting places will be realized. Clothing, grooming kit, &c., was disinfected in a solution of cresol or perchloride of mercury.

Duration of the Disease and Period of Convalescence.

In newly-imported horses the duration of the disease was about fourteen to twenty-one days; in acclimatized, conditioned horses, about a week. In the former the period of convalescence was from fourteen to twenty-one days; in the latter ten to fourteen days.

Convalescence could not be hurried, and it took months of care and feeding to get debilitated newly-imported horses into condition. Running loose in paddocks with a plentiful supply of water and green food, and shade if possible, were the best conditions for bringing convalescents to a state fit for quiet work. The time the disease existed in units varied from six weeks to over three months.

Susceptibility of Types and Immunity Conferred by an Attack.

All classes of animals are equally susceptible. The immunity conferred by an attack appears to be strong. With one exception, reports from all veterinary officers were unanimous in stating that no second attack was observed. In the one exception it was stated that certain horses which had been discharged as cured were again affected with the disease six weeks afterwards. Many horses appeared to have considerable natural immunity, and although it is doubtful whether any second infection occurred after a complete recovery from an attack, in a small percentage of cases relapses occurred during the early convalescent period.

It is interesting to compare this with immunity in human influenza where there is a short-lived immunity in some cases, but usually an attack seems to predispose to a second or third visitation of the disease.

Complications and Sequelæ.

Pneumonic complications were the chief cause of the mortality. A few cases of very severe cellulitis were seen, with gelatinous degeneration of the tissues in the fetlock and coronary region; and occasional cases of pyæmia, synovitis or arthritis, with permanently enlarged joints, were also observed.

The chief sequelæ were lymphangitis (usually of one hind limb), chronic debility, laminitis, broken wind, and fatty degeneration of the heart, with hypertrophy and dilatation. In some cases in which the horses were destroyed for chronic debility old pleuritic lesions were found. Paraplegia and roaring were also noted. Purpura hæmorrhagica, pericarditis, endocarditis, iritis,

enteritis, paraphimosis, abortion, and shedding of the hoof were among the complications met with.

An interesting fact generally observed was that nearly all wounds seen on animals affected with the disease became, to a varying extent, the seat of acute cellulitis, causing severe pain. The wounds healed very much more slowly than in healthy animals and thus often seriously prolonged the period of convalescence.

Taking into consideration the number of animals affected, the deaths and destructions on account of sequelæ of an incurable nature were small.

REFERENCES.

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DISCUSSION.

Dr. R. J. REECE (President) said that Hirsch had stated there could be no question of the relation of influenza epidemics in man to epizootics of the same character prevailing at the same time, especially among horses, and, next to them, among dogs, cats, and the like. In the oldest epidemiological records there were indications of these coincidences both as regards time and place, as well as of the identity or at least similarity of the form of the disease; and the number of these observations was so remarkably large that the suggestion of an ætiological and perhaps also pathological connexion between the epidemics on the one hand and those epizootics on the other, might be regarded as provisionally proved. From the descriptions, however, that had been given of "horse influenza," it would seem that various diseased processes appeared to have been included under this term.

He (the President) did not gather from Colonel Williams' paper that, at the time of the epizootic among horses that he described, influenza had been present at the same time in epidemic form among man either in Calcutta, on the transports, or in Australia. Colonel Williams had been able to fix the incubation period as not less than three days. The disease broke out among the horses shortly after they had arrived in Calcutta. The matter of keeping horses in segregation at Calcutta would then be questionable. This would lend support to the suggestion that the infection had been acquired in Calcutta, and that the horses were in a condition which would render them peculiarly liable to contract disease. Still, it would be of interest to know whether there had been sickness among the horses while on board ship, and whether any like illness had occurred among the horses preceding shipment in Australia, and whether any comparable illness had occurred among the crews of the transports or among the personnel, grooms and others, whose duties brought them into close association with the horses either on board ship or on shore. If any like illness in man was associated with the sickness among the horses it would be one step on the path of knowledge leading to a solution of the problems of allied infection in man and in animals.

Although it was believed that the infection of influenza in the human was mainly spread by close association of one person with another, yet there were authenticated instances which could not be explained on this ground, and it would be of interest to know whether any like occurrences had been observed in connexion with "horse-influenza." Colonel Williams had pointed out that segregation camps should be located at least a mile from any unit.

There were certain cases on record in which influenza had appeared among the crews on board ships on the high seas which had not previously communicated with an infected shore, and at a time when influenza was generally prevalent on land nearest to the position of these ships. One such instance occurred in September, 1781, when the crew of an East Indiaman were attacked by influenza, and hardly any of them escaped, when on a voyage from Malacca to Canton. It was stated that at the time

the vessel left Malacca the disease had not been prevalent there, but when they arrived at Canton it was found that the outbreak on board the ship while she was in the China Sea had occurred at the very time that the disease was manifesting itself with great intensity at Canton.

In the Report on the Health of the Navy for the year 1857 [p. 41], Dr. A. Bryson, recording the occurrence of influenza on board an English ship of war, wrote that "this affection first made its appearance while the vessel was cruising off the coast of Cuba, with which, however, she had no communication;" the greater part of the crew fell ill with influenza, "but it was subsequently ascertained that it was prevalent" during the very same time at Havana, in Trinidad, and in the West India Islands. He added: "There was nothing in the state of the atmosphere to attract special attention; a question therefore arises, whether it might not have been caused by infection wafted from the shore. There are instances on record, improbable as it may appear, which favour this view of the case." Dr. Bryson had written that sixty-seven years ago, and we were not yet in a position to explain what part the atmosphere might play in influencing epidemics of disease.

Dr. MAJOR GREENWOOD said that a possible relation between influenza of horses and influenza of man had long been suspected and the former disease had been recognized for many years. Whether indeed one could safely go so far back as Colonel Williams did, to classical times, might be doubted. He (Dr. Greenwood) thought his reference to Virgil must be to the Third Book of the *Georgics* (lines 480 *et seq.*) where the poet described a general pestilence affecting animals and was perhaps indebted to both Thucydides and Lucretius for points in his description. The poet did, however, particularize the symptoms of horses, and part of his account might be quoted:—

> Tum vero ardentes oculi atque attractus ab alto
> Spiritus, interdum gemitu gravis, imaque longo
> Ilia singultu tendunt; it naribus ater
> Sanguis, et obsessas fauces premit aspera lingua.

Both ocular symptoms and dyspnœa were consistent with Colonel Williams' account but hardly the hæmorrhage from the nostrils. Hirsch considered the first trustworthy account to be that of Molineux in 1693, since whose time, as Colonel Williams had noted, there had been many accurate studies. It was interesting to notice that in the great American outbreak of 1872-3, the fatality—according to Hertwig cited by Hirsch—was 3 to 4 per cent., not so very much less than in the Indian epidemic studied by Colonel Williams. Much time might be spent in defining the terms "same" and "different" when used to qualify the noun "disease": in the ordinary usage of words, he thought that the non-communicability from horse to man entitled them to say that horse influenza and influenza of man were different diseases but that in their behaviour as epidemics they were so similar that the study of one threw light upon the other. Colonel Williams' paper was, therefore, a valuable contribution to general epidemiology and he believed that all Members of the Section would look forward to other contributions from him and from his colleagues. The Section would welcome epidemiological studies by veterinarians of, for instance, swine fever and foot-and-mouth disease. Their veterinary colleagues had opportunities for epidemiological study not enjoyed by Members of the Section, who, on the other hand, had some sources of information not open to veterinarians. Evidently co-operation was necessary and Colonel Williams had created an important precedent.

Professor HOBDAY stated that although he had seen many hundreds of cases of influenza in horses he had never known an authenticated case of its transmission to man or animals other than those of the equine tribe. Papers like that just read by Colonel Williams were of particular value when presented before a mixed assembly of medical men and veterinarians, as showing the analogies and differences between a disease to which the same name was given; although in reality, except in its symptoms and manner of spread, influenza of men and influenza of horses were not in any way allied in so far as any common causal organism was concerned.

Major-General Sir JOHN MOORE said that Colonel Williams had traced up the particular outbreak originating in Calcutta in an able and lucid manner. He, personally, could see no connexion between the influenza of humans and the influenza of the horse. He had had considerable experience in the remounting branch of the Army in India, in the United States, and in this country, and he could not recall to mind influenza in humans resulting from the equine ailment. Catarrhal sickness in animals on board ship certainly existed without similar prevalence amongst the attendants. He could quite understand the extent to which equine influenza gained ground in dealers' stables and other locations of horses in Calcutta and Bombay, for such places were usually not of a hygienic order. With regard to equine influenza and catarrhal diseases during the war, great trouble and severe mortality had been experienced amongst the heavy classes of horses in France in the early days of the war before adequate shelter for remounts could be provided. It was well known that that class of horse could barely be moved from its accustomed stable or habitat without coughs or catarrhal affections ensuing, and careful measures had to be adopted accordingly. He was glad to see a combination between the Sections of Epidemiology and Comparative Medicine. Such combination was most apt: it embraced a common field, and he hoped more similar gatherings would be arranged.

Sir WILLIAM HAMER said that the Section had always been interested in epidemic disease affecting the lower animals and a paper on influenza in the horse was especially welcome. Comparison between the accounts given of influenza in man, by Franklin Parsons, and of influenza in the horse by D. E. Salmon, the great American authority, showed, epidemiologically speaking, remarkable similarities. In both cases there was insistence upon the stupefaction of the brain and nervous system, the great depression of the vital forces, and the liability to complications affecting the lungs, intestine, brain, heart, &c.; both writers, moreover, laid special stress on the fact that at certain seasons the gastro-intestinal form of the disease might prevail epidemically, and Salmon remarked that "while inflammatory changes of the brain more rarely commenced in epizootic form they were to be found in a percentage of cases in all epizootics."

Study of the relationships between human and horse influenza should not be merely limited to such questions as that of possible cross-infection from horse to man or man to horse; epidemiologically speaking, examination of the inter-relationships between epidemics was of much greater importance. In the case of human influenza it was known that "there were concurrences, similarities and inter-relationships between outbreaks of cerebro-spinal fever, poliomyelitis and outbreaks of influenza, bronchitis and pneumonia." The question as to possible similar inter-relationships in horse influenza therefore arose.

A point deserving of note was the fact that the epidemiographical records showed, in most instances, that outbreaks of influenza in animals had either preceded or followed, rather than had been coincident with, pandemic influenza in man. They had concurred with the atypical forerunners, or with the "trailers" following upon a pandemic human influenza, rather than with that prevalence itself. The American epizootic of 1872-73 was a case in point, and so of course was the Indian epidemic of 1915. The veterinary profession was to be congratulated upon having insisted for centuries upon the important influence exercised by immunity in connexion with epizootics. Colonel Williams had referred to this in his discussion of the Indian prevalence. When Charles Darwin was writing about immunity years ago he had had to appeal to authorities on animal diseases for examples. He cited for example an old Chinese writing, and again he referred to Columella, whom Colonel Williams mentioned at the outset, as having noted that the native born beast was immune to diseases which affected imported animals. There had been a tendency of late years, on the part of the medical profession, to stress the seed rather than the soil, but perhaps immunity would come into its own again in the fullness of time.

Dr. F. PARKES WEBER thought that, from the parallelism of the symptoms, influenza in horses must be closely analogous to influenza in human beings, though, he understood, the two were not pathologically identical. In regard to occasional resulting

changes in the hoof, he supposed they must be analogous to the nutritional changes in the nail-bed that not rarely occurred as a result of severe pneumonia and other severe diseases in men, and led to transverse grooves in the nail, from the position of which the approximate date of a recent illness could be estimated. Sweating and miliarial eruptions, which were not a feature in horse-influenza, were in human cases, he believed, often due to want of washing, dirty bed-clothes, stuffy rooms and salicylic compounds. He wondered whether the œdema sometimes occurring in horse-influenza was mostly due to local inflammatory changes in the subcutaneous tissue (or in the muscles also—a kind of "dermatomyositis"), or was non-inflammatory œdema—of cardiac, renal or localized thrombotic origin.

Section of Epidemiology and State Medicine.

President—Dr. RICHARD J. REECE, C.B.[1]

A Survey of the Mortality due to Childbearing in London from the Seventeenth Century.

By REGINALD DUDFIELD, O.B.E., M.B.

THE data for this survey have been taken from the London Bills of Mortality and the Annual Reports of the Registrar-General. It is natural, therefore, to divide the years covered by the survey into two periods, the first ending in 1830—taken from the Bills, and the second beginning with 1837—the first year of civil registration.

1629-1820.

The date of issue of the first weekly Bill is uncertain. Creighton found an undated Bill which he identified as having been published in 1533. It seems to be not unreasonable to conclude that parish registers of burials were kept—in London at least—before the issue of Thomas Cromwell's "Injunctions" in 1538. One of the requirements of those Injunctions was the keeping by every parson, vicar and curate of true and accurate registers of weddings, christenings and burials. Given such registers, the compilation of Bills of Mortality was possible in London through the agency of the Confraternity of St. Nicholas, otherwise the Company of Parish Clerks.

According to Graunt, the regular issue of weekly Bills began in 1603 (December), and the first annual Bill was published in 1604. The Bills were published in MS. until 1625, when the Company of Parish Clerks were authorized to establish a printing press.

The first yearly Bill containing the number of christenings is that of 1606, i.e., for the period December, 1605, to December, 1606. The ages of the persons buried were first given in the Bill for 1629, that Bill also containing an analysis of the reported causes of death—"Diseases and Casualties." In 1647, the sexes of the persons christened and buried were added, but not in combination with the causes of death.

The Registers kept by the Company of Parish Clerks from December, 1664, are in the Library at the Guildhall. Three collections of reprints of the yearly Bills are known to me, viz.:—

Graunt's collection, first published in 1662, containing the Bills for the years 1629-36 and 1647-60;

An anonymous collection—attributed by some to Birch—published in 1759, containing the Bills for the years 1657-1758; and

Marshall's, published in 1831, giving the complete series for the years 1629-36 and 1647-1831.

The last-named includes tables of christenings and burials dating from 1604.

[1] [Died April, 1924.]

Although Graunt made considerable use of the statistics drawn from the Bills—including the construction of a life-table—he was the first to write disparagingly of the material thence derivable. Ogle was even more emphatic in condemning the material as untrustworthy. It may seem, therefore, somewhat presumptuous on my part to venture to submit any figures from this source, but I hope to show that the Bills are not, for certain uses at least, altogether worthless.

The Bills, undoubtedly, do not give the full tale of christenings and burials. Being based on the numbers of each function recorded in the registers of churches belonging to the Established Church only, christenings and burials of Roman Catholics and Dissenters were not included in the Bills, although a few christenings of Dissenters may have been. No material exists now for estimating the proportion of the inhabitants of London who were outside the pale of the Established Church.

A further cause of deficiency was the practice, which increased as the years went on, of taking bodies for burial in cemeteries beyond the limits of the Bills. That deficiency was, in all probability, neutralized in part by the bringing of bodies of persons dying without the Bills for burial (e.g., in family vaults) within the Bills.

It is curious that the deficiencies referred to were estimated by Graunt to be in each instance equal to one-sixth of the numbers recorded. How he arrived at that estimate is not disclosed, but Birch (?) apparently accepted it as a sufficient approximation. Whatever the deficiencies were, I doubt whether they materially affect the values of the ratios I propose to submit.

Apart from the question of deficiencies in numbers recorded, the material furnished by the Bills suffers from a more serious defect when consideration is given to the "Diseases and Casualties." I may remind you of the methods employed for securing the particulars necessary for these entries. On a death becoming known either by an order for a grave or by the tolling of the "passing bell," the "searchers" visited the house of the deceased to make inquisition of the cause of death. These "searchers" were women who might be termed ancient, selected by the authorities and sworn to their office. They never professed to any knowledge of medicine and, according to Graunt, were not immune to the soothing influence of either a gratuity over and above their statutory fee (a groat) or the inspiriting effects of a draught of ale or something stronger. Graunt's description brings to mind the characteristics of that worthy dame yclept "Mrs. Gamp." Doubtless in many instances the cause of death reported by the searchers was that communicated by the deceased's medical attendant, but I fear that even he, judged by present-day standards, would not rank high as a diagnostician. In other instances, I imagine the searchers would report as the cause of death what was acceptable to the deceased's relatives, or the fashionable complaint of the day. I doubt whether any reliance can be placed on the figures for diseases requiring medical skill to diagnose, except such complaints as plague, small-pox and a few others. On the other hand, I think that it may be safely assumed that a woman of the type of Mrs. Gamp can be trusted to know whether a woman died in childbed, even if she could not discriminate between the various "diseases and accidents of pregnancy and childbirth."

After considering all the pros and cons, I have come to the conclusion that the numbers recorded in the Bills of Mortality furnish a fair approximation of the fatality "in childbed," not of the fatality "childbearing," as "abortion, miscarriage" did not appear in the Bills until 1709. On the assumption that the deficiencies in the numbers of burials and christenings are proportionately

equal, the only uncertainty which exists is that attaching to the cause of death reported by the searchers, with which I trust I have dealt successfully.

From the data contained in the Bills I have calculated ratios (per mille) of deaths of women in childbed to christenings (from 1629) and to all burials of females (from 1657) for each year down to 1829. Considerations of space prevent me from including tables showing the ratios for each year, and I am, therefore, submitting quinquennial averages only for the period 1660-1829 prefaced by some remarks dealing with those for the years 1629-59.

The tabulation of quinquennial averages has been dated from 1660 for two reasons, although burials of females were recorded separately from 1657. In 1660 the number of parishes included in the Bills was increased from 123 to 130 and that year was further signalized by a marked change in the " run " of the ratios, a change sufficient to warrant a new datum for observations.

Owing to the loss of the records for the years 1637-46, it is necessary to divide the period 1629-59 into two sub-periods, 1629-36 and 1647-59. During the eight years 1629-36 the christenings averaged 9,592 per annum and the deaths in childbed, 157. The mean ratio for the period was 16·39 per 1,000 christenings. The ratios calculated for each year were :

1629	...	...	15·15	1633	...	...	13·20
1630	...	...	16·85	1634	...	...	14·50
1631	...	...	13·13	1635	...	...	16·22
1632	...	...	17·84	1636	...	...	24·15

During the years 1647-59 the christenings averaged 6,376 per annum, and the deaths in childbed, 182. The mean ratio was 28·78 per 1,000 christenings. There was apparently a continuous rise in the ratio of deaths to christenings during the thirty-one years (1629-59) under review. That is brought out by dividing the twenty-one years for which data are available into three periods of eight, eight and five years, the only practical division owing to the gap 1637-46.

	Annual Averages.		Mean Ratios.
	Christenings.	Died in Childbed.	
1629-36	9,592	157	16·39
1647-54	6,286	158	25·19
1655-59	6,520	203	32·66

The ratios calculated for each of the thirteen years 1647-59 were :—

1647	...	...	21·95	1654	...	...	29·00
1648	...	...	16·19	1655	...	...	25·27
1649	...	...	19·57	1656	...	...	28·51
1650	...	...	20·84	1657	...	...	35·30
1651	...	...	33·93	1658	...	...	36·30
1652	...	...	34·75	1659	...	...	39·71
1653	...	...	25·67				

To illustrate the change in these ratios which took place after 1659 the ratios for the next five years are set out here.

1660	...	...	27·83	1663	...	...	20·01
1661	...	...	25·29	1664	...	...	21·24
1662	...	...	17·46				

The mean ratio was 21·91, a decrease of nearly 33 per cent. below the mean for the preceding five years.

In Table I, I present the quinquennial mean ratios per 1,000 of deaths in childbed to all burials of females and to christenings for the years 1660 to 1829. The former ratio fell during the 170 years from 25·82 per 1,000 to 21·53, the latter from 21·91 to 8·78. To continue the series to the latest possible date, corresponding ratios for the years 1840-1919 have been included in the table, such ratios being calculated from the Registrar-General's Annual Reports.

As regards the ratio to all burials of females, the highest recorded during the whole period was that of 1666, 40·47 per 1,000, that for 1671 (36·11) being next in order. The highest ratio to christenings was that of 1665 (62·70 per 1,000). Presentation as quinquennial mean ratios has resulted in smoothing, which is to be regretted.

That the ratios given in Table I do represent very approximately the fatalities of the periods covered by the table, is, I think, confirmed by statistics published recently by Kisskalt, who had access to unpublished archives of the City of Königsberg. Caution is necessary in comparing his figures with those I am submitting on account of the fact that Kisskalt's ratios are based on the numbers of births after inclusion of still-births. The exclusion of still-births would make Kisskalt's ratios higher.

The fatality from childbirth, in Königsberg, during the years 1770-1802, was at the rate of 10·6 per 1,000 births (and still-births), with a maximum rate of 23·0 (in each of the years 1775 and 1776) and a minimum of 5·0. According to the data contained in the Bills of Mortality the mean ratio for London during 1770-1802 was 10·1 per 1,000 births, with a maximum of 15·8 (in 1770) and a minimum of 6·9.

Kisskalt quotes certain other ratios observed in German cities which are set out in the appended table together with the London ratios for the same periods.

City	Period	Ratio (*a*)	Ratio (*b*) London
Berlin	1746	9·3	12·9
,, ...	1757	9·8	12·4
,, ...	1758-63	10·7	15·8
,, ...	1764-74	12·2	12·3
,, ...	1785-94	7·0	10·3
Leipsig	1759-63	18·3	16·6
,, ...	1764-68	15·9	13·0
,, ...	1769-74	12·7	10·4

(*a*) Per 1,000 births including still-births. (*b*) Per 1,000 births excluding still-births.

1837-

In passing to the records of civil registration a regrettable gap occurs in the series of statistics. Although the issue of the Bills of Mortality was continued until 1848 I have not been able to learn the whereabouts of any Bills to continue the table (ended in 1831) compiled by Marshall. I believed that Ogle had access to those Bills.

The data for the half year July to December, 1837, and those for the following two years are presented in the Annual Reports of the Registrar-General in such a form as to be almost useless for my present purpose. I have been compelled, therefore, to date the beginning of my survey of registration figures from 1840—thus leaving, for the time being only, I hope, a gap of ten years (1830-40).

Before proceeding to consider the data available it is desirable to call attention to two facts which undoubtedly materially affect a comparison between the rates of fatality furnished by the Bills of Mortality and those deducible from civil registration. The first is the fact that "London" in the Annual Reports of the Registrar-General was, even at the beginning of the last century, a much larger area than that covered by the Bills of Mortality. In 1837, when the extension of area took place, many of the districts taken in were practically rural in character. I am unable to say how, or to what extent, the addition of the data from those areas affected the fatality rates.

The second fact, which did not become operative until within the last fifty years, is the decline in the birth-rate, coupled with later age at marriage. This

fact will tend to reduce the ratio of deaths due to pregnancy and parturition to all deaths of all females and to raise the ratio of deaths to births, by reason of the higher fatality among women who bear children for the first time at more mature ages. Evidence of the latter will be given later on.

It may be urged that, having regard to the difference in the method by which the statistics were originated, viz., compulsory registration as against more or less voluntary compilation, the resulting statistics are so dissimilar as to be unsuited for comparison. It is, I think, admitted that registration during the years following its inception was far from complete, a fact which would very materially depreciate the value of rates based on population, but will not, in all probability, notably affect ratios based on numbers derived solely from registration. In other words, if the registration data be regarded as drawn from a sample of the population—as in effect were the data from the Bills of Mortality—we can, I think, accept both sets of data as giving pictures of the case, if they do not state the whole case.

Beginning in 1685, or thereabouts, the ratios of deaths in childbed to deaths of females at all ages, and to christenings, showed an almost continuous decline. Bearing that fact in mind, the following comparison of the annual ratios during the years 1820-29 and 1840-49 lends support to the view that the ratios deduced from civil registration are—to put it at its lowest—very fairly comparable with those deduced from the Bills of Mortality.

DEATHS IN CHILDBED: RATIOS (PER 1,000) TO

Year.	DEATHS OF FEMALES—all ages.		Year.	Year.	CHRISTENINGS; BIRTHS.		Year.
1820 ...	21·56	15·05 ...	1840	1820 ...	8·88	6·07 ...	1840
1821 ...	22·26	15·12 ...	1841	1821 ...	8·00	5·91 ...	1841
1822 ...	20·35	14·01 ...	1842	1822 ...	8·17	5·22 ...	1842
1823 ...	20·03	15·52 ...	1843	1823 ...	7·33	6·00 ...	1843
1824 ...	17·47	13·95 ...	1844	1824 ...	6·56	5·44 ...	1844
Mean ...	20·33	14·73 ...	Mean	Mean ...	7·78	5·72 ...	Mean
1825 ...	21·07	16·89 ...	1845	1825 ...	8·38	6·10 ...	1845
1826 ...	20·28	18·33 ...	1846	1826 ...	9·39	6·38 ...	1846
1827 ...	24·62	17·29 ...	1847	1827 ...	9·02	7·44 ...	1847
1828 ...	18·49	20·17 ...	1848	1828 ...	7·38	8·01 ...	1848
1829 ...	22·93	14·57 ...	1849	1829 ...	9·76	6·94 ...	1849
Mean ...	20·77	17·45 ...	Mean	Mean ...	8·78	6·97 ...	Mean

The quinquennial mean ratios for the years 1840-1919 are given in Table I. The fall from the rate (per 1,000 births) from 5·72 during 1840-44 to one of 3·22 during 1910-14, represents a reduction of 44 per cent. The maximum mean rate 6·98 was recorded during 1845-49, and the minimum (2·99) during 1910-14, the difference representing a reduction of 57 per cent. of the former rate.

Causes of Mortality.—In the Bills of Mortality "childbed" is the only entry recognizable as a cause of mortality due to pregnancy and parturition. It is true that "miscarriage" (one death) is mentioned in the Bill for 1709, but that entry did not appear regularly until 1723. The maximum number of deaths assigned to miscarriage in any year was seven, in 1733, so such entry is of little statistical value. When recorded, the deaths from miscarriage have been added to those from childbed.

In the Annual Reports for 1837-42 "childbed" is again the only entry available. When the tabulation of deaths by causes was resumed in 1848, distinction was made between "metria" and "childbed." The term "puerperal fever" was first used in the report for 1850, being given as a synonym for "metria," the latter designation disappearing from the reports in 1881.

In 1881 Farr's classification was adopted and puerperal fever appears under

the "*Specific Febrile*, or *Zymotic, Diseases: Septic Diseases*" and the other causes of death during pregnancy and the puerperium were tabulated under six entries:—

VI Local Diseases: 8, Reproductive system, (*b*) Diseases of Parturition—

Abortion, miscarriage	Placenta prævia, flooding
Puerperal mania	Phlegmasia dolens
Puerperal convulsions	Other accidents of childbirth

In 1901 Farr's classification was given up, diseases being divided into "General" and "Local." Puerperal fever was placed under the former head and subdivided into four entries, viz.: Puerperal septicæmia—puerperal septic intoxication—puerperal pyæmia—phlegmasia alba dolens—and puerperal fever (not otherwise defined). Puerperal diseases of the breast were included with puerperal pyæmia.

The "other causes" were tabulated under five entries, viz.: Abortion, miscarriage—puerperal convulsions—puerperal mania—placenta prævia, flooding—other accidents of pregnancy and parturition.

In 1901 the International Schedule of Causes of Death was adopted as a basis of classification. The subdivision of puerperal fever was given up, "phlegmasia dolens" was made a separate entry, and "placenta prævia, flooding," and "other accidents" subdivided.

Tabulation by ages in conjunction with cause of death was intitiated in 1851.

It appeared to me that in the absence of any published figures of the age-distribution of married women for years other than those in which the census was taken, it was desirable to arrange quinquennial periods with the censal and mid-censal years central to each quinquennium. By such arrangement, I hoped to obtain more trustworthy rates than I could expect to reach by estimating the numbers of married women for the quinquennial periods more generally used (e.g., 1851-55). The tabulation of the calculated mean rates begins, therefore, with the triennium 1851-53—since the census of 1841 did not give the ages of married women—and thereafter the same rates for each quinquennium to 1918. (See Table II.)

To complete the figures the annual rates (total fatality) for the years 1841-50 have been taken out, together with the mean rates and also the fatality rates from "sepsis" and "other causes" for the years 1847-50.

PER 1,000 BIRTHS REGISTERED.

	Total fatality		Total fatality	Fatality due to: Sepsis	Fatality due to: Other causes
1841 ...	5·91	1846 ...	6·38 ...	— ...	—
1842 ...	5·22	1847 ...	7·44 ...	4·14 ...	3·28
1843 ...	6·00	1848 ...	8·01 ...	4·49 ...	3·51
1844 ...	5·44	1849 ...	6·94 ...	3·52 ...	3·41
1845 ...	6·10	1850 ...	5·91 ...	2·64 ...	3·27
Mean ...	5·73	Mean ...	6·92 ...	— ...	—
		Means 1847-50 ...	7·08 ...	3·70 ...	3·38

There is one peculiarity about the rates shown in Table II which calls for comment. Between 1851-53 and 1884-89 the tendency in the rates of fatality and other causes was towards a reduction, but in 1899-1903 there was an increase followed again by a downward tendency until 1914-19. It was in 1901 that the classification was altered to the lines of the International Schedule. Was that change the cause of the increase?

In Table III the mortality rates per 1,000 married women both for the one age-group 15-50 and for five sub-groups are shown, together with the birth-rates.

From 1881 onwards the indefinite group, "Other Causes," can be subdivided into five definite groups. The subdivision is limited to that number in consequence of the changes in tabulation made in 1901 and 1911. In Table IV the total numbers of births and deaths in each period are given, together with the fatality rates per 1,000 births registered for the forty years 1881-1920. In Table V the mean annual numbers are set out with the proportions (percentages) of the numbers under each head to the totals from "Other Causes." The most striking feature of the two tables are the increases in ratios of deaths to births from "P. Convulsions" and "Phlegmasia a. d." and the proportions of deaths from those diseases to all deaths from "Other Causes."

In Table VI the births and deaths (under five heads) recorded during the periods 1911-15 and 1916-20 are compared, together with the fatality rates per 1,000 births, and in Table VII a similar comparison is instituted between the mortality rates per 10,000 married women in three age-groups. In these tables an attempt has been made to show the effects of the war conditions, it being assumed that the marital relations of men called up for service were normal in the former period and had not been completely restored to the normal in the latter. Moreover, civil medical practice, conducted under difficulties during the autumn of 1914 and the early months of 1915, was practically disorganized in the latter part of that year by the claims of the Services. It will be noted that in the second period the fatality (all causes) per 1,000 births increased 12 per cent., while the mortality per 10,000 married women (all ages) decreased nearly 9 per cent. Further inquiry into this part of the subject is needed.

1911-22.

In 1911 tabulation by "Administrative Districts" was substituted for that by "Registration Districts," &c. "London" from that date means the "County of London" and tables for the cities and boroughs within the County have been included in each Annual Report. In Table VIII are set out the fatality rates per 1,000 births for the years 1911-20 from "Sepsis" (puerperal fever) and "Other Causes," in the whole County and in the five geographical subdivisions thereof. In Table IX the rates are shown for the two periods 1911-15 and 1916-20 and for the years 1921 and 1922. It will be seen that the rates for 1921 and 1922 are generally lower than the mean rates for 1916-20, and in some instances below the means for 1911-15.

It will be found that the rates in the "West" areas are generally higher than the rates for the County, and those in the "East," lower. During 1911-15 the mean rate for the County was 2·99, that for "West" 3·44, and that for "East," 2·70. During 1916-20 the rates were: County, 3·35; "West," 3·75; "East," 2·84. These differences were, to me, unexpected, and at present no explanation can be offered.

The contrast is even greater if made between individual cities and boroughs. Paddington and Kensington are usually regarded as "healthy" districts, an adjective which is not usually applied to Bethnal Green or Poplar. Comparing Paddington with Bethnal Green, it will be found that the total rate in the former district during 1911-15 was 30 per cent. higher, and that during 1916-20, 35 per cent. Similarly the rate recorded in Kensington during the former period was 17 per cent. above the rate in Poplar, and during 1916-20, 10 per cent. There is evidently scope for an intensive inquiry to elucidate those differences.

In examining Table IX one detail requires to be kept in mind. In certain boroughs—e.g., Chelsea, Hampstead—the fallacies attaching to smallness of the samples cannot be ignored.

Deaths in Institutions.

In the Reports for seven of the years included in the decennium 1911-20 tables of deaths in institutions distributed by sex and causes have been included. From those tables Table X has been compiled.

That there has been a great increase in the practice of treating in institutions complications arising during pregnancy and parturition is clearly shown by the figures given in Table X. During 1911 and 1912, 55·2 per cent. of the deaths of pregnant and parturient women were recorded as occurring in institutions, the proportion rising to 63·1 during 1914 and 1915, and to 70·5 during the years 1918-20. There is, unfortunately, no means of distinguishing between the deaths of women admitted to institutions for labour and those admitted after labour for treatment of complications which set in before admission to such institutions. Data are, I think, wanted to show the proportion of children born in institutions, the more so when one considers the changing housing conditions. In this connexion mention may be made of the records which have been kept in Paddington since 1905.

In 1905 the births in institutions formed 7·4 per cent. of the total number including 3·1 per cent. in the Poor Law lying-in wards. In 1911 the total percentage was 8·7 and that in the Poor Law lying-in wards, 1·8. Those percentages were based mainly on the local returns, as it was not until 1912 that complete returns of births belonging to Paddington which took place in outlying districts, were received. In that year the proportion rose to 11·5 per cent., with 2·7 per cent. in Poor Law institutions. In 1923 the total percentage was 28·8, 17·3 per cent. taking place in maternity hospitals and homes, 4·1 in general hospitals, and 6·3 in Poor Law institutions. Comparing the averages for 1914-18 with those for 1919-23, the proportion of births in all institutions shows an increase of 48 per cent. and that of births in Poor Law institutions, 59.

If the experience here recorded be typical of the country generally or of London in particular, and if the risks attaching to labour in institutions are really smaller than those attaching to labour in the women's homes, it is remarkable that the (apparent) increase in the proportion of births taking place in institutions has not been accompanied by a notable decrease in the fatality of childbearing.

TABLE I.—LONDON.

Quinquennium	Per 1,000 Burials Females	Per 1,000 Christenings	Quinquennium	Per 1,000 Burials Females	Per 1,000 Christenings	Quinquennium	Per 1,000 Burials Females	Per 1,000 Christenings	Quinquennium	Per 1,000 Burials Females	Per 1,000 Christenings
—	—	—	1700-04	22·78	15·13	1800-04	20·60	10·12	1900-04	11·79	3·35
—	—	—	1705-09	23·34	15·82	1805-09	20·72	9·19	1905-09	10·77	2·96
—	—	—	1710-14	19·22	14·05	1810-14	21·29	9·29	1910-14	10·71	2·99
—	—	—	1715-19	20·25	14·49	1815-19	23·73	9·76	1915-19	8·59	3·22
—	—	—	1720-24	21·16	14·98	1820-24	20·30	7·75	—	—	—
—	—	—	1725-29	16·81	13·25	1825-29	21·53	8·78	—	—	—
—	—	—	1730-34	19·76	14·78	—	—	—	—	—	—
—	—	—	1735-39	18·18	14·55	—	—	—	—	—	—
—	—	—	1740-44	14·95	14·28	1840-44	14·72	5·72	—	—	—
—	—	—	1745-49	15·44	13·55	1845-49	17·31	6·98	—	—	—
—	—	—	1750-54	17·31	12·51	1850-54	15·19	5·34	—	—	—
—	—	—	1755-59	18·77	13·07	1855-59	14·28	4·93	—	—	—
1660-64	25·82	21·91	1760-64	22·10	16·46	1860-64	13·98	4·79	—	—	—
1665-69	24·11	31·33	1765-69	17·26	12·21	1865-69	13·00	4·35	—	—	—
1670-74	24·96	20·65	1770-74	18·31	12·00	1870-74	16·31	5·20	—	—	—
1675-79	28·65	21·88	1775-79	18·02	11·30	1875-79	12·00	3·69	—	—	—
1680-84	31·29	24·25	1780-84	17·28	9·59	1880-84	13·74	4·09	—	—	—
1685-89	28·89	21·42	1785-89	19·16	10·29	1885-89	12·62	3·72	—	—	—
1690-94	27·20	19·06	1790-94	18·35	9·31	1890-94	13·98	4·44	—	—	—
1695-99	25·00	15·64	1795-99	17·56	8·81	1895-99	11·39	3·44	—	—	—

TABLE II.—LONDON: FATALITY PER 1,000 BIRTHS.

	Total	Sepsis (Puerperal Fever). All Ages	15-	20-	25-	35-	45-	Other Causes. All Ages	15-	20-	25-	35-	45-
1851-	5·26	1·50	0·08	0·31	0·78	0·32	—	3·75	0·06	0·57	1·67	1·38	0·06
1854-	4·81	1·94	0·12	0·48	0·92	0·44	0·01	2·86	0·06	0·41	1·31	1·00	0·05
1859-	4·63	1·93	0·09	0·38	1·03	0·41	0·01	2·69	0·09	0·39	1·23	0·94	0·04
1864-	4·68	1·94	0·09	0·45	0·93	0·44	0·00	2·73	0·11	0·42	1·21	0·96	0·01
1869-	4·70	2·25	0·13	0·52	1·10	0·48	0·01	2·44	0·07	0·34	1·12	0·86	0·02
1874-	4·43	2·30	0·11	0·55	1·10	0·51	0·01	2·12	0·07	0·30	0·94	0·76	0·03
1879-	3·84	2·32	0·09	0·57	1·15	0.48	0·01	1·52	0·06	0·22	0·68	0·52	0·03
1884-	3·89	2·41	0·09	0·57	1·14	0·57	0·02	1·48	0·05	0·18	0·65	0·55	0·02
1889-	4·30	2·19	0·06	0·45	1·13	0·53	0·07	2·11	0·06	0·33	0·98	0·70	0·03
1894-	3·53	1·76	0·05	0·38	0·92	0·39	0·01	1·77	0·05	0·24	0·83	0·61	0·03
1899-	3·28	1·59	0·03	0·32	0·82	0·39	0·00	1·68	0·03	0·25	0·78	0·59	0·02
1904-	2·94	1·42	0·03	0·27	0·75	0·34	0·01	1·52	0·03	0·21	0·71	0·53	0·01
1909-	2·91	1·42	0·02	0·25	0·73	0·39	0·00	1·46	0·06	0·17	0·67	0·53	0·03
1914	3·20	1·54	0·04	0·27	0·76	0·45	0·01	1·66	0·06	0·23	0·71	0·61	0·03
1919	3·73	1·89	0·06	0·24	0·95	0·60	0·02	1·84	0·07	0·31	0·81	0·61	0·02
1920	3·39	1·80	0·04	0·24	1·04	0·46	0·00	1·59	0·04	0·23	0·73	0·57	0·01

TABLE III.—LONDON: MORTALITY PER 1,000 MARRIED WOMEN.

	Birth Rate	Total	Sepsis (Puerperal Fever)						Other Causes					
			All Ages	15-	20-	25-	35-	45-	All Ages	15-	20-	25-	35-	45-
1851-	239	1·25	0·35	2·02	0·63	0·44	0·22	—	0·90	1·44	1·16	0·96	0·97	0·13
1854-	235	1·13	0·45	2·68	0·86	0·53	0·30	0·02	0·67	1·46	0·81	0·75	0·69	0·11
1859-	238	1·10	0·45	1·89	0·76	0·60	0·28	0·02	0·64	1·89	0·78	0·71	0·65	0·08
1864-	250	1·17	0·48	1·96	0·94	0·57	0·33	0·01	0·68	2·55	0·86	0·74	0·71	0·03
1869-	235	1·16	0·56	2·76	1·03	0·66	0·36	0·03	0·60	1·65	0·69	0·68	0·64	0·05
1874-	254	1·12	0·58	2·70	1·12	0·67	0·39	0·03	0·54	1·62	0·61	0·57	0·71	0·08
1879-	251	0·96	0·58	2·11	1·15	0·70	0·36	0·03	0·38	1·40	0·45	0·41	0·39	0·06
1884-	242	0·94	0·58	2·52	1·15	0·66	0·41	0·04	0·35	1·55	0·37	0·38	0·39	0·06
1889-	230	0·99	0·50	1·93	0·88	0·63	0·35	0·01	0·48	1·72	0·64	0·54	0·47	0·05
1894-	187	0·77	0·38	1·64	0·74	0·49	0·24	0·02	0·39	1·64	0·46	0·44	0·39	0 05
1899-	207	0·68	0·33	1·29	0·62	0·41	0·23	0·01	0·34	1·29	0·47	0·39	0·35	0·05
1904-	198	0·58	0·28	1·33	0·57	0·36	0·18	0·02	0·30	1·66	0·44	0·34	0·29	0·02
1909-	180	0·52	0·25	1·39	0·55	0·33	0·19	0·01	0·26	3·26	0·38	0·30	0·26	0·04
1914-	144	0·46	0·22	1·39	0·46	0·28	0·17	0·01	0·23	2·08	0·40	0·26	0·23	0·03
1919	126	0·47	0·23	1·38	0·35	0·32	0·20	0·01	0·23	1·66	0·46	0·27	0·20	0·01
1920	184	0·62	0·33	1·38	0·51	0·52	0·22	0·00	0·29	1·38	0·49	0·36	0·27	0·01

TABLE IV.—LONDON.

	Totals recorded						Fatality per 1,000 births				
	Births	Puerperal mania	Puerperal convulsions	Placenta prævia, flooding	Phlegmasia a.d.	Other causes (Abortion)	Puerperal mania	Puerperal convulsions	Placenta prævia, flooding	Phlegmasia a.d.	Other causes (Abortion)
1881-	400,716	58	91	281	26	191 (63)	0·14	0·22	0·70	0·06	0·47 (0·15)
1884-	668,062	71	130	474	48	264 (69)	0·10	0·19	0·70	0·07	0·39 (0·10)
1889-	660,268	61	127	453	32	724 (111)	0·09	0·19	0·68	0·04	1·08 (0·16)
1894-	666,478	38	102	394	18	629 (84)	0·05	0·15	0·59	0 02	0·94 (0·12)
1899-	658,347	50	139	274	23	623 (99)	0·07	0·21	0·41	0 03	0·94 (0·15)
1904-	627,534	36	138	277	37	466 (60)	0·05	0·21	0·44	0·05	0·74 (0·09)
1909-	568,312	16	193	219	83	335	0·02	0·33	0·38	0·14	0·58
1914-18	462,931	9	205	166	85	304	0·01	0·44	0·35	0·18	0·65
1919	82,525	1	52	25	16	58	0·01	0·63	0·30	0·19	0·70
1920	120,529	4	57	42	27	62	0·03	0·47	0·34	0·22	0·51

TABLE V.—LONDON.

	Mean Annual Numbers					Index Numbers. (Other causes (total) = 100)				
	Puerperal mania	Puerperal convulsions	Placenta prævia, flooding	Phlegmasia a.d.	Other causes (Abortion)	Puerperal mania	Puerperal convulsions	Placenta prævia, flooding	Phlegmasia a.d.	Other causes (Abortion)
1881-	19	30	94	9	64 (21)	9	14	43	4	30 (10)
1884-	14	26	95	10	53 (14)	7	13	48	5	27 (7)
1889-	12	25	91	6	145 (22)	4	9	33	2	52 (8)
1894-	8	20	79	3	126 (17)	3	9	34	1	53 (7)
1899-	10	28	55	4	125 (20)	4	13	25	2	56 (9)
1904-	7	28	56	7	93 (12)	4	15	29	4	48
1909-	3	39	44	16	67	2	23	26	9	40
1914-18	2	41	33	17	61	1	27	21	11	40
1919	1	52	25	16	58	1	34	16	11	38
1920	4	57	42	27	62	2	30	22	14	32

TABLE VI.—LONDON.

	1911-15		1916-20		Index numbers
	Totals	Fatality	Totals	Fatality	
Births	545,922		453,916		
134. Accidents of pregnancy	193	0·353	125	0·275	79
135. Puerperal hæmorrhage	174	0·318	163	0·359	113
136. Other accidents of childbirth	141	0·258	152	0·334	129
137. Puerperal fever	787	1·441	753	1·658	115
138. Puerperal albuminuria, convulsions ...	217	0·397	237	0·522	131
139. Puerperal phlegmasia a.d., thrombosis	113	0·206	85	0·187	91
140. Puerperal insanity	14	0·025	8	0·017	68
141. Puerperal diseases of the breast ...	2	0·003	6	0·013	433
Totals	1,641	3·005	1,529	3·368	112

Fatality per 1,000 births. Index numbers—rates 1911-15 = 100.

TABLE VII.—LONDON.

	Deaths recorded								Mortality: Mean annual							
	1911-15				1916-20				1911-15				1916-20			
	15-	25-	45-	Totals	15-	25-	45-	Totals	15-	25-	45-	Totals	15-	25-	45-	Totals
134. Accidents of pregnancy	18	170	5	193	14	105	6	125	0·64	0·68	0·10	0·60	0·46	0·42	0·10	0·38
135. Puerperal hæmorrhage	12	159	3	174	14	146	3	163	0·42	0·64	0·06	0·54	0·46	0·58	0·04	0·50
136. Other accidents of childbirth	17	119	5	141	32	119	1	152	0·60	0·48	0·10	0·44	1·08	0·48	0·00	0·46
137. Puerperal fever	159	627	1	787	138	610	5	753	5·72	2·56	0·02	2·46	4·72	2·48	0·08	2·32
138. Puerperal albuminuria, convulsions ...	71	143	3	217	86	151	—	237	2·56	0·58	0·06	0·68	2·94	0·60	—	0·72
139. Puerperal phlegmasia a.d., thrombosis	13	98	2	113	8	76	1	85	0·46	0·40	0·04	0·34	0·26	0·30	0·00	0·26
140. Puerperal insanity	3	11	—	14	—	8	—	8	0·10	0·04	—	0·04	—	0·02	—	0·02
141. Puerperal diseases of the breast ...	—	2	—	2	2	4	—	6	—	0·00	—	0·00	0·06	0·00	—	0·00
Totals ...	293	1,329	19	1,641	294	1,219	16	1,529	10·55	5·46	0·40	5·16	10·06	4·98	0·30	4·71
	17·8	80·9	1·1	100	19·2	79·7	1·0	100	—	—	—	—	—	—	—	—

Mortality per 10,000 married women.

TABLE VIII.—FATALITY PER 1,000 BIRTHS.

	Sepsis											
	1911	1912	1913	1914	1915	1916	1917	1918	1919	1920	1921	1922
LONDON	1·38	1·50	1·23	1·77	1·29	1·59	1·56	1·39	1·87	1·78	1·42	1·36
West ...	1·53	1·97	1·35	2·26	1·28	2·12	1·61	1·31	2·36	2·02	1·53	1·95
North ...	1·44	1·43	1·39	1·84	1·31	1·73	1·61	0·88	2·26	1·67	1·61	1·10
Central	1·29	0·80	2·61	3·14	0·98	1·08	3·54	—	2·20	2·27	0·72	1·45
East ...	1·31	1·60	1·11	1·80	1·15	1·15	1·29	1·16	1·18	1·39	1·19	1·22
South ...	1·34	1·37	1·06	1·43	1·36	1·55	1·50	1·84	1·76	1·90	1·41	1·32
	Other Causes											
	1911	1912	1913	1914	1915	1916	1917	1918	1919	1920	1921	1922
LONDON	1·36	1·62	1·59	1·73	1·49	1·77	1·62	1·72	1·85	1·60	1·56	1·47
West ...	1·82	1·86	1·35	2·38	1·41	1·66	1·85	1·39	2·52	1·92	2·62	2·02
North ...	1·14	1·68	1·78	1·54	1·26	1·79	1·90	1·96	1·54	1·90	1·15	1·58
Central...	1·55	1·34	1·16	1·14	3·29	2·52	0·89	1·56	1·32	1·42	1·44	0·72
East ...	1·36	1·22	1·50	1·17	1·26	1·81	1·50	1·83	1·46	1·45	1·31	1·28
South ...	1·30	1·70	1·65	1·89	1·61	1·73	1·50	1·68	1·93	1·42	1·49	1·34

TABLE IX.—MEAN RATES PER 1,000 BIRTHS.

	Total				Sepsis				Other causes			
	1911-15	1916-20	1921	1922	1911-15	1916-20	1921	1922	1911-15	1916-20	1921	1922
LONDON—	2·99	3·35	2·98	2·83	1·43	1·64	1·42	1·36	1·56	1·71	1·56	1·47
West	3·44	3·75	4·15	3·97	1·68	1·88	1·53	1·95	1·76	1·87	2 62	2·02
Paddington ...	3·07	3·75	4·50	2·19	1·34	2·05	2·07	0·73	1·73	1·70	2·42	1·46
Kensington ...	3·27	3·50	4·09	4·75	1·71	1·53	1·46	2·53	1·65	1·98	2·63	2·22
Hammersmith	3·91	3·55	3·95	3·26	1·18	2·12	1·43	1·45	2·73	1·63	2·51	1·81
Fulham ...	3·08	3·89	3·96	4·00	1·59	2·18	1·70	2·77	1·49	1·71	2·26	1·23
Chelsea ...	3·89	4·85	3·64	9·34	2·27	2·18	0·91	1·86	1·62	2·67	2·73	7·47
Westminster...	4·81	3·32	4·68	3·18	2·61	1·27	1·04	2·12	2·20	2·05	3·64	1·06
North	2·96	3·45	2·76	2·69	1·48	1·63	1·61	1·10	1·48	1·82	1·15	1·58
St. Marylebone	2·76	4·56	5·11	—	1·37	1·89	1·70	—	1·39	2·67	3·40	—
Hampstead ...	4·33	3·25	2·98	4·65	2·63	1·29	2·23	2·32	1·70	1·96	0·74	2·32
St. Pancras ...	3·11	3·78	2·10	3·07	1·61	1·71	1·47	0·87	1·50	2·07	0·63	2·19
Islington ...	2·57	2·97	2·17	2·77	1·21	1·36	1·53	0·79	1·35	1·61	0·64	1·98
StokeNewington	3·73	4·91	0·93	6·40	1·46	2·08	—	6·40	2·28	2·83	0·93	—
Hackney ...	2·99	3·26	3·81	1·89	1·52	1·87	2·00	0 84	1·27	1·39	1·80	1·05
Central	3·46	3·36	2·16	2·18	1·76	1·82	0·72	1·45	1·70	1·54	1·44	0·72
Holborn ...	3·09	1·40	3·08	1·50	2·06	1·09	1·54	1·50	1·03	0·31	1·54	—
Finsbury ...	3·37	4·20	1·50	2·58	1·74	2·12	0·50	1·55	1·63	2·08	1·00	1·03
City	6·35	1·60	7·57	—	0·88	1·60	—	—	5·46	—	7·57	—
East	2·70	2·84	2·50	2·50	1·39	1·23	1·19	1·22	1·30	1·61	1·31	1·28
Shoreditch ...	2·79	3·39	2·28	2·36	1·19	1·78	1·30	1·01	1·60	1·63	0·98	1·35
Bethnal Green	2·35	2·77	2·33	2·34	1·22	1·22	0·66	1·34	1·13	1·55	1·66	1·00
Stepney ...	2·72	2·39	2·09	2·66	1·43	0·95	1·29	1·49	1·28	1·44	0·80	1·16
Poplar ...	2·81	3·18	3·34	2·48	1·58	1·25	1·33	0·90	1·23	1·93	2·00	1·58
South	2·94	3·36	2·90	2·66	1·31	1·71	1·41	1·32	1·63	1·65	1·49	1·34
Southwark ...	2·47	3·21	2·77	2·84	1·08	1·71	1·38	1·31	1·39	1·29	1·38	1·53
Bermondsey ...	2·18	3·25	2·70	1·53	0·88	1·10	0·90	0·61	1·30	2·15	1·80	0·92
Lambeth ...	3·22	3·33	3·56	2·82	1·74	1·72	2·67	1·88	1·48	1·61	0·89	0·94
Battersea ...	2·84	3·31	3·74	2·45	1·19	1·34	1·06	0·81	1·65	1·85	2·67	1·63
Wandsworth...	3·62	3·25	2·09	3·26	1·49	2·27	0·64	1·27	2·13	0·98	1·45	1·99
Camberwell ...	2·70	3·17	2·86	2·58	1·17	1·61	1·75	1·89	1·53	1·56	1·11	0·68
Deptford ...	2·85	3·03	2·99	3·04	1·12	2·30	1·49	1·14	1·73	0·73	1·49	1·90
Greenwich ...	2·74	3·15	3·43	0·93	1·51	1·03	1·28	0·46	1·23	2·02	2·14	0·46
Lewisham ...	3·18	3·43	2·02	4·05	1·41	1·32	0·57	1·55	1·77	2·11	1·44	2·49
Woolwich ...	2·61	4·73	3·12	2·04	1·06	2·31	1·56	1·02	1·55	2·41	1·56	1·02

TABLE X.—LONDON: DEATHS IN INSTITUTIONS.

	1911-12			1914-15			1918-20		
	Deaths			Deaths			Deaths		
	Total	In institutions	Percentage	Total	In institutions	Percentage	Total	In institutions	Percentage
134. Accidents of pregnancy	71	30	42·2	86	42	48·8	68	42	61·7
135. Puerperal hæmorrhage	72	13	18·0	70	27	38·5	89	50	56·1
136. Other accidents of childbirth	48	17	35·4	61	26	42·6	97	55	56·7
137. Puerperal fever ...	323	231	71·5	327	260	79·5	469	383	81·6
138. Puerperal albuminuria, convulsions	80	46	57·5	77	47	61·0	150	106	70·6
139. Puerperal phlegmasia a.d., embolism, &c.	56	8	14·2	43	15	34·8	58	18	32·1
140. Puerperal insanity	7	5	71·4	6	6	100	6	6	100
141. Puerperal diseases of the breast	—	—	—	1	1	100	3	2	66
Totals	657	350	53·2	671	424	63·1	938	662	70·5
In Poor Law Institutions		145	41·4		186	43·8		308	46·5
In Hospitals, Homes ...		205	58·6		236	55·7		354	53·5

Note upon the Provision for Lying-in Women in London up to the middle of the Eighteenth Century.

By G. C. PEACHEY.

I HAVE seen it mentioned by some historian, but the reference has escaped me, that the Statute of Labourers contained a special proviso in favour of pregnant women. And although my search into the wording of the Act itself has not rewarded me, it is likely enough that the Royal Ordinance of 1349, upon which the Statute of 1350 was based, may have contained some such beneficent clause. For if we remember that England had just been stricken with the Black Death, which had reduced the population by fully one half (Gasquet), measures directed towards the preservation of infant life might almost be expected. Failing any positive evidence in this particular, the first information I have come across is derived from an Act of the second year of Henry V, which orders that whereas hospitals founded by noble Kings of this realm and Lords and Ladies both spiritual and temporal, who gave great part of their goods, lands and tenements to sustain impotent men and women, lazars, men out of their wits, and poor women with child, and to nourish, relieve, and refresh other poor people in the same, be now withdrawn and spent in other uses whereby many men and women have died in great misery: it is ordained that the Ordinary shall inquire, correct, and reform such

abuses according to the laws of Holy Church. Again, evidence exists that in 1437, St. Bartholomew's, then a religious house, was granted certain privileges in consideration of the great charges incurred by it in receiving poor, feeble and infirm people, and lying-in women and their infants, and keeping them until their purification.

But after the Reformation and the dissolution of the monasteries and hospitals, all provision for women in labour came to an end, excepting the very small number of such cases admitted into the two Royal Hospitals of St. Bartholomew and St. Thomas.

The closure of the religious houses had produced an increase of vagrancy, poverty and destitution which necessitated the various enactments comprised in the Elizabethan poor law—until quite recent times the basis of our own. But all that was compulsory in it was the maintenance of the poor. Working-houses for the able-bodied, and dwellings for the aged and infirm were authorized, but were not ordered. Few indeed were the parishes in which the able-bodied were well employed: in fewer still were the sick and impotent well attended. Of provision for lying-in women there was none. And even though the erection of working-houses in each parish was authorized, it was illegal for two parishes to combine together and share the expense of establishing a common workhouse. That power was withheld until 1723, when an Act of George I legalized the combination of two or more parishes to form a Union, giving an impetus to the workhouse movement, and eventuating in the provision of workhouse infirmaries. The accommodation afforded by these only consisted at first of one or more wards in the workhouse which were set aside for the sick, and in some cases for lying-in women, until in time the infirmary became a separate building.

The first evidence we have of any specific provision for lying-in women occurs in the proposal of the Westminster Charitable Society, which in 1716 launched a scheme for the relief of the sick and needy, and for the lodging, maintenance, and treatment of poor lying-in women in the parish of St. Margaret. As the parent of our voluntary hospitals this society deserves record and recognition. Three years later the Westminster Infirmary was established, and in 1734 St. George's opened its doors: but in each case lying-in women were specifically excluded. The accommodation was small and inadequate; the want of sanitation horrible to contemplate. But eighteen months after its foundation, a suggestion was made and considered by the governors of St. George's for the setting aside of one ward for lying-in women, to receive twenty-five cases, which were to be attended by a midwife and four nurses. The project, however, did not mature, and from my knowledge of the then condition of the hospital, I think it was for the best.

The establishment of other voluntary hospitals followed in quick succession at Winchester, Bristol, York, Exeter, Bath and Northampton in the provinces, and in the Metropolis the London Hospital at Mile End in 1740, and the Middlesex in 1745. But in none of these was any provision made for maternity caess, until in July, 1747, special wards on a separate floor were set aside at the Middlesex for married lying-in women, an example which was followed in 1749 by the establishment of the Lying-in Hospital in Brownlow Street, Holborn, and in 1750 by the City of London Lying-in Hospital in Aldersgate Street. But these likewise were for married women only.

It has been supposed that together with the celebrated Dublin Charity—the Rotunda—which was opened in 1745 by Dr. Benjamin Mosse at his own expense, these were the first lying-in hospitals in the British Isles. But this is incorrect.

Among the Sloane MSS. in the British Museum is a letter[1] from Sir Richard Manningham, which shows that in the summer of 1739 he rented and opened a house, or part of a house, in Jermyn Street, next to his own residence, for the reception and treatment of twenty-five lying-in women. It was supported by voluntary subscriptions, and has claim to further recognition. Founded by Manningham, who was the foremost obstetrician of his time, it appears to have been carried on under his auspices until about 1752, when Dr. Felix Macdonough succeeded him in its management. In that year Macdonough advertised that he would give a course of lectures on midwifery at his house in Bury Street, St. James's, and at the Lying-in Infirmary in Jermyn Street. In 1753 Macdonough's advertisement was repeated, but the lectures were given at *his* lying-in hospital in Duke Street, Grosvenor Square. In 1754 he was still living in Bury Street, but the lectures were given at the General Lying-in Hospital for unmarried and married women in Duke Street, Grosvenor Square; and Maitland in his "History of London," published in 1756, confirms its identity by saying that it was formerly in Jermyn Street. Advertisements of the charity show that it remained in Duke Street till 1762, but I have evidence that shortly afterwards it was moved to Quebec Street in the parish of St. Marylebone, being still known as the General Lying-in Hospital. There it remained till about 1773, when it was again moved to St. George's Row, near Oxford Street turnpike—the Marble Arch—and at that date Dr. Macdonough was still its medical officer. In 1791 it was moved to Bayswater Hall, near Queen's Road: and in 1809 it was still at that address, and its financial position was very bad. But it managed to secure the support of the Duke of Sussex, by whose influence Queen Charlotte became its patron and lent her name to the institution, and this same year it was moved to the site which it still occupies. The history of Queen Charlotte's Hospital was written by its quondam secretary, Mr. Ryan, who begins with the statement that it was founded in 1752, and that its history, and even its site, during the first thirty years of its existence is uncertain. There is, however, no longer any doubt of its identity with the institution founded by Sir Richard Manningham in 1739, and as such it can claim to be the earliest lying-in hospital not only in London, but in the British Isles. I may perhaps add that these particulars are published here for the first time.

I will conclude these somewhat disconnected remarks with a few words on the early teaching of midwifery in London.

The first advertisement of such lectures which I have come across is that of John Maubray.

"7 November, 1724.—To be opened on the 17th instant, at 11 a.m., by John Maubray, M.D., at his house in New Bond Street, near Hanover Square. A course of midwifery

[1] Letter from Sir Richard Manningham.

(Sloane MS. 4056, pp. 84-51).

Sufficient being subscribed to make a beginning, and several having promised, so soon as the undertaking is set on foot, to send their benefactions, I have, therefore, taken an apartment at 32 pounds p. a. rent, which will accommodate 300 women in a year, and am making preparations for the reception of some by midsummer day next; the bearer, therefore, waits on you with a receipt from our treasurer, Thomas Hardy, Esq., for the four guineas subscription you were pleased to honour our charitable undertaking with,

I am, Sir, with the utmost esteem,
Your most obliged and most obedient servant,
R. Manningham.

17 May, 1739,
Jermyn Street, St. James's.

comprehending many new and undeniable rules for the safe and expeditious performance of deliveries in all cases of births, according to the latest improvements of this most ingenious art, wherein the doctrine of manual operation will be clearly explained, its certainty and facility manifestly made out, as also the vile practice and abominable use of instruments will be openly exposed and confuted. But because the theoretical part is not altogether sufficient for the full instruction of such as design to apply themselves this way, the doctor proposes also to find them that enter as pupils proper subjects and sufficient opportunities for practical experience conformable to the laudable custom of France and other foreign parts. These useful lectures of natural philosophy will be continued twice a week all the winter season: Author also of the book lately published entitled 'The Female Physician,' which is printed and sold by James Holland, at the Bible and Ball, in St. Paul's Churchyard."

Maubray continued his lectures till 1731, and possibly later. He does not appear to have undertaken the instruction of midwives.

The next teacher of whom I have definite knowledge was Edmund Chapman, who advertised, in 1736, that he would instruct young gentlemen in the art of midwifery, in Orange Street, Red Lion Square. Chapman was the author of a work on midwifery.

Then comes Sir Richard Manningham, whose first advertisement appeared September 20, 1739:—

"From the Lying-in Apartment in the great Auction-house in Jermyn Street, St. James's. As hitherto midwives and other practitioners of midwifery here in England have very often been at a great loss for want of proper opportunities of instruction in the art and practice of midwifery, therefore, at the said house Lectures will be read and young physicians, surgeons, and women, perfectly taught the art and practice of midwifery, and the performance of deliveries of all kinds even the most difficult with the utmost decency and dexterity by means of a contrivance made on the bones or skeleton of a woman with an artificial matrix (of glass), whereby all the inconveniences which might otherwise happen to women from pupils practising too early on real objects will be entirely prevented: for by this method and contrivance each pupil will become in a great measure a proficient in his business before he attempts a real delivery. And the young physicians will be particularly instructed in the whole theory and practice of the said art and of all diseases incident to women during pregnancy and child-bed, and of those incident to children; by Sir Richard Manningham, Kt., M.D., and man-midwife, &c. Note that men and women are instructed on different days."

The number of lecturers on midwifery during the first half of the eighteenth century was remarkably small, but among them was William Smellie who, in June, 1742, advertised that he would

"begin a course of lectures on the theory and practice of midwifery, wherein all the branches of that art would be fully explained, and the whole illustrated with proper machines so contrived as to represent real women and children: at his house in the New Court, formerly the Key and Garter Tavern, over against St. Alban's Street, in Pall Mall. N.B.—He has houses where poor women with child are delivered, at which deliveries those who are his pupils may, on reasonable terms, be present."

And as I have pointed out in a forthcoming work which deals with the teaching of anatomy during the first half of the eighteenth century, Frank Nicholls, as early as 1732, introduced midwifery as one of the subjects of his complete course of human and comparative anatomy. During the latter half of the century the number of teachers of midwifery was greatly increased and included such men as Kelly, Orme, Mackenzie, Leake, and Denman.

DISCUSSION.

Sir D'ARCY POWER congratulated Dr. Peachey upon his success in unravelling the early history of Queen Charlotte's Hospital. In commenting upon Dr. Dudfield's interesting contribution he drew attention to the Broadsheets catalogued in the Library of the British Museum with the press-mark 669f. Amongst them was "A true Report of the Great Costs and Charges of the Five Hospitals in the City of London," &c. This report was issued yearly, the five hospitals being Christ's Hospital, St. Bartholomew's Hospital, St. Thomas's Hospital, Bridewell and Bethlehem. The Reports for 1645, 1648, 1649, 1650, 1653, 1655 and 1656 show that the mortality approximated 10 per cent. per annum, except in 1648, when for some reason it was 20 per cent. at St. Bartholomew's, whilst the average remained unchanged at St. Thomas's.

Dr. L. G. FISHER (Brooklyn, New York) said that in addition to more expert training of midwife and physician, the co-operation of the patient herself should be sought. This could best be accomplished by the establishment of the ante-natal clinic, where frequent clinical and laboratory examinations would enable any deviation from the normal progress of the pregnancy to be detected, and where the patient would be instructed as to her mode and manner of life before, during and after the puerperium.

Section of Epidemiology and State Medicine.

Chairman—Dr. M. GREENWOOD (Hon. Secretary).

The Elucidation of Cancer.

By LOUIS WESTENRA SAMBON, M.D., F.Z.S.

Lecturer in the London School of Tropical Medicine.

AN after-dinner story I heard some years ago told how a scientist, who had been praising the discovery of argon, when asked, "But what is argon?" replied, "Well, you see, as yet we haven't discovered much about it except its name."

To-day we are asked, "What is cancer?" Must we give the same reply? Only names—futile names!

I think not. A vast amount of valuable work has been achieved in field, hospital and research laboratory and, no doubt, at this very moment, cause, prevention and cure of cancer are within our grasp. All that is needed, it seems to me, is to piece together fittingly the thousand and one irregular fragments of a bewildering jig-saw puzzle. Certainly, to-day, opinion differs greatly with regard to the cause of cancer, but what of that? Contention is to progress what the whetstone is to the razor.

CANCER THEORIES.

What cause have we not brought forward to account for the production of cancer? Some have accused the light of the sun, others radium. Hutchinson incriminated the internal use of arsenic long continued. Hoffman believes it to be the outcome of civilization, a difficult word, which as luckily he explains, means "the faulty habits and abnormalities of life, induced by the complex conditions of modern environment, favoured by food abundance, nervous strain, and the many by-products of material prosperity." Siverston and Dahlstrom ascribed cancer to the increasing employment of machinery, and a biologist suggested the erect attitude of man! Holden Webb, of Melbourne, explained that it was brought about by the crystallization of cholesterin from the living cells, and proposed a method of treatment by hypodermic injections of soap solution. A great London surgeon, the late Sir Frederick Treves, put it down to canned foods, cold storage meats, and the use of preservatives, while Kelling attempted to prove that tumours are made up of the tissues of cows, pigs and chickens taken as food. A similar theory has been advanced with regard to vaccination by Drs. Moseley, Rowley and others, who believed that the person inoculated with cow-pox not only would suffer from "beastly" diseases, but also assume bovine features.

Many are the bacteria, yeasts, fungi and protozoa incriminated. Some bear high-sounding names such as "*Rhopalocephalus carcinomatosus*," "*Histosporidium carcinomatosum*," and "*Cancriameba macroglossa*." All have passed

away together with the less pretentious "strombodes" or "X-bodies," and other cell-inclusions now looked upon as degeneration products. But history repeats itself, and one cannot help recalling how the malaria parasites also were looked upon as degeneration products, even after Laveran's brilliant exposition of their livingness and specificness.

A Mountain of Cancer.

The transplantation of mouse-cancer from one mouse to another, first conceived by young Morau of Paris, led to an extrordinary development at the hands of Jensen of Copenhagen. The Danish investigator generously distributed his cancer material to all who wanted it, and soon numerous laboratories, in all parts of the civilized world, started growing the "Jensen strain" until a very mountain of mouse-cancer was raised in order that all cancer questions might be settled; but such a mountain, should it conceive, what other progeny could be expected but a mouse? *Parturiunt montes, nascetur ridiculus mus.*

Of course, it must be understood that Jensen's strain was only grafted but never induced primary cancer in the mouse used as stock. This grafting can go on indefinitely just like the grafting of desirable plant varieties while, just as pear-scion may be grafted upon quince-stock and plum upon peach, so also rat-sarcoma may be grafted successfully upon the brain of rabbit or mouse, and even upon the brain of the pigeon, as the Japanese Shirai has shown.

Van Helmont's Mice.

To-day the pathologist vaunts a greater triumph. He claims that he can produce primary cancer experimentally by merely brushing repeatedly, for some months, the ears of rabbits or the backs of mice with coal-tar or wood-tar derivatives. Think of it, carcinoma produced at will, at any time and in any quantity! Why, it is Van Helmont's trick reversed. Van Helmont claimed that he could produce mice out of soiled rags, a few grains of wheat or a morsel of cheese!

Under the tar-brush, we are told, the normal cells gradually undergo certain changes; then, after a time, benign growths arise, some of these gradually become malignant. "These growths," say Yamagiwa and Ichikawa, "have all the appearances of epitheliomata, but they tend to undergo retrogression and are healed by scar formation as well as by new growth of epithelium over the ulcer left by their necrosis and disintegration."

Reading between the lines of reports on the production of primary cancer by tarring, one is inclined to be sceptical. When the bacteriologist, the parasitologist and epidemiologist submitted their findings, the pathologist invariably objected that the growths they had been playing with probably were not cancer. May we not urge this objection against the pathologist now that he tells us he has produced cancer *de novo*? But there is another objection brought up against every new organism incriminated, namely, "the proportion of successful results claimed does not appear to exceed greatly that of spontaneous tumours in mice subjected to other traumatic influences." Does this argument not fit his case? Has he really created cancer with a few waves of the magic brush, or are not his growths, when truly cancerous, those very same spontaneous tumours which have baffled others?

Antiquity of Cancer.

Cancer was well known to the Ancients; its history can be traced backwards to the Classic period in Europe, to at least 4,000 years ago in Egypt. It is mentioned in the Papyrus Ebers, written about 1500 B.C., and in the

Edwin Smith Papyrus, the oldest known work on surgery (c. 1700 B.C.). Leonidas, of Alexandria, remarked on the importance of nipple retraction as a diagnostic sign of cancer of the breast, and even non-medical writers referred to the deadly nature of the disease. Ovid's "*Utque malum late solet immedicabile cancer serpens*" is an apt illustration. The ancient surgeons knew when and how to operate for cancer, using knife and cautery alternately in order to avoid hæmorrhage and prevent dissemination.

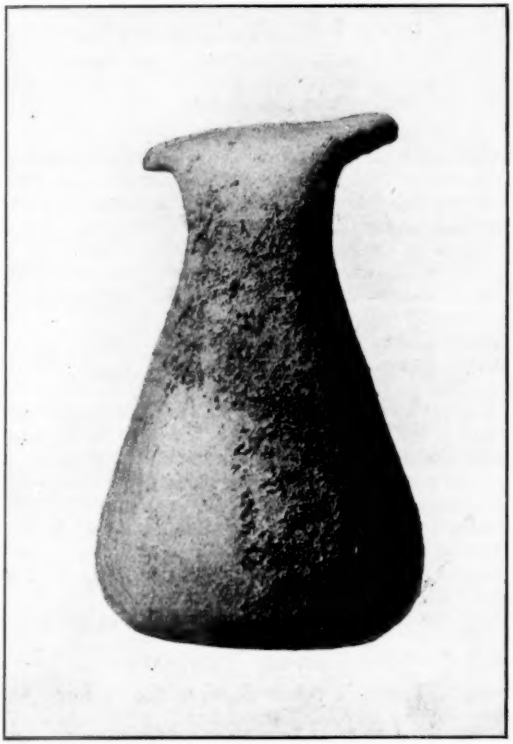


FIG. 1.—*Donarium* representing pedunculated fibroma of *labium majus* from Temple of Maternity, Capua. Second or third century B.C. (Photograph by Dr. Sambon.)

Among the many votive offerings (*Donaria*) of medical interest I collected in Italy, at the time of the Eleventh International Medical Congress, held in Rome in 1893, is a terra-cotta model of a pedunculated fibroma of the *labium majus* (fig. 1). It is the very image of a vulvar fibroma, removed from a West Indian negress, which I had the opportunity of seeing in 1913 at Port-of-Spain, Trinidad. This interesting donarium, found with many others representing breasts, pudenda, wombs, ovaries, septate uteri, placentæ, and small images of

women, some fashioned in the kneeling posture of parturition, others with enormously hypertrophied breasts, came from the ancient Temple of Maternity in Capua. Vulvar fibromas are benign tumours, but their tendency to undergo sarcomatous changes is well known. Finally, in the Civic Museum of Corneto Tarquinia, where I went to choose skulls still furnished with the admirable dentures of the ancient Etruscan dentist who fixed artificial teeth by means of gold bridge-work, I found a femur[1] showing evidence of periosteal sarcoma.

The name of crab (cancer—*καρκίνος*) given by the Ancients to a malignant tumour has been explained by the assumed resemblance between the neoplasm and the crustacean. Indeed, in the second century A.D., Galen says:—

"In the breasts we often find a tumour in size and shape closely resembling the animal known as a crab, for as in the latter the limbs protrude from either side, so in the tumour the swollen veins radiate from its edges and give a perfect picture of the crab."

The Ancients who knew the bird-attendant (*Pluvianus ægyptius*) of the crocodile, the crab commensal (*Pinnotheres*) of the pinna, the actinia-bearing hermit-crab and the sucking-fish (*Remora*) were, of course, acquainted with the striking tumour-like egg-sack of the Sacculina so common beneath the abdomen of the crabs they portrayed on their coins, gems and sculptures. Is it not natural to infer that the striking likeness between the crab parasite and a permeating malignant neoplasm should suggest the name of the tumour-bearing crustacean for the dread disease?

The Sacculina is a common entomostracan parasite of crabs. In its earliest stage it is an active, free-swimming larva, very like the Nauplii of Cirripeds, but it soon attaches itself to the body of a young crab, casts off every part of its economy save a small bundle of cells which, penetrating the body of the host, come to rest in the latter's intestine just beneath the stomach. Here, surrounded by a new cuticle, it shapes itself into the "sacculina interna," and like a germinating bean-seedling, proceeds to throw out branched suckers which, root-like, extend through every portion of the crab's anatomy (fig. 2). Growing in size, the parasite presses upon the underlying walls of the host's abdomen, causing them to atrophy, so that when the crab moults, a hole is left in this region corresponding in size to the body of the parasite. Through this opening the tumour-like body finally protrudes and becomes the mature "sacculina externa," free to deliver its active young into the open waters.

Dr. Ghetti's Proposition.

At the time of the Official Celebrations of the Pasteur Centenary held in France last May, whilst travelling from Paris to Strasbourg I had the good fortune to meet Dr. George Ghetti, of Faenza, who apprised me of the great prevalence of cancer in Romagna, Italy. "Might it not be possible," he asked, "for the better financed British investigators to avail themselves of the exceptionally favourable circumstances presented by the provinces of Ravenna and Forli for an intensive study of the cancer problem?" Dr. Ghetti contemplated a joint investigation by British and Italian physicians, the Italians supplying the material and, so far as possible, the use of their hospitals and laboratories, the British furnishing the necessary funds.

[1] The Etruscans used bridges made of gold rings holding human teeth, but I found a denture in which a single ox tooth filed lengthways replaced the missing central upper incisors. I had this copied by a Neapolitan dentist, Dr. Guerini, and the copy now is in the Wellcome Historical Medical Museum.

On my return to London I submitted Dr. Ghetti's proposal to the Committee of the British Empire Cancer Campaign and a few weeks later I was informed that, though holding its operations should not extend outside the limits of the British Empire, the Committee had decided to make me a small grant in order that I might verify the conditions reported by Dr. Ghetti.

I accepted and forthwith went to Italy with Mr. O. Benediktsson, who kindly volunteered to act as my assistant.

Statement of Views.

My training early inclined me to the parasitic theory of cancer. In a paper on "Principles Determining the Geographical Distribution of Disease," published twenty years ago in the *Journal of Tropical Medicine*, I upheld the infectious theory, urged the study of cancer in the wider field of

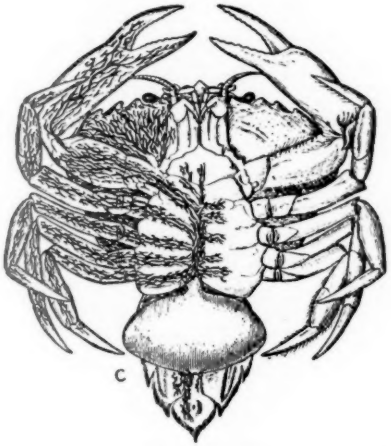


Fig. 2.—Tumour-like *Sacculina* protruding from ventral surface of crab's abdomen. Its root-like processes which permeate the body of the host are omitted from the left side. (After Delage.)

comparative pathology, and pointed out instructive analogies between the neoplasms of man and animals on the one side and the Cecidia or galls of plants and, more especially, the club-root diseases of Crucifers on the other (fig. 3). Analogy between animal and vegetable tumours already had been indicated by Roger Williams in 1888, Sir James Paget in 1887, and Woronin, the discoverer of *Plasmodiophora brassicæ*, in 1878. The Russian botanist had suggested that malignant growths in man might eventually prove to be due to a parasite similar to his plasmodium-forming fungus. Paget, in his well-known Morton Lecture, "On Cancer and Cancerous Diseases," setting aside the old doctrine that cancer is a "constitutional" disease, had said:—

"I have not used the word 'constitutional,' for it has become as indefinite and misleading in pathology as it seems to be in politics. . . . I believe that micro-parasites, or substances produced by them, will some day be found in essential relation with cancers and cancerous diseases. Mr. Ballance and Mr. Shattock have, indeed,

lately failed to find any; and if, in such a question as this, negative evidence could prove a negative, certainly theirs might make us hopeless. I would not be so, especially if workers so earnest and so skilful as they are will continue the search."

In 1905 Ford Robertson and Wade claimed to have found in carcinomatous tumours a parasitic micro-organism closely allied to the fungus of cabbage club-root disease and proposed to call it *Plasmodiophora carcinomatis* (figs. 4, 5). Their finding was not confirmed, but now, as twenty years ago, I believe the study of vegetable tumours is likely to afford valuable help, remembering that some of Hunter's best work upon the diseases of bones was illustrated by the similar changes he met with in diseased tree-branches and twigs.

In 1904, at Belfast, Ireland, on behalf of the Royal Institute of Public Health, I delivered an address on "The Part Played by Insects in the Transmission of Diseases." I stated that the rôle played by insects and other animals in the propagation of disease-germs is as essential, as wonderfully adapted and as widespread as the part played by insects and other animals in

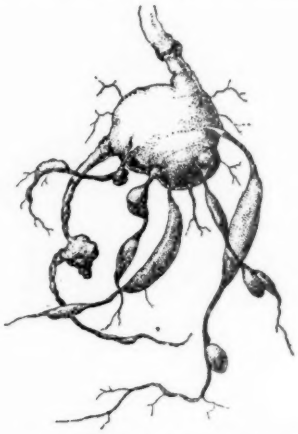


FIG. 3.—Root of young turnip affected by "club-root disease." One-half natural size.

the cross-fertilization of flowers and in the dispersal of seeds. I suggested that, were home diseases studied with the same care, interest and energy lately displayed in the elucidation of tropical diseases, we should find that with us also insects and other animals play as interesting, important and essential a part in the fosterage and dissemination of disease-germs. Speaking of cancer, I assumed a relationship between human cancer and the cancerous diseases of dogs, cats, mice, rats and other animals whose domestic habits bring them more or less in touch with man. At the same time I ventured even to suggest a possible connexion with certain household insects such as the cockroaches and the meal-beetles, because these were known to foster the larval stages of certain nematode worms (*Spiruroidea*) which, when adult, inhabit the alimentary tract and other organs of mammals, giving rise to swellings long looked upon as cancerous. I thought these and other helminths such as *Schistosoma*, *Opistorchis*, *Trichinella*,[1] &c., possibly might act as propagators

[1] Already in 1863 Klopsch had indicated a possible connexion between trichinella infection and the production of carcinoma.

of the unknown cancer virus. At the same time I also suggested that *Chrysops dimidiatus*, a day-biting *Tabinid* long incriminated by the natives of Old Calabar, Africa, probably was concerned in the dissemination of Loa infection, because in their sallies into the peripheral circulation the larvæ of

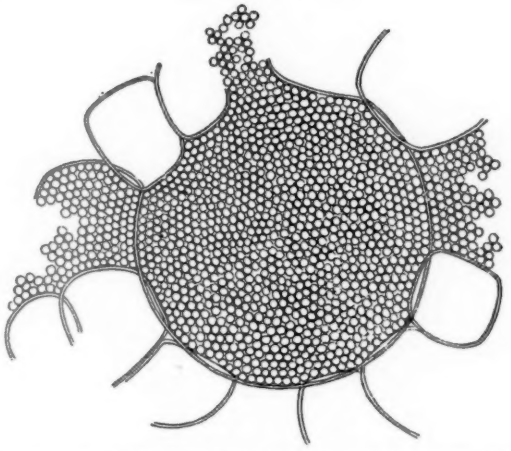


FIG. 4.—Spores of *Plasmodiophora brassicæ* Wor. as seen in cells of turnip root. Enlarged 400 diameters.

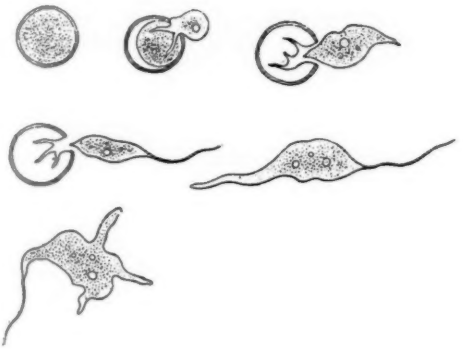


FIG. 5.—Germinating spores and swarm spores of *Plasmodiophora brassicæ*.

Loa loa exhibit a marked day periodicity contrasting strikingly with the night periodicity of the larvæ of *Filaria bancrofti* which are fostered by night-biting mosquitoes. This suggestion has proved true, like two others I made concerning the transmission of sleeping sickness[1] by the Dusky Tse-tse Fly

[1] *Journal of Tropical Medicine*, July 1, 1903.

(*Glossina palpalis*) and the dissemination of typhus fever[1] by the Body Louse (*Pediculus vestimenti*). My address was illustrated by sixty coloured diagrams, exquisitely delineated by Mr. A. Terzi, and among them were: one showing the Common Cockroach (*Blatta orientalis*) and *Spirocerca sanguinolenta*, and another representing the Yellow Meal Beetle (*Tenebrio molitor*) and *Protospirura muris*. Terzi's diagrams were bought by Mr. Henry S. Wellcome in 1906 and ever since have been hanging on the walls of the Wellcome Bureau of Scientific Research.

In 1906, at the Royal Institute of Public Health, I endeavoured to organize a special department for the study of the parasites and diseases of household vermin, in order to anticipate, if possible, other methods of research and try to discover soonest any possible connexion there might be between the parasites of household pests and the diseases of man. With cancer chiefly in view, I began to study rats, mice, cockroaches, meal-worms and even bed-bugs[2] collected in London and the neighbourhood and, at the very start, interesting observations were made some of which were recorded briefly in press articles describing the inauguration of the new department on June 20, 1906. Unfortunately lack of pecuniary support prevented any further researches being carried out at the Royal Institute of Public Health.

Finally, last spring, at Strasbourg, I had the privilege of meeting Professor Johannes Fibiger, of Copenhagen, at the new Pasteur Museum directed by Professor A. Borrel, himself a great cancer student, and I was able to examine the beautiful photographs illustrating Fibiger's work on the carcinomatous growth of rats brought about artificially by feeding healthy rats on cockroaches (*Blatta orientalis*, *Periplaneta americana*) bearing the larvæ of *Gongylonema neoplasticum*.

I make these statements in order to explain that I went to work with some knowledge of the theories and views now prevailing on the nature of malignant neoplasms and that, though certainly in favour of the infectious theory, I endeavoured to approach the subject with an unbiased mind. Indeed, I determined to avail myself fully of the criticism, often very pertinent, brought forward by the adversaries of the parasitic theory and become myself the severest critic of the views I was inclined to favour.

La Romagna.

The region rapidly surveyed by me is called Romagna.[3] It is a territorial division of Italy lying south of the River Po, between the Reno and Cæsar's Rubicon, the foot-hills of the Apennines and the Adriatic Sea, or as Dante puts it:

"Tra il Po, il Monte e la Marina e il Reno."

In Cicero's time it formed part of *Gallia Cisalpina* and more particularly of *Gallia Togata*. The great Roman orator praises it mightily, calling it "the flower of Italy, the support of the Roman Empire, the ornament of excellence,"

[1] *See* chapter on "The Spotted Fever of the Rocky Mountains," in "A System of Medicine," edited by Sir Thomas Clifford Allbutt and Sir Humphry Rolleston, vol. ii, part ii, "Tropical Diseases," London, 1907.

[2] Moran (1891) transferred bed-bugs from the cages of cancerous mice to healthy ones, whereupon a large number of the latter developed cancer.

[3] The confines of Romagna have varied considerably at different periods. The division once formed the main part of the Exarchate of Ravenna, later it became an important part of the Papal States. It now comprises the provinces of Ravenna and Forli, but some authors hold that it should include also the provinces of Ferrara and Bologna.

and Strabo tells us how Ravenna, though built in a silty lagoon, was so salubrious that the fencing masters chose it for the rearing and the training of those who had to fight at close quarters.

To-day the Romagna is one of the most fertile and best cultivated of agricultural regions, and still might be one of the healthiest, were it not for the fearful cancer scourge which menaces it like the sword of Damocles.

The rich alluvial plains extending from the Apennines to the Adriatic shore are neatly divided into narrow rectangular areas of cultivated land by rows of trees, to the stems and lower branches of which are "married" or trained beautifully festooning vines. The supporting trees usually are elms and mulberries, but poplars, maples, willows and olives are used also. The white mulberry was introduced into Italy together with the silkworm in the latter half of the twelfth century, but all other trees are either indigenous or of ancient introduction, and were well known to Pliny and Virgil.

Principal Cultures.

The principal cultures are: wheat, barley, oats, maize, hemp, sugar-beet, peas, lentils, French and kidney beans, tomatoes, tobacco, and a number of fodder-plants such as red clover, medick, fenugreek, sainfoin, and various vetches.

At one time rice used to be grown extensively in the province of Ravenna, but its cultivation has been reduced and malaria has diminished in consequence. On the other hand, the cultivation of the sugar-beet has became paramount.

Food Plants and Animals introduced since 1500.

Whilst the majority of crops in Romagna have been cultivated from a very early age, there are a few which, being of American origin, cannot be older than four centuries. These are Maize, the Tomato, the Potato and Tobacco.

Maize was introduced into Europe by the Spaniards from South America, soon after the discovery of the Western world, and the history of its cultivation in Spain, France and Italy begins about the middle of the sixteenth century. It was already extensively grown and much used throughout Northern Italy at the beginning of the seventeenth century. Writing in 1631, Antonio Frugoli, in his "*Prattica e Scalcaria*," says: "Formentone is called by some Turkish, but in reality it comes from the West Indies, and there are four kinds, according to the colour of the grain, because some are red, others black, yellow or whitish, although it all belongs to one species; and it is found in various parts of Italy, particularly at Ferrara and the towns of Lombardy.

From the latter half of the eighteenth century, when pellagra was first described in Spain and Italy, until 1910, maize was looked upon as its very cause. Indeed, this ætiologic view, almost universally accepted, became in Italy, under Lombroso and his school, an intangible dogma. My epidemiological researches, carried out throughout Europe, the Southern United States of America, and the West Indies, together with the discovery that pellagra is endemic throughout the British Isles, have overthrown the maize theory completely.

The potato was brought to Italy from Spain about the middle of the sixteenth century. As in other parts of Europe, no one would touch it; some said perhaps it might not poison pigs, others anathematized it as a sinful plant because it was not mentioned in the Bible! It took centuries and many a famine to make us appreciate its virtues.

The tomato is of special interest, because it is one of the several foods accused of causing cancer. Indeed, some years ago this view was so much discussed in the daily papers that the staff of the Cancer Hospital was obliged

to repudiate the supposition, "seeing no ground whatever for entertaining any such charge."

The tomato is mentioned by Matthioli in 1554. Speaking of the egg-plant and other Solanaceæ, he says: "Another kind is brought now to Italy called golden-apples; these are flattened like russet apples and deeply furrowed; they are at first of green colour, but, when ripe, blood red on some plants, the colour of gold on others. They are eaten in the same way."

The fact that cancer existed in Europe long before the introduction of the brilliant and excellent Peruvian vegetable and the comparative scarcity of the disease in parts of Southern Italy, where the tomato is consumed daily in large quantities, negatives any theory associating the tomato with the causation of cancer.

Water Supply.

The Romagnoli make excellent wine, but their drinking water is bad. In some places spring water is available, but, as a rule, all the water used throughout the flat country is obtained from uncovered shallow wells fed by surface waters. This well water, raised by means of buckets, is peaty in some places and everywhere more or less polluted, the ground being fertilized extensively with stable, barnyard and human manures. The dangerous nature of the water is indicated by the frequent occurrence of typhoid fever and other diarrhetic diseases.

About the second century A.D. the Romans built in this region one of their grand aqueducts for the purpose of carrying a pure water supply from the mountains above Forli to Ravenna; ruins of its arches still may be seen in the vicinity of Meldola. At the beginning of the sixth century a Byzantine Emperor, Theodosius III, had it repaired, but it did not last long. A new aqueduct is now being built; it will convey excellent sparkling water from the many springs about Fumaolo to Cesena and Ravenna.

Apart from the usual incidence of cosmopolitan infectious diseases, such as measles, scarlet fever, whooping cough and mumps, the maladies most common in Romagna are tuberculosis, pneumonia and a not inconsiderable amount of diphtheria. Malaria, though present, especially along the Adriatic coast, is sparse and chiefly represented by tertian and quartan types.

At the commencement of the Christian era, Ravenna stood, like Venice to-day, in a lagoon, but gradually the lagoon was converted into a salt marsh and now Ravenna is four miles distant from the sea. With the changed conditions, malaria became prevalent and continued so until 1839, when the Lamone, a river flowing from Mount delle Travi to the Adriatic by way of Marradi, Brisighella and Faenza, broke its banks, flooded a vast area of low-lying marshy lands, filled it with its sediment, and started a series of hydraulic improvements by warping and draining, which, understood, extended and continued by man, have improved enormously both the agricultural and sanitary conditions of the region.

Prevalence of Cancer.

With regard to cancer there can be no doubt that it is gravely prevalent in Romagna. For many years Ravenna and Forli have held the unenviable privilege of being, of all Italian provinces, those most affected by malignant neoplasms. The average cancer mortality in Italy is roughly 1 per 1,500 of population; in Romagna to-day it stands almost as high as 1 per 500!

Professor Alberico Testi, Chief Physician in the Faenza Hospital, was the first to draw attention to the great prevalence and progressive increase of cancer in Romagna. His earliest communication on the subject was a paper he read before the Eleventh Congress of Internal Medicine, held at Pisa in October,

1901. He then stated that the great prevalence of cancer in Romagna was well known to the local physicians, and that its progressive increase was truly impressive. In the hospital of Faenza, where poor patients convened from a town and territory numbering about 40,000 people, they had witnessed so great an increase that cancerous diseases now represented quite one-third of all chronic affections admitted.

At the Third National Congress of District Medical Officers, held in Rome, October, 1909, Dr. Massimo Chiadini, of Forli, pointing out the excessive prevalence of cancer in Romagna, strongly urged that all district medical officers should study the dread scourge collectively and systematically in the interest of science and for the honour of their calling.

In June, 1923, in a letter to the Editor of the *Riforma Medica*, Professor Testi reiterated his warning. "Here in Romagna," he said, "not the physicians alone, but the people also are obsessed by the cancer terror. In my dual capacity of hospital physician and consultant, for over thirty years I have been able to follow the progressive increase of malignant new growths in our region. The spread of cancer has assumed very notable proportions."

Professor Sante Solieri, Chief Surgeon in the Forli Hospital, told me he had practised in Siena, Grosseto and Forli. "In Forli," he said, "the spread of cancer is truly fearful, in Siena cancer cases are not half as numerous, in Grosseto probably not one quarter."

On consulting the Mortality Returns of Faenza and territory, I found that the mortality from cancer is about double that from tuberculosis, a disease still sadly prevalent. Thus in 1922, in the town and territory of Faenza, there occurred forty-six deaths from tuberculosis, eighty-seven from cancer.

A fact which at once strongly impressed me as bespeaking the great prevalence of the disease in Romagna is that almost every physician I came across reported that there were, or had been, cases of cancer in his own family. I have the list. It is appalling.

Progressive Increase of Cancer.

All the local physicians and surgeons whom I happened to meet were unanimous in asserting that cancer, more especially cancer of the stomach, had increased steadily in frequency within recent years. I pointed out that a similar progressive increase in the cancer death-rate had been reported from many other parts of the world, but that it was looked upon by many cancer students abroad as only apparent, owing to a better knowledge of malignant tumours, improved methods of diagnosis, more careful certification of the causes of death, a more sapient reading of mortality returns and a longer tenure of life through immunity to infectious diseases. They answered that King and Newsholme's arguments were known to them, but that they had little faith in corrected death-rates, and could assign no importance to the alleged lengthening of human life in view of local circumstances and of the comparative shortness of the period under consideration. Their statements were based on personal experience of many years' duration. They considered the increase of cancer in Romagna to be real not merely apparent, and it was proved by the mortality returns. In all cases of tumour, the cause of death was ascertained by microscopical examination. The introduction of X-ray photography certainly had facilitated the early diagnosis of inaccessible tumours, but had not replaced the sound clinical, necroscopic and histological methods long used in all the major hospitals of Romagna.

Notwithstanding its progressive increase in certain regions, cancer exhibits in its incidence noticeable fluctuations from year to year. This peculiar fluctuation, common to all endemic diseases, has been pointed out by Testi,

Foa, Galli, and others who have made a special study of the disease in Italy.

Irregular Topographical Distribution.

In an unpublished work on "Cancer in Faenza and Territory," generously placed at my disposal, Professor Testi and Dr. Galli draw attention to the irregular distribution of the disease in town and country. In certain districts of the town the mortality from cancer is considerably less than in others; thus, notwithstanding greater density of population, it is decidedly less south of the Via Emilia, a main road which divides the town from East to West into two almost equal portions: the Northern comprising the so-called "Green" and "Yellow" districts, the Southern the "Red" and "Black" districts. The cases tend to group themselves in certain streets, or rather house-blocks, as can be seen on the spot-map subjoined by the authors.

In the country, likewise, cancer is unequally distributed and occurs in isolated houses. In the parishes surrounding the town the spotting is more or less uniform, but in the rest of the territory it presents striking inequalities.

Professor Ortali, of Ravenna, informed me that at Mezzano, a village, or mere agglomeration of houses, holding about 1,500 inhabitants, he had found cases of cancer in almost every family.

At Castiglione di Cervia, a locality long immune, he had seen four cases which had suddenly occurred in different families all at about the same time.

Dr. Chiadini, of Forli, stated that whilst cancer frequently occurs in the parishes of Romiti, Villagrappa, Villanova and Castiglione of Forli, it is unknown in the parish of San Varano, or, at any rate, during the last fifteen years not one case has been seen or heard of there.

In a letter recently received, Dr. Ghetti informs me that a "cancer belt" has been discovered about Cotignola.

Similar inequalities in the topographical incidence of cancer have been noticed throughout the world and have been reported frequently, yet they have not received the attention they deserve. To my mind, they are of the greatest importance: they strongly support the infectious theory and, properly investigated, should inevitably lead to the elucidation of the ætiology of cancer.

Not only does cancer show well marked variations in its topographical distribution, but, notwithstanding occasional increase in certain localities, total disappearance in others; the relative frequency of local incidence shows that remarkable persistence which is characteristic of the "foci" or "stations" of well-known endemic diseases such as malaria, leprosy and pellagra.

We need not leave the British Isles to find evidence of topographical inequalities in the incidence of cancer. Already, in 1865, C. H. Moore pointed out that in England cancer was more prevalent in the Southern and Eastern Counties, less so in the North-Western and Northern Counties and in Wales. Some years later, Haviland studied the subject and confirmed Moore's results. He went further, he endeavoured to find a reason for the local variations and, perhaps, to a limited extent, he did find it in the topographical and geological conditions. He asserts that there is "an infrequency in places characterized by elevated sites and limestone formations or even by sites subject to floods, but within the immediate influence of calcareous rocks," whilst "high mortality is associated with flooded, low-lying and clayey areas." In like manner, the intermittent fevers were long connected with the swamp and called paludal fevers, but, though the Ancients clearly indicated it, not until recently did modern science recognize the particular element in the swamp—the plasmodium-infected mosquito—which brings them into being.

In 1899 a Committee appointed by the Birmingham Branch of the British Medical Association investigated the influence of locality on the incidence of malignant tumours, in parts of Warwickshire, Staffordshire, Shropshire and Worcestershire. They concluded that there were districts in which cancer occurs with frequency much above the average, side by side with other districts in which it is rare. The high-mortality districts were usually poorly drained, flat, low-lying or bordering on streams, whereas the low-mortality districts were high, dry and well drained. They believed that there is a direct connexion between the presence of subsoil water within a certain distance of the surface and the prevalence of malignant disease.

As Hoffman says in his recent work on "The Mortality from Cancer throughout the World": "The fact is incontrovertible that an enormous range in cancer frequency is met with throughout the world, and that as yet no generally accepted theory in explanation of such a wide degree of divergence has been advanced."

Cancer Houses.

Although the term "cancer-house" may be a misnomer, there can be no doubt that, for an indeterminate period, in certain houses, house blocks or districts, cancer is prone to recur, affecting people of different family and stock who successively inhabit such houses, house blocks or districts.

The Cancer Committee appointed by the Birmingham Branch of the British Medical Association found that second and third cases of cancer occur in particular houses more frequently than can be accounted for by coincidence, and that particular groups of houses may be similarly affected. They suggested that this association implies contaminated soil. Old houses appeared to furnish a higher proportion of cancer cases than new ones.

In the town of Faenza, one single building was marked by no less than twenty-seven spots on the Testi-Galli map, and that meant that in a period of fifteen years no less than twenty-seven of the inmates had died of cancer, but the building happened to be an institution for the aged poor, and the preponderance of persons of the cancer age there assembled might well explain the high number of deaths from cancer. In some private houses an unusual number of cancer deaths, likewise, might be explained by an unusual number of aged lodgers, but there were other houses in which three, four, or more cases had occurred within comparatively short periods, either in the same family or in different families inhabiting them successively. With regard to these, the cancer incidence could not be explained in the same easy way, and the buildings were looked upon as "cancer houses."

In Cotignola, a small town of about 7,000 inhabitants, on the River Senio, the local Health Officer, Dr. Lorenzo Venturi, took Dr. Ghetti, Mr. Benediktsson and myself to see a house in Via Farini where five cases of cancer had occurred within a short period in different families that had inhabited that house. I examined the house very carefully; it was old and somewhat dilapidated, but well-built, spacious, amply ventilated and clean; it was inhabited by people of comfortable means. Having discovered evidence of the presence of cockroaches in two of the rooms on the ground floor, I asked permission to inspect the adjoining building. This granted, I found that the two rooms mentioned backed an old bakery oven. On inquiry, the proprietress of the bakery declared her oven room to be so badly infested by cockroaches that, notwithstanding repeated endeavours at eradication, she had not been able to make the least impression on the swarming insects. Rats were frequently seen in the courtyard and mice in the bakery.

Some days later I motored to Predappio together with Dr. Oreste, Provincial

Medical Officer of Forli, Dr. Teodosio Prati, Health Officer of Predappio, Dr. Chiadini, and Mr. Benediktsson. Predappio is a hill village of about 3,000 inhabitants, placed on the road leading from Forli into Tuscany which follows the upper course of the River Rabbi. Having reached the little town clustered round a rocky eminence, the Pietra d'Appia, I was shown, in Via Mulino, two contiguous houses where lately five cases of cancer had occurred. The first house visited belonged to the family Capacci, in which two cases of cancer had occurred, one of the breast, the other of the stomach. The next house, which adjoined a mill, was inhabited by the miller, Francesco Zoli, who had lost father, mother and grandfather from cancer of the stomach. This house was full of sacks of grain and flour. Both buildings were infested by mice, cockroaches, and other household pests attracted by meal.

Site Incidence.

Figures kindly supplied by the Health Officers and by the Directors of the Hospitals of Ravenna, Bologna, Faenza and Forli show that throughout Romagna cancer is essentially a disease of the upper part of the alimentary tract (mouth, tongue, pharynx, œsophagus, and more especially cardiac end of stomach). Dr. Utili, Radiographer to the Faenza Hospital, particularly insisted on the location of cancer in the cardiac region of the stomach and produced splendid radiographs in support. After the digestive tube and its accessory glands (the latter chiefly through metastasis), the organs most affected are those of the female generative tract and the allied mammary glands. With few exceptions I believe this is the case in most countries.

In their work on "Cancer in Faenza and Territory," Testi and Galli gave a table showing the relative frequency of site incidence in 1,124 cases of cancer which occurred in Faenza between 1891 and 1910. The figures are as follows:—

	M.	F.
Alimentary tract.		
Lip and tongue	11	0
Parotid	1	3
Pharynx	5	1
Œsophagus	5	3
Stomach and liver	308	287
Liver (primary)	21	17
Pancreas	8	8
Colon	16	22
Rectum and Anus	8	9
Mesentery	3	7
Retro-peritoneal glands	12	12
Abdominal sarcoma	7	8
Generative tract.		
Testicle	1	—
Penis	2	—
Prostate	1	—
Ovary	—	5
Uterus	—	116
Breast	—	33
Urinary tract.		
Kidney	8	4
Urinary bladder	18	3
Respiratory tract.		
Larynx	12	3
Lung and pleura	3	3
Thorax and mediastinal glands	2	3
Other organs.		
Skin	13	8
Bones	10	7
Brain	7	5
Cervical glands	6	8
Site undetermined	27	34
Total	515	609

To the figures reported by Testi and Galli I can add those taken from the mortality returns of the eighteen months. January 1, 1922, to June 30, 1923, and kindly supplied by the Health Officer of Faenza. They are as follows :—

	M.	F.
Alimentary tract.		
Pharynx	0 ...	1
Stomach and liver	33 ...	30
Liver (primary)	7 ...	0
Pancreas	1 ...	1
Colon	2 ...	2
Mesentery	0 ...	1
Abdominal sarcoma	2 ...	3
Generative tract.		
Uterus	0 ...	6
Breast	0 ...	3
Urinary tract.		
Kidney	1 ...	1
Urinary bladder	2 ...	0
Respiratory tract.		
Lung and pleura	1 ...	1
Thorax and mediastinal glands ...	1 ...	2
Other organs.		
Bones	1 ...	1
Brain	0 ...	2
Site undetermined	4 ...	3
Total ...	55	57

Sex, Age and Occupation.

The crude statistical data collected by Testi and Galli show that in Romagna, as elsewhere, cancer is more common in women than men, the greater prevalence being due to the frequency with which the uterus and breast are affected, as compared with the corresponding structures in man.

With regard to age, one may say that in Romagna cancer appears sooner and lasts longer in the female. It starts, as a rule, between the fortieth and forty-fifth years, reaches its maximum between the sixtieth—sixty-fifth years, continues high up to the eightieth year, then suddenly drops.

In males the greatest cancer prevalence is amongst "farmers" and "farm labourers" (202 out of 359 cases); in females among housewives (214 out of 541), but as the majority of housewives employ their spare time in weaving hemp—the oldest and most important local industry—this would account for the local belief that cancer prevails chiefly in women engaged in the textile industry.

Race Incidence.

In the course of my researches on pellagra throughout Europe, the West Indies, South America and the Southern United States, I had the opportunity of seeing many cases of carcinoma and other neoplasms in patients belonging to different races and living under very different conditions of climate, environment, food, clothing, housing and culture. Several were the kinds of tumours I saw in the West Indian Islands (Trinidad, Barbados, Grenada, Antigua). Cancer seems to be particularly prevalent and steadily increasing in some of the islands. The relatively high rate for the island of St. Thomas (118·0 in 1913) is notorious. Surgeon-General K. S. Wise has called attention to the indubitable increase of cancer in Trinidad and Tobago, where, during the

seven years 1911 to 1918, 823 deaths from this cause were reported (83 buccal cavity, 211 stomach and liver, 85 peritoneum, intestines and rectum, 217 female genitals, 56 breast, 2 skin, and 169 organs not specified). However, cancer is no new disease in the Antilles. Jackson, in 1867, stated that it was of common occurrence in Barbados, and Rufz, in 1869, mentioned 93 cases (of which 31 were uterine, 16 mammary) seen in Martinique during a practice of about twenty years. The old impression that cancer is very rare in tropical countries is not borne out by the facts. Dudley, in 1908, reported carcinoma of the cervix as very common in Manila. In the Southern United States cancer occurs more frequently in the coloured than in the white race.

Contagiousness and Heredity.

Notwithstanding the great prevalence of cancer in Romagna, the local physicians seemed almost unanimous in considering the disease not directly contagious. Cases of cancer in two, or even three members of a family, of master and servant, of master and dog, cat or hen were mentioned, but, in a locality where cancer is very prevalent, multiple cases are likely to occur in the same family or house without necessarily implying direct contagion. Against direct contagion stands the long, repeated, invariable experience of large hospitals in which numerous cancer patients are constantly under treatment. Probably, as in the case of elephantiasis, cancer may be acquired through some intermediate agent which determines the unequal topographical distribution of the disease.

With regard to the alleged heredity of cancer, opinion varied among the Romagnese physicians. The majority held that the hereditary transmission of cancer had not been demonstrated satisfactorily. The researches of Karl Pearson, including the family history of some 3,000 cancerous persons and a comparative study of the same number of non-cancerous patients, appear to establish the conclusion that "there was practically no difference between them in respect to the prevalence of the disease among their relatives."

Cancer in Animals.[1]

I came across several instances of animals suffering from cancerous disease in close association with human cancer cases. Sometimes the malignant growth in the animal had preceded, at others it had followed the disease in man. The cancerous animals thus associated were dog, cat and barn-fowl. These, as is well known, suffer from true cancer, indistinguishable from that of

[1] I made inquiry into any possible association noticed between cancer in man and cancerous diseases in the domestic animals, but found that no special attention had been given to the subject either by physicians or veterinarians. However, several cases were mentioned which had obtruded themselves on public observation. Thus Dr. Angelo Lama told me that in Faenza, at 21, Via Torricelli, Luigi Tassinari, his wife, a domestic servant, a cat and a dog had all died of cancer. The Veterinary Inspector of the Province of Ravenna informed me of a lady who had died of cancer of the uterus; she had a pet cat that died of multiple mammary cancer some weeks later. Professor Testi and Dr. Galli, in their work on Cancer in Faenza, mention a curious case. They say: "We may mention the case reported to us by Professor Fedeli concerning a man who, dying from cancer of the stomach, entrusted to his nephew two pet animals, a dog and a hen, which he had kept for several years in his own room and were dear to him. Some months later the dog died of a consumption, and was found to have a cancerous disease of the stomach and liver; not long after the fowl also succumbed to cancer of the œsophagus. Further, we may cite a case in our own practice of a gentleman of Faenza who died of carcinoma of the liver, when previously in his own family had died first a dog and later a maid-servant of a similar disease." On August 24 Dr. Ghetti drove me to see one of his cancer patients in the parish of Santa Lucia, Fondo S. Giovanni. This man, Giuseppe Dapporto, 61 years of age, had a dog 12 years old presenting a conspicuous mammary growth. Several farmers told me on inquiry that they had noticed new growths in cats, dogs, and especially in fowls.

man in both histological characteristics and biological features. I am inclined to attach considerable importance to the prevalence of cancer in domesticated animals which live in close touch with man.

House Vermin.

A cursory inspection of farmhouses and other dwellings in which cases of cancer had occurred invariably revealed infestation by the Common Cockroach (*Blatta orientalis*) (fig. 6). These insects, as a rule, were found in the meal-ark, an indispensable article of kitchen furniture present in all farm-houses. Besides cockroaches, I found in the meal-flour meal-beetles (*Tenebrio molitor* and *Tenebrio obscurus*), meal-moths (*Asopia farinalis*) and meal-mites (*Aleurobius farinæ*, *Tyroglyphus longior*). In the stables adjoining the living-rooms of farm-houses, beneath the alfalfa fodder-heaps, always I found cellar beetles (*Blaps gages* and *Blaps gibba*) and wood-lice (*Porcellio*), whilst mice, field-mice and rats everywhere abounded. The two so-called "cancer houses" visited by

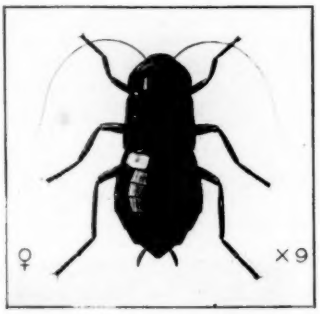


Fig. 6.—Common cockroach (*Blatta orientalis*).

me, one at Predappio, the other at Cotignola, the first adjoining a mill, the second a bakery, were both heavily infested with cockroaches and other meal-attracted vermin.

Now cockroaches, meal-beetles, meal-moths, cellar-beetles and wood-lice all harbour within their body-cavity or musculature the encysted larval forms of various kinds of round and flat worms which spend their adult stage in the alimentary tract of domesticated animals and man. The most important are certain Spiruroidea of the genus *Gongylonema* and one or two small tapeworms of the genus *Hymenolepis*.

Gongylonema.

Nematodes of the genus *Gongylonema* are of special interest with regard to the ætiology of new growths. Professor Fibiger has shown that a species discovered in the stomach of rats and named by him and Ditevsen, in 1914, *Spiroptera neoplastica* (= *Gongylonema neoplasticum*) (figs. 7, 8) is a frequent cause of gastric carcinoma in rats. The quiescent larval form of the parasite was found encysted in the musculature of cockroaches collected in Danish sugar refineries whence the infected rats had come. The beetles belonged to

the American species called *Periplaneta americana* (fig. 10), and probably had been imported with raw sugar from the Danish West Indies. By experimentally feeding healthy rats from other localities with gongylonema-infested cockroaches carcinoma of the stomach followed almost invariably.

Thirteen species of *Gongylonema* are already known to science: they have been found in man, ape, hedgehog, rat, mouse jerboa, guinea-pig, rabbit, squirrel, ox, sheep, goat, horse, ass, camel, fallow-deer, hog and domestic fowl. In some of these animals, such as cattle, sheep and hogs, the filiform,

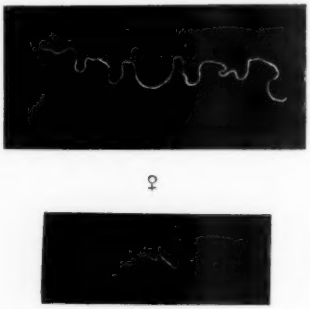


FIG. 7.—*Gongylonema neoplasticum*, natural size. (After Fibiger.)



FIG. 8.—*Gongylonema* in gastric mucosa. (After Fibiger.)

sub-epithelial parasite was found to be exceedingly common when special search was made for it. In man four cases only have been recorded, all from the United States of America, since 1916, when Professor Henry Ward, of Illinois, demonstrated the first case. The patient, in this case, was an untravelled girl, aged 16, who had lived always in Jefferson, Arkansas. The worm was noticed on October 1, 1916, beneath the mucosa of the lower lip; it wandered actively to and fro between lip and fauces after the manner of another filarioidea, the well-known African *Loa loa*. It was removed by means

of a needle by the attendant physician, Dr. Robert Lee Covington, of Jefferson, who sent it to Ward for determination. Ward's diagnosis was that the small thread-like worm belonged to the genus *Gongylonema*, and might possibly be *Gongylonema pulchrum*, a parasite of the hog, the larval form of which no doubt occurs in insects. He says:—



FIG. 9.—Eggs of *Gongylonema neoplasticum*.
(After Fibiger.)

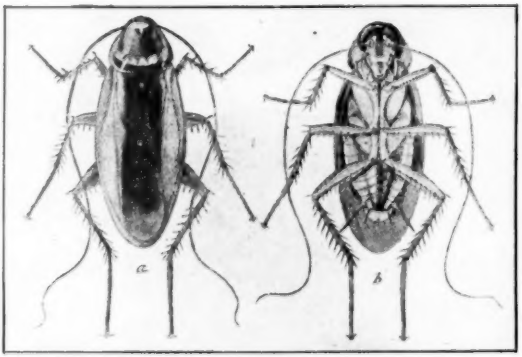


FIG. 10.—American cockroach (*Periplaneta americana*).

"The source of the infection in the case at hand cannot be positively ascertained, and yet recent studies suggest a very probable explanation. Thanks to the careful work of Ransom and Hall, which has extended over several years, it is now known that the larvæ of *Gongylonema scutatum* occur in various species of dung beetles, and that they have been raised experimentally not only in these, but also in croton bugs, and at the close of a month embryos were found encysted in the final stage and ready for transmission.

"In the present case it is easy to see how an infected insect, very likely a croton bug, might have been ingested whole, or some fragments of it included by accident in meal, flour, milk or other materials used in cooking. Such sanitary mishaps are very common with poorly prepared food, and thus the infection of the human host would have been achieved. The sanitary conditions noted in the clinical history of the case favour such an occurrence."

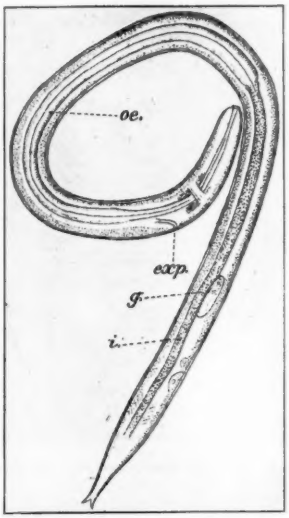


FIG. 11.—Larva of *Gongylonema neoplasticum*. (After Fibiger.)

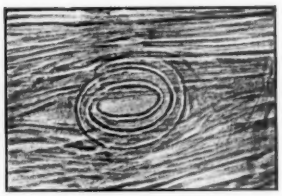


FIG. 12.—Larva of *Gongylonema* encysted in musculature of cockroach. (After Fibiger.)

In 1917, Dr. C. Wardell Stiles described a second case of *Gongylonema* infestation in man. The parasite was found in the lower lip of a girl in Florida.

A third case was reported by Stiles in 1921 from Georgia. The worm, again a female, 35 mm. long, but unfortunately badly macerated, also came from the mouth of a woman who verged on the age of 50.



FIG. 13.—Transverse section of rat's œsophagus showing *Gongylonema* in situ. (After Fibiger.)



FIG. 14.—Tuberous tumour in stomach of rat infected with *Gongylonema neoplasticum*. (After Fibiger.)



FIG. 15.—Enormous papillomatous tumour in stomach of rat from sugar refinery. Natural size. (After Fibiger.)

A fourth case was reported by Ransom in 1923. It occurred in Louisiana.

Until adequate material from man be available to establish the specific characters, Dr. Stiles suggests, on purely practical grounds, the name of *Gongylonema hominis* for the species so far found in man.

Though hitherto overlooked or unrecognized, it is probable that *Gongylonema* occurs far more frequently than supposed both in man and domesticated animals, but physicians have not made search for it post mortem, neither have our veterinarians looked for it at abattoirs. Yet, the little knowledge we have of the occurrence of *Gongylonema scutatum* in our cattle and sheep does not necessarily mean that in Europe this parasite is absent or less frequent than in the United States of America, or in the North of Africa, where it has been looked for by such competent men as Stiles, Ransom, Hall, Seurat, Fayet, Boulant. Several examples might be quoted to show how easily inconspicuous parasites may escape observation unless specially looked for. I need but

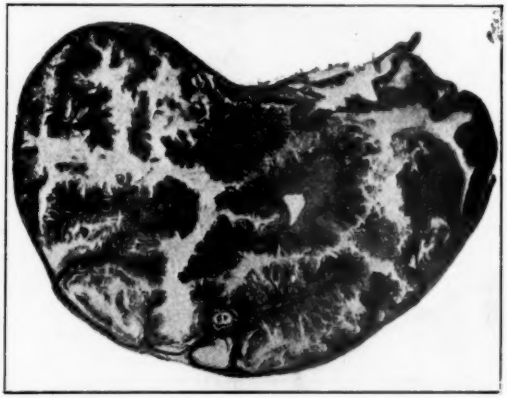


FIG. 16.—Transverse section of stomach shown in preceding figure × 2. (After Fibiger.)

mention the nymphal form of the cosmopolitan tongue-worm *Linguatula serrata* and the Ray Fungus (*Nocardia bovis*).

One might ask: "How is it that *Gongylonema* if present in man in Europe, has never come under observation, as in the four American cases just quoted?" The retort also might be a question: "Why was it not noticed sooner in America?" But are we sure that it never was observed in Europe? I believe it was. Indeed, I am inclined to ascribe to the genus *Gongylonema* some old imperfectly described nematodes, such as the one discovered in 1864 by Professor Carlo Pane, of Naples, Italy. Pane's worm appeared beneath the epithelial covering of the upper lip in a Neapolitan medical student 20 years of age. On account of irritation and itching of two or three days' duration, the young man everted and examined his lip in a mirror and, noticing something white moving beneath the epithelial layer, extracted it by means of a steel pen. The captured worm was brought to Professor Pane, who, considering it to be a *Filaria*, proposed to call it *Filaria labialis* on account of its site (fig. 17). It was a female

worm of hair-like breadth, measuring 30 mm. in length. The anus was placed ventrally near the posterior extremity, and the vulva opened at 3 mm. from the caudal end. Four small papillæ were noticed about the mouth. Pane gives no further details, but a semi-schematic drawing of the parasite by a noted Italian naturalist, Professor Paolo Panceri, reveals other important features. There are a short vagina and two

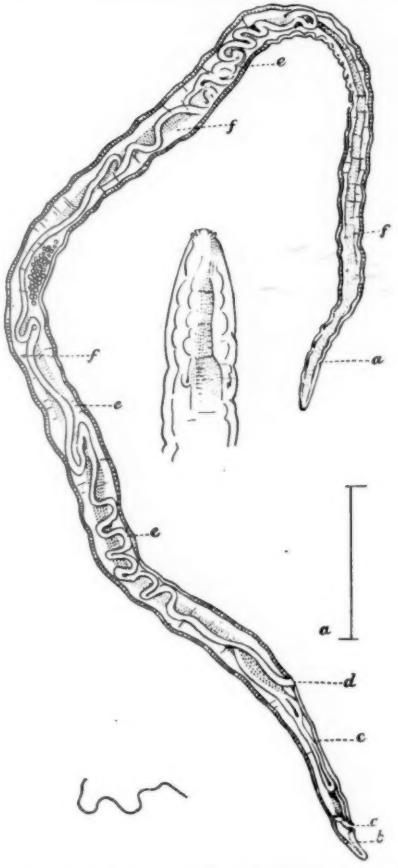


FIG. 17.—Pane's *Filaria labialis*. After drawing by Paolo Panceri showing characteristic features of *Gongylonema*.

divergent uteri plainly visible; the anteror uterus and ovary longer than the posterior, a long œsophagus consisting of a slender anterior portion and thicker, longer posterior portion and a somewhat blotchy outline which, together with other vague indications of unevenness on the surface of the anterior portion of the body, suggest the characteristic longitudinal rows of large cuticular bosses of *Gongylonema*. This being so, the name of *Gongylonema hominis*,

proposed by Stiles for the, as yet undetermined, species found in man, would become a synonym of *Gongylonema labialis* (Pane, 1864).

Doubtless in the near future *Gongylonema* will be found to occur frequently in man throughout the world. Probably, as in the case of *Hymenolepis nana*, it has been overlooked hitherto on account of its slenderness, transparency and peculiar location. At one time the Dwarf Tapeworm (*Hymenolepis nana*), not more than 25 mm. long, was considered to be a rare accidental parasite in man, but Stiles pointed out that there were indications showing that, in certain parts of the world at least, it would be found to be the most common tapeworm of man. He was right. The hookworm campaign, involving as it did a systematic and minute examination of fæces, incidentally brought it to light.

In their adult stage, gongylonema worms select as habitat the œsophagus and cardiac end of the stomach, but they may be found in other parts of the body, as in the lips, tongue or liver, the rumen in ruminants and the crop in fowls. This peculiar location corresponds with the most common site incidence of cancer in man and animals.

If *Gongylonema* were indeed a factor in the ætiology of cancer in man, not only should we understand the peculiar site incidence of certain forms of the disease, but also the simultaneous occurrence of similar neoplasms in animals, wild and domesticated, the unequal topographical distribution of the disease and the observed association between cockroaches and cancer houses in certain districts. Moreover, cockroach bionomics would explain the occurrence of the disease in old buildings and its prevalence in low-lying, clayey and damp districts, because these *Orthoptera* require not only warmth and darkness, but moisture especially.

Against any possible correlation between cancer and cockroach, someone probably might object by again adducing the twice-told tale of the insect's recent introduction into Europe. But this story of recent importation is based on a mistake of interpretation, repeated by all compilers of entomological text-books since White of Selborne told us, in 1790, that, prior to that date, he had never observed cockroaches in his house, but could not tell how long they had abounded in England. Of course the incidence of cancer goes back to time immemorial, and I believe I have myself adduced some interesting and important evidence in support of its great antiquity. The common cockroach is fully as ancient in this country and other parts of Europe. Indeed, the discovery of wild specimens in the Crimean peninsula, living under dead leaves, vegetable detritus, and stones in woods and copses far from any human habitation points to Caucasia as possibly its cradle land, unless it be a case of reversion from domesticity. The common cockroach is described and figured in unmistakable way by Thomas Moffett in his famous book on insects, published in 1634, many years after his death, and by the Italian, Mattioli, in his Commentaries on Dioscorides' Materia Medica, published in 1548, also by Aldrovandi and others, who in turn quote Greek and Latin authors, such as Pliny the Elder, to whom the fœtid "black-beetle" was well known as a bread-eater and polluter of meal. However, we should not forget that besides cockroaches, Darkling-Beetles (*Tenebrionidæ*) and Dung-Beetles (*Scarabæidæ*) may foster gongylonema larvæ. Indeed, certain coprophagous beetles seem to be their normal hosts.

Other Helminths.

But though *Gongylonema neoplasticum*, as shown by Fibiger, may certainly bring about carcinoma in the stomach of rats, it does not seem to be the sole parasite able to induce the formation of malignant new growths in these

rodents. Already in 1906, Borrel had incriminated *Cysticercus fasciolaris*, the larval form of the Cat's Thicknecked Tapeworm (*Tænia tæniæformis*) as a cause of sarcoma of the liver in rats and mice. His suggestion was based on the fact that primary sarcomatous tumours occurred in the liver and other organs of rats infested by the larvæ of the cat tapeworm. He did not consider the bladderworm to be the actual cause of cancer, but looked upon it as a possible carrier of some cancer microbe as yet unknown.

Four years previously Regaud had published two cases of malignant growth associated with bladder-worms in rats. The first, a female rat with mammary tumour and secondary peritoneal carcinosis, presented, on the free border of the liver, a cyst containing a living cestode larva. On this cyst, as also upon the various organs, were found cancerous nodules. The second rat had an omental tumour within a cavity of which lay a coiled-up strobilate larval cestode 25 cm. long.

At the Tunis Pasteur Institute, Bridré and Conseil, on examining 2,000 wild rats (*Rattus norvegicus*) found sarcoma of the liver in five of them. Four of these tumours enclosed each a cyst containing a cestode larva (*Cysticercus fasciolaris*). The bladderworms were embedded in the new growths which belonged to the spindle-celled type of sarcoma.

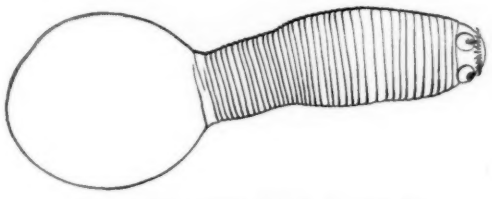


FIG. 18.—*Cysticercus fasciolaris*. (Original.)

In 1911-12 Woolley and Wherry examined 23,000 American wild rats, and found three with liver sarcoma each with a cysticercus in the growth.

In 1913, Borrel again insisted on a connexion between *Cysticercus fasciolaris* and a typical cancerous tumour of rats which can be transplanted in series. He pointed out that the bladderworm often is found in the very centre of the tumour, and claimed that observations by Regaud, Saul, McCoy, Bridré and others supported his view of the ætiological importance of *Cysticercus fasciolaris* in the incidence of sarcoma in rats and mice. Bridré had examined no less than 20,000 rats and had found the bladderworm in 8,000, a proportion of 40 per cent. Of these parasitized rats only twenty presented sarcomatous growths, but invariably the *Cysticercus* was lodged in the very centre of the tumour. In one instance Bridré had been able to witness the very beginning of the sarcomatous growth—intra-hepatic nodules no larger than ordinary shot, in each one of which he had found a tiny cestode larva surrounded by typical sarcomatous tissue.

Some years ago I examined about 800 rats (*Rattus rattus*, *Rattus norvegicus*) captured by rat-catchers at the London Meat Market, the London docks, and on board ships hailing from various parts of the world. In many of these rats I found *Cysticercus fasciolaris*, but I cannot recall a single case of sarcoma in the liver of the parasitized animals. In one rat (*Rattus rattus*) from the

Albert Docks the liver was so crammed with bladderworms that the hepatic tissue was almost obliterated, yet there was no evidence of cancer. In all rats and mice examined by me the *Cysticercus* was limited invariably to the liver. A few negative observations such as these have little value and do not disparage Borrel's interesting and suggestive indication.

In 1920 Bullock and Curtis produced a large number of cases of sarcoma of the rat's liver by the simple expedient of feeding rats with the ova of *Tænia tæniæformis*: the tumours arose from the encapsulating tissue surrounding the bladderworms (*Cysticercus fasciolaris*) (fig. 18).

Another parasite of man likewise incriminated by Borrel as a possible vector of the cancer germ is *Demodex folliculorum*, a microscopic vermiform Acarine which, head downwards, infests the hair-follicles and sebaceous glands, especially on face, alæ of nose, forehead, lips and chin. I have found it in comedones on the scrotum and penis of a syphilitic middle-aged chimney-sweep. If, as suggested by Borrel, this minute acarus is truly instrumental in producing new growths, possibly it might explain the frequency of cancer of the scrotum in chimney-sweeps, because comedones seem to be particularly common about the genitals of men who work in soot and coal-dust.

Teulschlaender reports the formation of cancroids on the legs of fowls previously affected by "scaly leg." This latter highly contagious disease is caused by a minute acarine—*Cnemidocoptes mutans*—which burrows within the skin, excites the cells to multiply, raises the scales and accumulates crusty exudates which give the legs a swollen, rough, unsightly appearance. These lesions, like those of human psora, are invariably complicated by bacterial infection.

Other helminths, especially Trematodes (*Schistosoma hæmatobium, Schistosoma mansoni, Opistorchis felineus, Clonorchis sinensis, Hepatica hepatica*, &c.), have long been looked upon as possible factors in cancer incidence, and not without reason; still we must be careful not to confound mere coincidence with a possible relationship.

If any of these or other worms do play a part in the ætiology of cancer, it can only be one of portage. They must act as carriers of some cancer-germ as yet unknown. Possibly the lesions to which they give rise may favour the penetration of the germ, or in some way prepare a suitable soil for its development. The peculiar galleries pierced by gongylonema through and beneath the squamous epithelial covering of the gastric and œsophageal mucosæ, so like those of the itch mite in the corneous layer of the skin, seem almost "streaks" for the culture of cancer germs. Thus, worm, specific germ, and the varied bacterial flora which usually follow constitute a kind of metabiosis —a process well known to early man, who not only understood it, but mastered it, controlled it, and applied it with wonderful acumen to the production of commodities such as cheese, wine and vinegar.

Blackhead.

An instructive example is furnished by the so-called Blackhead, a disease of turkeys, barn-fowls and other birds, caused by a protozoal organism—*Histomonas* (*amœba*) *meleagridis*—discovered by Theobald Smith in 1895. The protozoön invades the cæca and liver of its avian host, often giving rise to a fatal entero-hepatitis. Experiments carried out by Smith and Graybill, in 1920, showed that the incidence and gravity of the disease could not be explained so simply as was at first surmised, but that probably some additional factor was involved in the transmission of blackhead. Indeed, whilst feeding

young incubator-reared turkeys with the fæces of old ones which had passed successfully through exposure to the disease gave negative results, soil recently occupied by old turkeys was infectious to the young experimental birds. Two cæcal parasites—a Coccidium (*Eimeria avium*) and a nematode (*Heterakis papillosa*)—suggested themselves as possible coadjutors. The coccidium, confounded by some with the specific amœba, invades the epithelial cells, while the histomonas is purely a connective-tissue parasite. The experiments which followed, however, incriminated the worm.

"Blackhead was produced in healthy incubator-raised turkeys, reared in the open in an environment where blackhead occurred, but without direct contact with old turkeys and other poultry, by feeding cultures of embryonated eggs of *Heterakis papillosa*, whilst it was not possible to start the disease by feeding incubated fæces or cultures from which the worms and ova had been removed."



FIG. 19.—Section of cæcum wall of Manchurian eared-pheasant (*Crossoptilon mantchuricum*) showing immature forms of *Heterakis isoloncha* encysted in cæcal submucosa. (Original.)

Smith and Graybill have not defined yet the exact part played by *Heterakis papillosa* in the transmission and pathogenesis of histomonas. They say:

"The production of acute blackhead by feeding embryonated eggs to turkeys in whose cæca adults of *Heterakis papillosa* are already present seems incomprehensible at first thought. A tentative explanation is that the worms, when invading the cæca in large numbers break down the resistance of the bird."

Whilst working at the Prosectorium of the London Zoological Society's Gardens in 1907, on several occasions I found species of Heterakis in the cæca of pheasants and other birds. The cæcum wall of a Manchurian Eared-Pheasant (*Crossoptilon mantchuricum*) (fig. 19) is literally crammed with small cyst-containing nodules, in each of which a more or less immature specimen of *Heterakis isoloncha* was enclosed. These peculiar cysts in the cæca of

pheasants infested with species of Heterakis were described by me in a paper on "The part played by Metazoon parasites in tropical pathology," read at a meeting of the Society of Tropical Medicine and Hygiene, December 20, 1907. I endeavoured to show, from my personal observations on *Ascaris*, *Agchylostoma*, *Œsophagostomum* and *Heterakis*, that the route followed by intestinal parasites was not, in all cases, a simple direct slide down the alimentary tube with water or food, as then supposed, but that the majority of these parasites entered the body of their host, either through the skin or through the mucosa lining the upper part of the digestive tract, then travelled by way of the blood-stream, or through connective tissue, often encysting for a time to undergo development before finally reaching the anatomical habitats in which they usually are found after attaining maturity. Thus it is that we find the immature forms of Heterakis encysted in the cæcal submucosa from which, on reaching maturity, like the immature forms of Œsophagostomum, they escape into the cæcal cavities to conjugate and oviposit. The lesions produced by these worms no doubt give the blackhead protozoon its opportunity.

Whilst incriminating *Cysticercus fasciolaris* as a possible factor in the ætiology of hepatic carcinoma in rats and mice, Borrel pointed out, as surest proof of his conjecture, that, as a rule, the bladderworm was contained within the new growth. In the case of gastric carcinoma, ascribed to the agency of *Gongylonema neoplasticum*, Fibiger also states that he has found the worm embedded in some of the primary growths, but never in any of the secondary growths or metastases. But the permanence of the worm in the new growth is unnecessary since doubtless its part is that of carrier and sower; the sowing done, it has no further concern in the development of the tumour. A parallel case is that of filarial elaphantiasis, in which, almost invariably, the worms having started the growth, die and disappear.

Elephantiasis.

In view of the working hypothesis I am suggesting, let me briefly recall the history of the elucidation of tropical elephantiasis, because of the light it throws on the ætiology of cancer. First mentioned by Lucretius as "*Elephas morbus*" in his poem "On the Nature of Things"; well portrayed by Rhazes and Avicenna, tumorous elephantiasis finds an excellent exponent in Hillary of Barbados, who, in the middle of the eighteenth century, clearly perceived the part played by lymphangitis in the building of it. Then Wücherer, Lewis and Bancroft successively discovered the various stages of the nematode worm —*Filaria bancrofti*—which gives rise to it, whilst Manson demonstrated its disseminating agent—the mosquito. Before Manson's discovery, elephantiasis, just like cancer to-day, was looked upon by some as a hereditary diathesis, by others as a directly contagious disease, whilst the majority of physicians speculated on various food causes, ascribing it in turn to the eating of pork, stock-fish or water-fowl, to some tropical fruit, such as the fragrant *Keura* or *Kaldera* (*Pandanus odoratissimus*), or to the drinking of stagnant water or swizzled cocktail. It was Manson who pointed out the relationship between filariasis and elephantiasis.

Filarial elephantiasis is unequally distributed. It ranges throughout the tropical and subtropical belts, extending north as far as Sicily and Southern Spain in Europe and Charleston in the United States of America; south as far as Brisbane in Australia; but its prevalence varies considerably from place to place, as does the prevalence of cancer. Whilst absent in some places, in others elephantiasis affects more than 50 per cent. of the population. Of places

with highest incidence one might mention Amoy in Southern China, where Manson made his momentous discovery, Samoa, one of the Navigator Islands, in the Micronesian chain, and Cochin in Southern India. In the itinerary of his voyage to the East Indies, published in 1595, Jan Huygen Van Linschoten gives a picture of "Cochin leg" as he saw it on the Malabar coast during the five years he spent at Goa; it is the earliest drawing of elephantiasis that I have been able to find. In the West Indies elephantiasis is very common in some of the islands, as in Barbados, where it used to be called the "Barbados leg," or the "glandular disease of Barbados," and where, in 1913, I found it still unpardonably prevalent. In other neighbouring islands, such as Grenada, it is unknown. Whilst travelling through British Central Africa, Daniels found filariasis and its attendant elephantiasis to be very common at the



FIG. 20.—"*Elephas morbus.*" Photograph by Dr. Sambon, British Guiana.

northern end of Lake Nyassa, but at the southern end he met with only one case of filariasis and no elephantiasis.

We now know that filariasis is transmitted by mosquitoes belonging to several genera (*Culex*, *Ædes*, *Tæniorhynchus*, *Anopheles*). These act as necessary intermediate hosts, and the relationship is so wonderfully adapted that when the mosquito host is a night-biter, the microfilariæ, though still enclosed in their egg-membranes, like boys in a sack-race, actively leave the lungs and come swarming into the peripheral circulation, at night time, to meet their liberators. Where it is a day-biter, or in places where both night- and day-biting species are acting concurrently, the periodicity of the sallies is altered to suit circumstances. This amazing correlation, between the young of the parasitic worm within the body of the vertebrate host and their insect nurse

in the outer world, is analogous to many similar relationships which have become established between pathogenic protozoa and their arthropod disseminators, or between flowers and pollenating insects. Many flowers which open early in morning are visited only by particular butterflies which leave their nocturnal haunts at the same hour; other flowers do not open until sunset, and they are visited by hawk-moths, silk-moths, owlet-moths and other *Noctuæ* which commence their ramblings at dusk. Then, again, the emitting of flower scent is simultaneous with the flying time of certain insects. The flowers of *Hesperis tristis*, as well as other flowers visited by small nocturnal moths, give off no scent during the day, but exhale a strong alluring hyacinth odour at twilight; on the other hand, many flowers visited during the day become scentless at night.

After a quiescent stage of growth and metamorphosis spent between the thoracic muscles of the mosquito, the filariæ actively migrate to the insect's head and make ready to emerge by piercing the most delicate parts, either the membrane which connects the chitinous pharynx to the base of the hypopharynx, as I have seen, or after entering the labium, the membrane between the labellæ, as Dutton and Lebredo have shown. The escaped filariæ now get on to the surface of the skin, penetrate the tissues and reach the lymphatic vessels. Here they pair, attain full maturity and oviposit. As a rule, like other filarids, they cause little or no harm, but, not infrequently, by obstructing lymphatic trunks, they may give rise to various pathological conditions such as lymphatic varices, adenitis, chyluria, chylocele, chylous ascites, orchitis, abscess and elephantiasis. These morbid conditions have been explained by Manson as due to lymphostasis brought about by obstruction of lymphatics. A single worm, a bunch of tangled worms, or the prematurely extruded ova of a dilacerated female filaria, he considered, might be the cause of obstruction. This explanation, which has been accepted very generally, no doubt is correct, but insufficient. It assigns undue importance to the filaria and neglects the essential repeated attacks of streptogenes inflammation. Associated with certain ubiquitous bacteria (*Coccaceæ* of various kinds) is a necessary factor in the ætiology of all filarial diseases, each one of which may occur independently of filariasis, though they are certainly met with far more frequently in filaria-infested persons, whilst their prevalence obviously is strictly dependent on the greater or less prevalence of filarial endemicity. From the very first, whilst leaving the mosquito's mouth-parts and wriggling over the surface of a man's skin, the filaria is likely to become covered with bacteria. Certainly, lymphangitis or erysipelatoid fever—Fayrer's "elephantoid fever," the so-called fever and ague of Barbados"—is the prelude to all forms of filarial disease. Elephantiasis, the most prominent of these, invariably is ushered in by attacks of lymphangitis recurring again and again until mammoth growths result. One such tumour of the scrotum weighed 224 lb. That elephantiasis usually is a "filarial disease" is well proved by the excessive prevalence of the disfiguring growths within filaria stations and their great rarity elsewhere.

Recapitulating, in the case of elephantiasis, we have:—

First, several mosquitoes capable of fostering and disseminating the larval forms of *Filaria bancrofti* within those parts of their combined distributional areas in which temperature and other œcological conditions are favourable to the worm's development.

Secondly, the adult filaria inhabiting the lymphatic system of man, sacculating the vessels in which they nestle, and occasionally obstructing them.

Thirdly, certain ubiquitous micro-organisms (*Coccaceæ*), possibly carried into the lymphatic vessels by the filariæ at the time they invade their human host, giving rise to repeated attacks of lymphangitis and gradually producing those enormous and hideous tumours which we call elephantiasis or pachydermia. These bacteria soon kill off the worms, hence microfilariæ are found seldom in cases of elephantiasis.

Also in certain forms of cancer, if I interpret observed facts rightly, we may have :—

First, certain cockroaches, meal-beetles, and possibly also dung-chafers, capable of fostering and disseminating the young stages of a spiruroid worm of the genus *Gongylonema*.

Secondly, the mature *Gongylonema* inhabiting as a rule the alimentary tract and more particularly long galleries pierced in the stratified squamous epithelium lining the œsophagus and the cardiac end of the stomach.

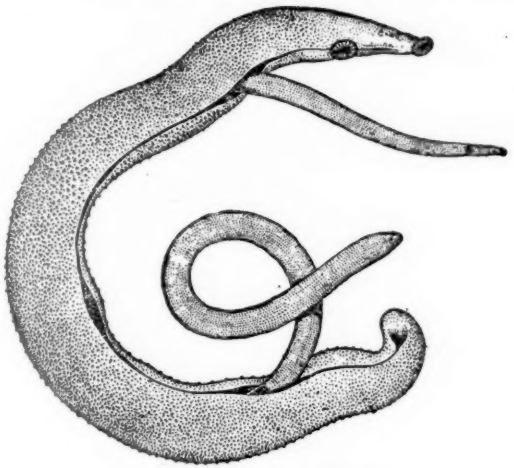


FIG. 21.—*Schistosoma hæmatobium.*

Thirdly, some ubiquitous micro-organism, possibly ultra-microscopic, which, favoured by the activities of the worm, may invade the tissue cells and give rise to malignant growths.

CALCULOSIS AND CANCER.

Other instructive and pertinent examples are afforded by the *Schistosomidæ*. Three, probably more, of these filiform blood-flukes occur in man. One, *Schistosoma hæmatobium*, described by Bilharz, in 1851, but, possibly like the *Heltu* worm (*Agchylostomum* of Dubini), known since time immemorial, is the cause of endemic hæmaturia in Lower Egypt, a disease represented in human form on ancient Egyptian monuments. Another, *Schistosoma mansoni*, previously confounded with the former, I separated in 1907, pointing out that it differed in morphological features, geographical distribution, anatomical habitat and pathogenic action. Moreover, though at that time the majority believed with Looss that there was no intermediate host, I suggested that the necessary

gastropod nurse which fostered its larval stages would be found to be specifically different from that of *Schistosoma hæmatobium*. All these things since have been proved correct by Holcomb, Da Silva, Flu, Castellani, Leiper, Lutz, Iturbe and others. The third known species is the Asiatic *Schistosoma japonicum*, first described by the Japanese author Katsurada in 1904.

Schistosoma infection is not infrequently associated with cancer, and it is of particular interest to notice that whilst *Schistosoma hæmatobium* (fig. 21), which affects the urinary and reproductive organs, is associated with carcinoma of the urinary bladder and genitals, *Schistosoma mansoni*, which affects the large bowel, is associated with carcinoma of the rectum and perineum. Kartulis (1885) discovered schistosoma eggs in an epithelioma of the right foot and leg of



FIG. 22.—Epithelioma of the penis, scrotum, and inguinal glands, secondary to old bilharziosis of the urethra and bilharzial infiltration and ulceration of the penis. (After Prof. Madden.)

a young Egyptian; therefrom he conjectured that the trematode parasite might enter directly through the skin, which we now know to be the case. However, it is not now to cancer but to vesical calculosis that I wish to draw attention. Stone in the bladder is so frequent in Lower Egypt (up to 80 per cent. of hæmaturic patients, Guiart) that it may be considered an endemic disease just as properly as its twin endemic hæmaturia, the endemic hookworm anæmia or the endemic mosquito-borne *Plasmodium* fevers. This great prevalence of vesical calculosis is brought about by the *Schistosoma hæmatobium*, the terminal-spined ova of which, like any other foreign body introduced into the bladder, may form the nuclei of urate, oxalate, or phosphate calculi. Eggs of

Schistosoma hæmatobium frequently have been found in the very centre of the stone by Sonsino, Looss and others. Probably bacteria are concerned in the formation of these calculi, just as "iron bacteria" are now known to be implicated in the obstruction of water pipes with deposits of iron oxide (iron rust) made from iron compounds present in the water in soluble form. Certainly intercalated layers of phosphates in uric acid stones indicate recurring attacks of cystitis. The majority of these stones are formed within the urinary bladder, hence, renal colic is uncommon in Egypt. Thus, in the Nile Delta, we find endemic calculosis brought about by a trematode worm and restricted to the discontinuous habitats of certain freshwater snails (several species of *Bullinus*) which foster the larval forms of this particular worm.

It is obvious that, by eliminating the gastropod intermediate host, we should prevent Schistosoma infection and thereby abolish hæmaturia, cystitis, calculosis and cancer, in all of which the blood-fluke is an important causative factor.



FIG. 23.—Epithelioma around the anus originating on site of extensive bilharzial disease in and around lower end of rectum. (After Prof. Madden.)

Wonderfully acute were those ancient Egyptians who, by consecration, protected the White Ibis (*Ibis æthiopicus*), a large curlew-like bird which every midsummer, with the "menstruating" or rising Nile, flew to the Delta to destroy the "winged serpents"[1] or alate worms, by devouring the freshwater univalve molluscs that harboured them. Is it unreasonable to surmise that the Ancients pitted the Ibis against the blood-fluke, they who used netting to bar the fever-mosquito, who pitted cat, mongoose, kestrel and rat-eating snake against the plague-carrying rat and the pellet-rolling Dung-Chafer against the Heltu worm (*Agchylostoma duodenale*)? And surely they must have known the schistosome, with its ventrally curved alate sides, just as well as they knew the itch mite, the hookworm, the pork-measle bladderworm, and the scriptural "Fiery-serpent" or Guinea-worm, still called serpent or dragon (*Dracunculus*) in our scientific nomenclature? For we know that they domesticated the ibis and that their wise priests, who wore linen clothing and

[1] These were exceedingly small according to Pomponius Mela.

shaved both head and body to prevent body vermin, only washed in water from which the ibis had been drinking. So say Plutarch and Ælian.

The agency of calculosis in favouring the production of malignant tumours is proved by the well-known association of carcinomas with biliary calculi. Cancer is hardly known to occur in the gall-bladder, except when the latter contains or has contained gallstones brought about by micro-organisms or liver flukes.

Among liver flukes particularly connected with carcinoma of the gall-bladder and larger gall-ducts are the Asiatic Liver Fluke (*Clonorchis sinensis*) and the European Cat Fluke (*Opistorchis felineus*). Both these trematode worms have a similar life-history. Their miracidia infest freshwater snails and mussels; their cercariæ become encysted in the musculature of Cyprinoid

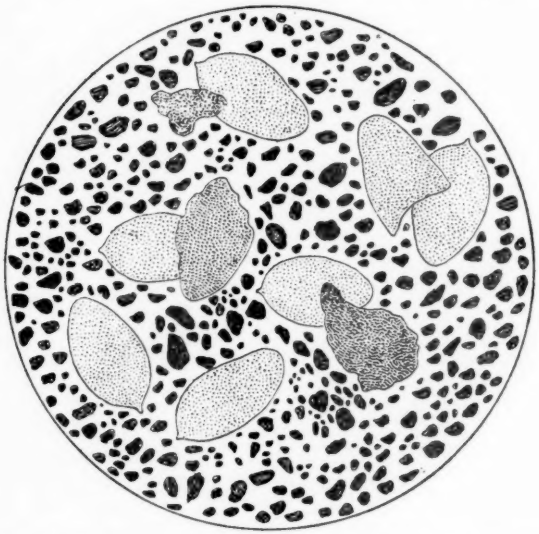


FIG. 24.—Section through nucleus of urinary calculus containing ova of *Schistosoma hæmatobium*.

fishes, and man becomes infected by eating the cyst-containing fish flesh in a raw state, as is the custom in many Asiatic and European countries. Askanazy in 1901 was the first to call attention to *Opistorchis felineus* as a cause of carcinoma of the liver in Prussia.

The multiplicity and variety of external and internal parasites, so far considered as possible factors in the ætiology of cancer, exclude any idea of specificity, even though many other kinds of external and internal parasites in no way seem concerned. Yet one would like to know how it is that whilst *Cysticercus fasciolaris* frequently gives rise to sarcoma of the liver in rats, *Hepatica hepatica*, which plugs the biliary canaliculi and extends through the hepatic parenchyma cramming it with its ova, does not appear to produce neoplasms in sheep. Again, how is it that whilst *Gongylonema neoplasticum*

will almost invariably produce carcinoma in the stomach of piebald rats and white mice, no such result can be obtained either in rabbits or field-mice, however heavy the experimental gongylonema infestation?

Should the cancer-producing acarine- or worm-parasites act merely as irritants, necessarily they would add only number to the already long list, which includes all possible kinds of physical, chemical and mechanical injuries.

The Irritation Theory.

However different may be the views hitherto expressed concerning the causation of cancer, on one possible factor there seems to be a general agreement—that is the importance of irritation. Yet, with regard to the nature and mode of action of the irritant there is great diversity of opinion.

Let us endeavour briefly to examine the mass of conflicting evidence before us, classifying it as far as possible.

Two principal modes of action are to be considered: the sudden, single, intense blow and the long-continued or oft-repeated irritation.

Let us consider first the blow. It is stated that single severe injuries, such as blows and fractures, have frequently been followed by the development of neoplasms. The female breast, on account of its exuberant development and exposed position, is injured so easily by blows that public opinion, confirmed by Courts of Justice, considers the blow a very likely cause of cancer, no matter how many years have passed between the infliction of the injury and the manifestation of the disease. But consider: what woman has not received an accidental blow upon the breast in the course of her life?

Statistics show that about 10 per cent. of patients with malignant mammary tumour can and do assign a definite injury as the initial cause. But we should not forget that whilst contusions and injuries of the breast are common in women, only a small proportion of such accidents are followed by malignant tumours, and that, whilst all women are liable to blows, the comparative frequency of cancer of the breast varies greatly in different parts of the world; thus, for example, the English rate (17·9) is nearly ten times greater than the rate for Japan (1·8). Surely wife-beating is not more common in merry England than in "Dai-Nippon," where, from early times, unchivalrous mal-estimation has surrounded the sweet, gentle, soft-voiced, unselfish "daughters of the Rising Sun." Then, again, it is not carcinoma that follows the blow, but sarcoma, a far less common tumour of the breast. Surgeons see many patients suffering from sarcomatous and carcinomatous tumours who cannot recall any injury to the affected part, and, though inclined to believe that a single blow occasionally may determine the development of a malignant growth, in their writings they seem loth to express any definite opinion as to a causal relationship between blows and cancer.

The great majority of blows do not give rise to tumours, and in most cases of injury followed by cancer, the connexion, to say the least, may be accidental. Certainly, it would be difficult to find a starting point for cancer upon or over which the patient could not remember having received a blow or injury of some sort within the two or three years preceding its appearance.

Many cruel, unnecessary experiments have been performed on old bitches either to prove or disprove the agency of traumatism in the causation of malignant tumours. The animals have been subjected to repeated blows upon the mammary region and others parts of the body during several months, yet though inflammatory swelling necessarily, and abscess sometimes, followed, no new growth ever was produced, notwithstanding that dogs are prone to cutaneous epithelioma and more especially to mammary cancer.

No doubt at times the single blow may prove a predisposing or determining factor in the development of a neoplasm, but the evidence adduced is, if anything, adverse to any direct causal relationship. Who knows not that tuberculosis of bone may develop after an injury? But a blow cannot engender Koch's bacillus; it merely ruptures an encapsuled focus which up to the time of injury had produced no symptoms. Is it not possible that when a sarcoma develops at the seat of a fracture, or a carcinoma of the stomach follows a blow dealt on the gastric region, the neoplasm was already present at the time of the injury, though as yet not made manifest by any symptom?

Epithelial cysts in the palm of the hand arising from torn pieces of skin carried into the deeper tissues by knife or shot have been adduced as examples of tumours caused by a single trauma, but surely cysts have no relation whatever with true neoplasms.

Now let us consider the agency of chronic irritation. Greater importance has been assigned to continued or frequently repeated local irritation, either mechanical or chemical, in the causation of cancer. It is claimed that most tumours appear after long-standing irritation which brings about inflammatory reaction involving long-recurring destruction and regeneration of tissue.

The various irritants incriminated are innumerable. I need mention but a few to give some idea of their diverse nature. Thus we find mentioned: contracting scars and carious teeth; pressure of pipe-stem in man; pressure of tight-laced corset in woman; changes produced by light, X-rays or radium; by coal, tar, pitch, naphthalene, aniline dyes, petroleum, vaseline, tobacco, arsenic, soot or manure; the dust from metal factories or spinning mills; certain diseases such as chronic endometritis, phimosis, psoriasis, leukoplakia, syphilis and tuberculosis cutis; betel-nut chewing, scalding drinks, coarse, irritating foods, badly-fitting tooth-plates and kangri-burns.

It would be long and tedious to discuss all these and the many other causes of cancer. We need consider but a few examples illustrative of the various types of irritation. However, in a general way, it is well to remember that against each case in which one or other irritant appears to be the essential factor, there are thousands more in which the very same irritants never give rise to cancerous formation.

Pipe, Lip-plug and Bit.

Pressure of the pipe-stem long has been considered a plausible cause of epithelioma of the lip in man, but this growth also occurs in smokers of cigars or cigarettes, and many cases have been reported in non-smokers. Often, instead of appearing in the pipe situation, it is found in the median line, or on the side of the mouth opposite to that usually occupied by the pipe. Some incriminate the long, heavy pipe, others the short cutty pipe, which, held firmly between the lips without support from the hand, conveys a painful degree of heat. Sir John Bland-Sutton points out that while cancer of the lip sometimes is spoken of as "countryman's cancer," on account of the frequency with which it occurs among agricultural labourers who use dirty clay pipes, "many professional men, including the vicar of the parish smoke as hard as the labourer, yet it is excessively rare to find a case of cancer either of the upper or lower lip among them." The American Indian is said to be practically immune to cancer, yet he has always been a great pipe smoker. Was it not he who first taught us the use of the "saintly herb," or "Devil's weed"? Was it not he who passed to us the beautifully carved stone-pipe which he had been smoking for centuries preceding the landfall of Columbus?

Was there no cancer of lip or tongue in this country previous to Raleigh's introduction of the noxious smoking habit?

And what about the use of labrets or lip-plugs so widely distributed throughout the world and perpetuated since time immemorial? The Tlinkits of Alaska, the Eskimo, further north, the Botocudos of East Brazil, the African women along the Nile and the Zambesi and around Lake Stefanie, all pierce or slit their lips and gradually stretch the openings by successively inserting wooden pegs of large dimensions as the lip apertures become more

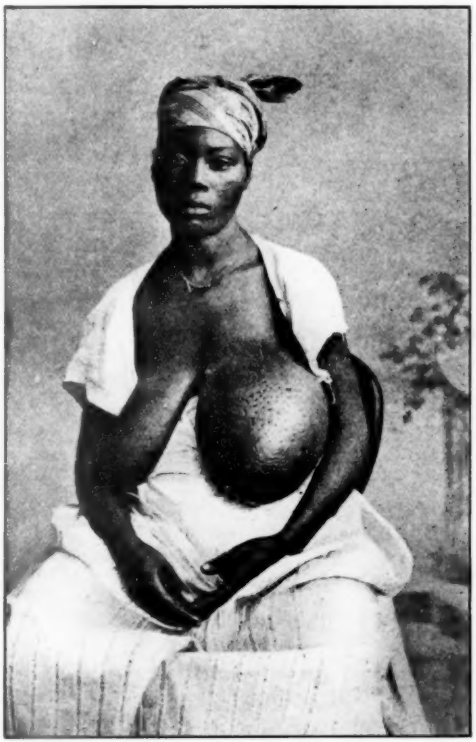


FIG. 25.—Elephantiasis of left breast in a negress of Martinique, West Indies.

distended. Thus, discs of wood, shell, ivory, glass, quartz or metal, two or three inches in diameter, may be seen fixed stud-like in the upper lip, while cylindrical pendants, often weighing a quarter of a pound, drag, elongate, evert the lower lip, hideously exposing teeth and gums. There can be no doubt as to the irritation caused by these strange ornaments, which often turn the upper front teeth inward by their pressure. Yet never have they been accused of producing anything more serious than comicality, especially when the duck-billed girls start chattering and laughing. Furthermore, with regard to pressure

of a short clay-pipe stem, considered as a possible cause of epithelioma of the lip, does the constant irritation produced by the bit in the mouth of horses give rise to malignant tumours either of tongue or lips?

Corset and Foot-binding.

Long ago it was suggested by Oelsner, and repeated by others, that pressure exercised by a tight-laced corset might cause cancer of the breast in women,



Fig. 26.—Fungating carcinoma of left breast in a negress of Grenada, West Indies. (Photograph by Dr. Sambon.)

but the several cases I have seen among West Indian negro women, who wear hardly anything around their chests, does not support the corset theory of mammary cancer, neither have we any evidence to prove that cancer of the breast was more prevalent between the sixteenth and nineteenth centuries when this beautiful part of the female body was tightly stayed in Greenland whalebone, Brazil cane and bars of steel.

If compression were to cause cancer, surely the foot would be the site of most frequent incidence, since no part of the human body has ever been subjected to greater pressure. Consider the "golden lily" or contracted foot of Chinese women. There is no better instance of compression. The "bow foot" is produced by applying a cotton bandage four yards long and three to four inches broad. The free end of the bandage is laid on the inside of the foot at the instep, then carried over the toes under the foot and round the heel so as to draw the toes across and beneath the sole, forming a bulge in the instep, while the beautiful arch of the foot becomes an abrupt, deep cleft. After a month, the tortured foot is soaked in hot water and the bandage unwound. As a rule, the foot is found to be ulcerated and, may be, one or two toes come off with the bandage. After proper attention, the foot is bound up again by means of a fresh bandage drawn up more tightly, while the binding is continued and renewed for two or three years or, indeed, until the foot is fully set. Cancer does not follow, and the poor victim of fashion is repaid by the joy and pride of being able to shoe her unsightly stump or hoof with a "golden lily," a mite of a slipper 4 to 5 in. long. The Chinese poet compares her stiff, tottering gait to "the waving of the willows."

Surely cows from milking are subject to constant mechanical irritation of their teats; yet they do not suffer from mammary cancer. The constant irritation of the harness on horses may cause excoriations, followed by inflammatory lesions due to the bacterial flora of the skin, but it does not give rise to malignant new growths.

The Pessary.

Carcinoma of the neck of the womb has been ascribed to the injuries incidental to childbirth and, indeed, it is especially common in women who have borne children. Everyone knows that the cervix of multiparous women is disfigured by radiating fissures, yet, though apparently bearing a causal relation to cancer of the neck, parturition trauma cannot be held responsible for cancer of the body of the uterus. Besides, cancer of the cervix often occurs in nulliparæ and virgins.

Were irritation and ulceration necessary for the production of cancer in the cervix, surely, as pointed out by Sir John Bland-Sutton, the pessary would be a chief cause. But no one has attributed any malicious influence to vaginal pessaries so far as cancer is concerned, though over 60,000 are sold yearly in this country. In his classic work on "Tumours, Innocent and Malignant," Sir John mentions several cases of long-forgotten pessaries, and states that the patients were examined under the impression that they were suffering from advanced cancer. In one of them the pessary had been in the vagina no less than twenty-seven years!

Kangri and Scaldino.

With regard to the Kangri-burn theory of epithelioma of the skin, again recently brought forward by Dr. Neve, I must point out that the "Scaldino" (fig. 27), a small portable earthenware brazier, is widely used throughout the south of Italy in winter, especially by women of the poorer class. There, however, it has never been accused of giving rise to cancer, though used for the same purpose and in the same way as in Northern India. Indeed, cancer incidence is much lower in the south than in the north of Italy, where the scaldino is rarely, if ever, used. The scaldino is similar in size and shape to the Kashmiri kangri, but, as a rule, lacks any insulating casing. It is heated in the same way

by means of incandescent charcoal kept smouldering in ashes. Roger Williams doubts the cancerous nature of Neve's cases, and, in evidence, points to the absence of metastases.

Similar destructive criticism might be applied to the many other forms of trauma, alleged as causative factors in the production of cancer. Yet, notwithstanding, we must recognize the indubitable influence exercised by various



FIG. 27.—The "Scaldino," a hand brazier very widely used during winter in Southern Italy.

irritants, animate and inanimate, in favouring the incidence, and more especially the site incidence, of malignant tumours. There can be little doubt that a single intensive blow or knock on breast, testicle or bone may be followed occasionally by a sarcomatous tumour. We know that carcinomatous tumours do often arise in a chronic gastric ulcer, in a scar left by a burn, in a fissure

following laceration at childbirth, in a leukoplakia patch of the tongue or at the edge of an old tuberculous sinus. We know that carcinoma of the gall-bladder is frequently associated with calculous cholecystitis; that cancer of the mouth is common in Tamil women who are inveterate betel chewers, and that, in this country, scrotal cancer has been observed frequently in chimney-sweeps and coal-heavers. But we should be careful not to fall into the usual confusion between *post hoc* and *propter hoc*. We must not mistake a predisposing or adjuvant factor for a true causative agent. Take the coal-miner, for instance. Fabry (1900) has shown that he is particularly prone to cutaneous tuberculosis on account of the numerous hand-injuries incident to his work; yet, because tuberculosis is known now to be an infective disease, due to the presence of the bacillus of Koch, no one would dream any more of considering coal dust a cause of tuberculosis.

I am fully convinced that irritation is not an essential cause of cancer, yet there can be no doubt that repeated irritation is a potent factor in the development of the disease and more especially in determining its location. Cancer often develops without any recognizable irritation; it is certain that neither trauma nor irritation play any part in the formation of secondary growths (metastases), whatever may have been their rôle in the initiation of the primary tumour. However, it is conceivable that injury (trauma) or irritation may determine or hasten the development of a malignant growth by lowering tissue resistance and thus favour a latent cancer germ. Indeed, we know that cancer is more liable to attack disordered, involuted or chronically diseased organs than those which are normal. An instructive example is seen in the frequency of cancer of penis and sheath in the ox and gelding as compared with its rarity in bull and stallion. Hence it has been argued that the atrophy following castration renders these parts more vulnerable, but the reason may be different. Indeed, cancer of the testicle is very common in the stallion, and its frequency, like that of cancer of the penis and sheath in geldings, may be due to the remarkable frequency with which agamous forms of *Strongylus equinus* affect the horse's genitals. *Setaria equina* is another nematode which may be found in the same parts. Other parallel examples might be quoted. Thus, at Chicago, in over two million cattle examined there were forty-nine cases of external cancer, and, singularly enough, all these were epitheliomas of the lachrymal caruncle. Such a location would be difficult to explain by any other theory than parasitism. We know that certain filarioidea such as *Thelazia rhodesi* live within the lachrymal canals of cattle, often causing serious irritation. They may be numerous and at times give rise to enzoötics.

Food and Cancer.

Many have ascribed cancer of the alimentary tract to constant irritation produced by coarse food, but how reconcile this theory with the stated rarity of the disease amongst raw food vegetarians and, again, in the alimentary tract of horses? In the horse, the œsophagus is liable to form pouches and to undergo distension and impaction with dry and irritating food materials. These pouch-like distensions, or "jabots" filled with chaff or meal, often cause serious pressure trouble and, unless it be possible to empty them by forcible external manipulation, may necessitate surgical treatment.

At present much importance is being ascribed to food in the causation of cancer. Indeed, the new vitamin theories have been made to cover the whole

range of disease from rickets, scurvy, sprue and beri-beri to pellagra, pyorrhœa alveolaris and cancer. To me, it seems strange that after the great advances recently made in the natural history of disease, especially tropical disease, we should be going back to the old exploded food theories which ascribed rickets, scurvy and scabies to deficiency of nutrition; leprosy to the eating of crimped salmon[1] or stock-fish; pellagra to garlic, onions, rancid oils, or Indian corn (*Zea mays*); beri-beri to polished rice[2]; cancer to tomatoes, salt-pork or tinned foods; sleeping sickness to the bitter cassava (*Manihot utilissima*)[3]; elephantiasis to salt-fish, water-fowl, or to the bright orange-coloured fruit of the fragrant kaldera (*Pandanus odoratissimus*), urinary calculosis to Norfolk dumplings and lousiness in children to the eating of earth nuts (*Bunium flexuosum*).[4]

The very different foods and food-habits of the peoples of various races within the world-wide range of cancer and the peculiar unequal distribution of the disease within comparatively small areas under the very same food conditions, strongly militate against any theory incriminating particular foods or food products, deficiency of food, lack of vitamins, malnutrition or excessive food indulgence.

Special researches into the history of foods and cookery, in all countries, throughout the ages, qualified me for investigation into any connexion there might perchance be between cancer and certain foods, food-products, or food-habits. I found none, unless we surmise a possible contamination of cereal products with infected cockroaches or meal-worms and a further possible danger in the eating of insufficiently cooked meats in which either certain helminths or the germs of cancer might be absconding.

"Ghosts."

An objection often brought up against the parasitic nature of cancer is the peculiar late age-incidence of the disease both in man and animals, but this objection is nullified by a similar lateness of inception in several well-known parasitic diseases. Thus tuberculosis, though frequently contracted in early childhood, may not manifest itself sharply until the age of puberty or still later. A like long period of latency is witnessed in syphilis; children who have been impregnated with the specific organism may not manifest any sign of the infection until the fifteenth or twentieth year. This fact is so well known that Ibsen based on it his play "Ghosts," and surely no medical man will deny the

[1] In his "Natural History of Ireland," London, 1652, Dr. G. Boate applauds the action of Parliament in prohibiting the use of salmon out of season, because he believes it to be the cause of the leprosy then prevalent in Ireland.

[2] Japanese physicians are beginning to recognize that beri-beri and the avitaminosis brought about by a diet of polished rice, though clinically similar, are two different diseases. The use of polished rice in Japan began towards the end of the seventeenth century. At that time beri-beri already prevailed extensively in Tokyo, Kyoto and Osaka. How ancient the disease is no one knows, but accurate descriptions of its clinical features appear in literature as early as 1315, in the Kamakura age.

[3] In 1902 Dr. Hans Ziemann (see *Journal of Tropical Medicine*, October 15, 1902) suggested that sleeping sickness might be due to the eating of raw or unsuitably prepared manioc. He concludes his paper by saying: "Whether my assertion that sleeping sickness is really a toxic disease in the same sense as pellagra be justified or not, whether my hypothesis that the ingestion of manioc unsuitably prepared causes the disease, be confirmed or not, I consider I am justified in making the following statement: In the blood of persons suffering from sleeping sickness there is no exciter of disease that can be cultivated by the usual methods."

[4] George Johnston, in "The Botany of the Eastern Borders," London, 1853, writes: "*Earth-nuts: Arnuts.* Common in old pastures and on the banks of open deans. May-June.—Children dig up the roots and eat them; but they are hindered from indulging to excess by a cherished belief that the luxury tends to germinate vermin in the head."

reality of his ghastly picture. In leprosy again we have an example of long latency, hence physicians ascribed to this disease an incubation period varying from a few months to as many as twenty-seven and thirty-two years. Leprosy is seldom seen below the age of ten years. An example even more appropriate to our case is that of elephantiasis. This disease is unknown in infancy, is rare in childhood, but common in adolescence, and increases in frequency with each decennium. Yet children are just as subject, if not more so, to mosquito-bite and, consequently, to filarial infection. Many other examples might be adduced. Thus in pebrine, a silkworm disease, we can actually see the causative organism (*Nosema bombycis*) in the egg of mulberry bombyx, but it remains quiescent during the embryonic stage, and does not develop until the growth of the caterpillar begins.

The Unknown Germ.

It may seem idle to speculate on the nature of a possible cancer germ when general opinion seems adverse to any parasitic cause, and, instead of grouping cancer with the granulomatous diseases, such as syphilis, tuberculosis, leprosy, verruga and granuloma fungoides, which it most resembles, endeavours to see in it fantastic unused embryonic remains, suggested by the teratomas which never should have been mixed up with malignant neoplasms. Of course, some teratomas occasionally may assume the appearance of malignant tumours, but only when a real malignant new growth invades them and extends into the patient's tissues. Certainly, the unequal topographical distribution of cancer throughout the world strongly indicates a biological cause, and idle is the objection that failure to demonstrate the true causal organism means that there can be no such organism. The history of tuberculosis, that of syphilis, the history of many other diseases, now proved to be of parasitic origin, should teach us caution in expressing a contrary opinion, especially when we cannot propound a theory of cause capable of explaining all the facts. Consider the case of yellow-fever, once ascribed to earthquakes, insolation, atmospheric electricity, want of acclimatization, excessive indulgence in food and drink, soil effluvia and a thousand and one parasites belonging to the bacteria, the fungi and the protozoa. The latest organism incriminated—Noguchi's *Leptospira icteroides*—may or may not be the true causative agent in yellow fever, yet, long before, from analogy, believing it to be an endemic infective disease, the epidemiologist had sought, found and actually subjugated the all-important intermediate agent—the Ædes mosquito, which determines its peculiar localized distribution—and thereby has stayed the deadly scourge. The cleansing of Havana, the sanitary transformation of the Panama Canal Zone, are striking examples.

If it be true that the most diverse forms of irritation, animate and inanimate, may give rise to cancer, then surely the cancer germ must be some frequent and ubiquitous micro-organism, like the germ or germs which, in association with *Filaria bancrofti*, or some like factor, bring about those enormous tumour-like formations we call elephantiasis and pachydermia.

Failure hitherto to find the cancer germ may have been due to faulty technique or to extreme minuteness. Recently, indeed, Rous and his fellow-workers have found that chicken tumours of connective tissue type may be reproduced in healthy fowls of the same breed by the injection of cell-free extracts of these tumours.[1] In each case the neoplasm produced in the

[1] Also by means of the dry powder of cancer tissue.

inoculated fowl resembled the tumour from which the extract was made. This shows that the type of tumour is determined by the individuality of the causative agent and not alone by the potentialities of the stimulated cells. The work of Rous and others seems to show that particular neoplasms have specific causative agents which in some cases can be separated from the neoplasm. They appear to be living ultramicroscopic organisms able to pass through a Berkefeld filter impermeable to *Bacillus fluorescens liquefaciens*. Already several diseases have been shown to be due to so-called filtrable viruses. In most cases the unknown germ is transmitted by some intermediate host, such as mosquito, body-louse, or stable fly.

A Rebellion of the Cells.

But is it not the cancer cell itself which produces both primary and secondary tumours? Has the pathologist not proved that the cancer cell is nothing more nor less than one of the ordinary tissue cells which, suddenly having freed itself from the co-ordinating influences of the organism, starts on an independent career of lawless development, multiplying in a frenzied way and building huge, shapeless masses composed of disordered elements more or less resembling the cells of the tissue from which it sprang? "It is a mutiny or a rebellion of the cells," said the late Sir Jonathan Hutchinson. "A revolt against all cytological laws." The presence of the riotous cells is resented and resisted by the tissues among which they lie, exactly as if they were alien growths; but they are resistless, they displace the normal cells, penetrate into the spaces of the surrounding tissues, permeate the lymphatics and travel by way of the blood-vessels. Follow the onrush of the anarchistic cells and you shall see them move ruthlessly along, penetrating even bone and marrow, growing at the expense of the organism which, finally, they inevitably destroy.

As early as 1835 Johannes Müller looked upon the cancer cell as a kind of parasite, and, indeed, like parasites, the cancer cells become disseminated through the system, forming colonies wherever they find lodgment. But if the cancer cell is a tissue cell gone mad, what is it that changes so profoundly its behaviour? To my mind its hasty, disorderly, continuous proliferation suggests a minute living organism lodged within the cell. Nothing could account more plausibly for the remarkable, abnormal, proliferative activity which is the essential characteristic of the cells of malignant growths and the surest evidence of defence against attack. A striking analogy is exhibited by a disease of plants called Crown Gall. Here the growths which occur on root or shoot are made up of irregular masses of round or spindle-shaped cells of reduced size, which grow and divide just like those of animal tumours. By means of continuous chains of cells from the primary growths, secondary tumours are formed and these reproduce the structure of the tissues in which the primary growth developed. Thus we may find lignin in a leaf tumour, just as we may find bone out of place in malignant animal tumours. Owing to the work of Erwin Smith and others, we now know that crown gall is due to a definite organism, the *Bacterium tumefaciens*, which has answered to Koch's postulates. "This organism occurs only inside of certain of the proliferating cells. When the cell divides, the organism is carried over into the daughter cells, in at least a part of which it multiplies. It occurs in the cell in comparatively small numbers and, owing to the granular nature of the protoplasm, cannot be made out satisfactorily even with high powers of the microscope." As in the case of rapidly growing animal neoplasms, so also in crown gall,

the fully developed tumour falls a prey to saprophytic bacteria and fungi, becoming an open septic wound.

I am inclined to look upon the endocellular cancer germ as a dominant semi-symbiotic organism which can compel the invaded tissue-cell to act as a parasite. Changes of habit equally profound are witnessed in other cases of symbiosis; thus the Rhabdocœla are greatly modified by their symbiotic algæ. Convoluta, once a carnivorous worm, has developed marked heliotropic movements and now lives at the surface of tide-pools after the fashion of green plants. Symbiosis is widely spread throughout organic nature and many are the protozoa containing one-celled algæ or bacteria in their single-celled bodies. Who knows not the association of *Chlorella vulgaris* and *Hydra viridis*, of "yellow cells" (*zoöxanthellæ*) and radiolarians, of green or blue-green algæ and fungi, forming that large, striking group of compound plants called lichens?

The Only Way.

Wading through the vast literature of cancer, we find far more speculation than observation concerning its causation. Fantastic inventions innumerable we find, which have served no other purpose than that of leading us away from the direct path of biological research. Cancer students seem to have forgotten that the greatest discovery of modern science is the knowledge of the fact that definite plants and animals cause disease, while this knowledge, already in a number of cases, has supplied us with appropriate means of prevention.

I believe that the malignant neoplasms should be inscribed on the tablets of Parisitology. What evidence is there *against* their extrinsic infective nature? Like other infections, are they not local in their initial stages? Are they not favoured by local lesions and lowered resistance? Do they not gradually infiltrate the adjacent tissues? Do they not affect the proximate lymph nodes? Do they not become generally disseminated by way of the lymphatics and blood-vessels, leading finally to constitutional disturbances—the cancerous cachexia of old writers? Indeed, have we not found at times innumerable metastatic foci — a "miliary carcinosis" in every way similar to "miliary tuberculosis"? The surgeon, has he not seen cancer recur after removal, possibly himself having fouled the field of operation?[1] On the other hand, have we not seen tumours spontaneously retrograde? Are they not combated, limited, overcome by the resisting forces of the organism just like other infections? Why obstinately close our eyes to the strikingly unequal topographical distribution of the disease which points so strongly to a biological cause? Why ignore the teachings of its wide zoological distribution —a distribution so similar in extent of species to that of tuberculosis, trypanosomiasis, diphtheritis and plague?

My researches have taught me that, like malaria, sleeping sickness, relapsing fever, elephantiasis and plague, so also cancer has a complex natural history unsuspected or strangely ignored by the laboratory investigator. Its study is the work of the epidemiologist and the field naturalist. The question of cancer is a grave and urgent problem; its solution a task truly stupendous.

My investigations in Romagna lasted about five weeks, of which only three could be devoted to field work, and, of course, it was only towards the end of my stay that I was able to gather some idea of the conditions which might explain the great prevalence of cancer and its peculiar topographical distribution in this region. I did not undertake any kind of laboratory work; my business

[1] This is known as "post-operative cancer infection."

was to verify Dr. Ghetti's statements only and to see how far opportunities in the locality might favour a proper scientific investigation of the cancer problem. I found conditions most favourable, some of the hospitals, like that of Forli, comparable to our best, the laboratories good, though lacking perhaps in scientific apparatus, the Romagnese physicians well read, capable and keen, while the people were most willing to help in every way. I believe a scientific investigation carried out by competent men under the auspices of the Italian Government and with the aid of the Romagnese Medico-Chirurgical Society, would be of great use.

DISCUSSION.

Dr. GREENWOOD (Chairman) said that perhaps Dr. Sambon would tell the meeting what part of Italy he had been investigating, i.e., in what administrative Province it fell. Statistics were available for the fifteen compartments.

Dr. SAMBON replied that the district came into the Province of Ravenna, Department of Emilia. It was on the Adriatic coast, from the foot of the Apennines to the Adriatic Sea.

Dr. M. GREENWOOD said he hoped some Members would discuss this extremely interesting paper. He might himself, merely as a statistician, point out that he thought Dr. Sambon had done a little unintended injustice to Italy in some of the phrases he used, which might lead some people who were not acquainted with the statistics of the different countries to suppose that cancer was particularly prevalent in Italy. As a matter of fact, of the countries which compiled reliable statistics, Italy had the lowest death-rate from nearly all forms of cancer. In 1912 the death-rate of males in Italy per 100,000 living was 58, and of females 70·6. The corresponding rates in England and Wales was 91·6 and 112. Neither was it quite correct to say that the rate of mortality from cancer in Italy was increasing rapidly. The latest figures issued from the Statistical Department showed that, taking the years 1919-21, it was in the case of males an increase from 58·6 to 65. These figures exaggerated the increase, because the latest rates supplied were still based on the population of 1911, the figures of the latest census not being available, so the denominator was too small. The variations between the death-rates from cancer in the sixteen official departments of Italy were very considerable. The greatest death-rate, on the figures if 1919-21, was in the Departments of Tuscany and Emilia, in which Dr. Sambon's investigations were conducted. But the death-rate in Tuscany, the highest, was 105·7, for persons, 102·5 for males, 108·9 for females, the latter still appreciably below the death-rate in England and Wales for women. There were extreme variations in Italy, the lowest being in Sardinia. He had been informed that the reliability of the figures in some of the Southern areas was considerably less than in the more industrial parts, and that was somewhat supported by the fact that in the Southern portions the difference between the death-rate in the principal towns and that of the province as a whole was very much greater.

Another peculiarity was the following, and he did not know how Dr. Sambon proposed to explain it. He referred to the extraordinarily low incidence of cancer of the breast, and the relatively high incidence rate of cancer of the uterus. For example, in Holland, which had a very high cancer rate, i.e., cancer in the body generally, there was a peculiarly low rate of cancer mortality for cancer of breast and uterus. Cancer

of the breast in England and Wales, in 1914, was at the rate of 19·9, as against 5·9 in Italy; and cancer of the uterus 25 in England, compared with 15·1 in Italy. But in Holland, which had a higher rate from cancer of all sites than England and Wales, cancer of the uterus was 18·1, i.e., below the Italian rate, although cancer of the breast in Holland was twice as fatal as in Italy; those were the forms the certification of which was probably most reliable.

To most epidemiologists it seemed hopeless to attempt an epidemiological investigation of cancer *en masse*, owing to the unquestionable differences in the accuracy of the site rates. He thought the view most epidemiologists would take was, that it was more desirable first to investigate the extraordinary diversity of the incidence in particular sites; and it was exceedingly interesting and valuable that Dr. Sambon should take up these field inquiries, because at the moment an international inquiry was being carried out on the remarkable differences he had just pointed out between the rates of cancer of the reproductive organs in women of the three countries—England, Holland and Italy—and the Italian medical service was at present carrying out a very careful intensive inquiry into the subject of cancer. In the view of the Italian public health service, light was likely to be thrown on the subject by following up the history of patients admitted to the hospitals. Under the guidance of some of the leading clinicians of Rome, such an inquiry was now in progress Obviously, any biological investigations, such as Dr. Sambon had described, would dovetail into the inquiries of a statistical and clinical character; and so, although all might not share Dr. Sambon's intensity of conviction, all would, the speaker was sure, wish him success and the best of luck in what was a most interesting and important biological inquiry.

He hoped Dr. Sambon would not take it as a discourtesy that there had not been more discussion; it might be accepted as a compliment, for he had left nothing for Members to say.

Dr. DUDFIELD remarked that he thought there was a good deal that needed saying on this subject. What interested most members of the Section was the question of "cancer houses." Those who had been in the State Medical Service would know that this was a theory which was mooted twenty-five or thirty years ago, and, as far as this country was concerned, he thought the theory had been killed. Certainly, however, what Dr. Sambon had said that evening on the subject raised doubts; and Dr. Sambon went a step further, and the further point was worth looking into. If there were not "cancer houses," were there cancer areas or cancer blocks? That question could only be studied by making spot-maps. He had a record extending over twenty-five years, the address of every cancer case recorded in his district. When he could make time to do it, it would be easy to plot out on a district map all those cases. He would certainly do that, because it had been done with instructive results in the case of tuberculosis.

He had been interested also in what Dr. Sambon said about the use of the *scaldino*. It had been mentioned in many communications, but it was news to him (the speaker) that the use of the *scaldino* in Italy was on the same system as Afghan heaters, and that they did not get cancer.

As to the tar-painting experiments, he (Dr. Dudfield) did not know whether members would be able to go as far as Dr. Sambon did. On that point he was led to bring out an old hobby of his, namely, the question of the consumption of preserved foods. It was a fairly well established fact that one did not find cases of ulcer of the stomach and bowel in countries where the people did not consume foods which were preserved by chemical means, e.g., salicylic acid and boric acid; but such cases

were fairly common in this country. Dr. Newton Pitt, in his Presidential Address before the Section of Medicine, had pointed out that those diseases had greatly increased in frequency in the last forty years, and in the same forty years the use of chemically-preserved foods had also increased. One caution was necessary in making any deductions, because of the "*post hoc—propter hoc*" argument. He had very little doubt as to the increasing amount of cancer of the stomach. Was that due to the irritation produced on those parts by the ingestion of indigestible foods?

He was sure the Section was greatly indebted to Dr. Sambon for his paper, and if it had been possible to circulate it to the members beforehand, be believed there would have been a full discussion on it. Such a mass of material, however, needed to be studied and assimilated at leisure, and he hoped to digest it at a later date.

Dr. SAMBON briefly replied.

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Section of the History of Medicine.

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Assyrian Medical Texts.

By R. CAMPBELL THOMPSON, M.A., F.S.A.,

Fellow of Merton College, Oxford.

THE following translations are from the Assyrian cuneiform tablets from Kouyunjik published in my *Assyrian Medical Texts* (Milford, Oxford University Press, 1923). Transliterations in type have not been included, chiefly because of the difficulty of printing them. I propose to discuss the texts generally, after the translations have been concluded.[1]

[1] It will be seen that many of the vegetable drugs have been translated without comment, and for these the reader is referred to my *Assyrian Herbal* (Luzac & Co.), shortly to be published. I must here record my thanks to Professor S. Langdon for the ever-ready way in which he has put his library, as well as his notes for his forthcoming Sumerian Dictionary, at my disposal.

The abbreviations used herein are: *ADD.*, Johns, *Assyrian Deeds and Documents*; *AH.*, my *Assyrian Herbal*; *AJSL.*, *American Journal of Semitic Languages*; *AM.*, my *Assyrian Medical Texts*; *ASKT.*, Haupt, *Assyr.-Sumer. Keilschrifttexte*; Br., Brünnow, *List of Cuneiform Ideographs*; *BSGW.*, *Berichte ü. d. Verh. d. kgl. Sächs. Gesellsch. d. Wissenschaften*; *CT.*, *Cuneiform Texts from Babylonian Tablets*; Del., *HWB.*, Delitzsch, *Handwörterbuch*; Diosc., Dioscorides, ed. Sprengel; E., Ebeling, in *Archiv für Geschichte der Medizin*; *EB.*, *Encyclopædia Britannica*, 11th ed.; Holma, Holma, *Körperteile*; Hrozný, *Getr.*, Hrozný, *Das Getreide im alten Babylonien*; IB., Ibn Beithar, in Leclerc, *Notices des Manuscrits*, xxiii, xxv, xxvi; *KAR.*, Ebeling, *Keilschrifttexte aus Assur, Religiosen Inhalts*; *KB.*, Schrader, *Keilinschriftliche Bibliothek*; Kü., Küchler, *Beitr. z. K. d. Assyr.-Bab. Medizin* (I = K. 191, II = K. 71*b*, III = K. 61); MA., Muss-Arnolt, *Assyrian Dictionary*; *OLZ.*, *Orientalistische Literaturzeitung*; *P.*, Squire, *Companion to the British Pharmacopœia*, 18th ed., 1908; *PBE.*, *Babylonian Expedition of Pennsylvania*; *PC.*, *Penny Cyclopædia*; Pliny, Pliny, *Natural History* (ed. Bostock); *PSBA.*, *Proceedings of the Society of Biblical Archæology*; R., Rawlinson, *Cuneiform Inscriptions of Western Asia*; *RA.*, *Revue d'Assyriologie*; *SAI.*, Meissner, *Seltene Assyrische Ideogramme*; *SM.*, Budge, *Syriac Book of Medicines*; *ZA.*, *Zeitschrift für Assyriologie*; *ZK.*, *Zeitschrift für Keilschriftförschung*.

Numbers, such as 21, 1, 1, or *AM.* 21, 1, 1, refer to page and tablet-number and line in *AM.*

An asterisk * affixed to a drug means that there is a very slight doubt about the exact species, *or*, a slight variability possible, owing to there being a closely allied and almost interchangeable candidate for the same drug-name; **, some doubt about the identification, but good reason for it; a query (?), when there is real and justifiable doubt.

TRANSLATIONS.

A. Diseases of the Head.

No. 1.[1] *AM.* 1, 2 (K. 6684), *top broken.*

1. . . . thou shalt anoint his . . . , and . . .

2. . . . does not press[2] . . .

3. . . . [in ?] . . . , [which] hath not . . . (?), thou shalt wash him, and lice shall not come nigh.

4. . . . in cedar-oil thou shalt mix, anoint.

5. . . . ([a drug] full of sweet . . . , *murru* (= myrrh) is its name) . . . [in his nostrils ?] for his cleansing he shall blow, and it shall restore him.

7. . . . thou shalt bray[3] . . . *urṭû*-plant, put it in oil, anoint, (and) the lice cannot exist.

8.[4] [If a man]'s [head] is full of scabies and itch,[5] thou shalt bray sulphur,[6] mix it in cedar-oil, anoint him.

---

[1] The catch-line at the end of K. 163 (see *AM.* 1, 1), "If a man's head hurt him, his tongue prick him," shows that K. 163 precedes *AM.* 21, 2.

[2] Cf. 94, 9, 3 . . . *tu-kap-par tak-pir-ta i-sa-an-niḳ-šum-*[*ma*].

[3] RAT = *ta-pa*, or more correctly *ta-zak* (so E. xiii, 6, ingeniously, taking PA = ZAK from ID.PA = *a-za-ag*, Delitzsch, *Sum. Gloss.*, 15). As he says, the sense "pound" is found in *mazuktu* "mortar".

[4] Duplicate of *KAR.* 202, 11.

[5] *Ikkitu* and *rišutu*. The similarity of *SM.* i, 554, a Syriac receipt ܠܣܚܛ ܡܟܝܫܐ ܘܫܥܐ ܒܪܫܐ "for the scabies and lice (or itch) in the head", is most marked. *Ikkitu* thus = Syr. ܫܥܐ "scabies", and *rišutu* Syr. ܫܥܐ "lice (or itch)". (Cf. also 21, 1, 1, [. . . *u*]*naš u rišutum* . . . , and 26, 1, 6. 17; 30, 1, 5, 9.) The verb of the former occurs as *ukkak* (74, 1, 32, 34, q.v.); *iraššišumma* occurs 1, 2, 10 "if scab takes a man's head, it tickles him". Cf. also 74, 1, 32, 34, where a form of eczema breaks out on the leg, and *iraššašumma ukkak*. It is thus certain that this verb is referable to Syr. ܢܫܥ = *râšu*, although by its form it might be *rašâ*.

[6] KI.A.ID or *kibir* ID (*nâri*). Miss A. M. Lunn, B.Sc., made the correct suggestion to me that this is "sulphur". This tallies with the use of a remedy for scabies and itch in the head, sulphur being thus prescribed in *P.* 1178. It is therefore obvious that *kibir* ID or *kibir nâri* "bank of the river" (becoming *kibrîtu* in *AM.* 33, 1, 35, 36), was taken over into Arabic as كبريت, and Hebrew גָּפְרִית "certainly foreign word" (Briggs-Driver, *Dict.*). The Tigris has several sulphur springs near it, and there are sulphur mines eight miles from Mosul (Ainsworth, *Assyria*, 258), and doubtless the Assyrians collected it from deposits on the river banks.

In *AM.* the use is constantly external (for *šiggati*, 32, 5, 5; soles of the feet, 75, 1, iv, 25; eyes, 19, 6, 13, etc.), and in fumigations (33, 1, 9; 51, 4, 6; 91, 1 *r.* 1; 99, 3, 5, etc.). For saliva (31, 4, 17): Quantity prescribed, one shekel (53, 1, 6). Both "white" and "black" sulphur are quoted (2, 1, 15).

9. [If ditto], thou shalt heat [sulphu]r (and) let (it) cool [1] in the oil of his head; thou shalt heat *idra* [2] of salt (and) let (it) cool in the oil of his head.

---

10. [If a man]'s [head] gets a scab,[3] it tickles him, and on his removing

[1] *Tukaṣṣa*, Kü. 104. Cf. also "three times a day *lî tukaṣṣa* (thou shalt cool dough" (8, 1, 9; cf. 11, 2, 46); *[ina ta]mgubi tukaṣṣa* (11, 2, 22); *ina* A.GEŠTIN.NA *tuballal ḳaḳkad-su tukaṣṣa* (65, 5, 8); *ina tinuri tešikkir tušelamma tukaṣṣa* "in an oven thou shalt bake, take out, let cool" (43, 5, 10; 56, 5 *r.* 4); cf. the alternative šÊ (Br. 3060; 56, 1, 9; 64, 1, 19) *ina marḫaṣi taraḫaṣ(aṣ) tušelamma tukaṣṣa* "in a bath thou shalt wash, take out, let cool" (98, 3, 15). Cf. also 6, 1, 5; 21, 6, 8; 40, 1, 58; 42, 2, 5; 49, 6 *r.* 8; 61, 7, 6, 7; 84, 1, 3. Especially notice "heat these 21 plants in the man's urine and beer in an oven, take them out and *enuma* EN.TE.NA *ba-aḫ-ru-us-su . . . enuma rupšêᵖˡ tukaṣṣa*" (98, 3, 4). (Cf. 57, 10, 7.)

This brings us to *baḫru*. E. (xiii, 10) is probably right in suggesting that *ḫu* = *baḫ*. He quotes *ba-aḫ-ru-su* (K. 2418, 12, my *AM.* 77, 1) and *ḫu-ru-su* (K. 8049, *r.* i, 23, my *AM.* 41, 1), etc.; *ba-aḫ-ra* (37, 3, 3) and *ḫu-ra*, K. 10625, 4 (51, 4, 4). But I think he is wrong in translating it "cold".

Consider the passage from 98, 3, 4 above: "and as its *baḫrûti* grow(s) cold [thou shalt . . . and] when thou hast cooled off the *rupšê*," etc. Obviously "cool" or "cold" is not correct.

Consider, then, also, *ba-aḫ-ru-su ina* SU *tediri* (77, 1, 12) "spread its *baḫrûti* on a skin"; *ba-aḫ-ru-us-su ina* KU *te-s*[*ir*] "enclose its *baḫrûti* in a cloth" (73, 1, 10); *baḫ-ru-su ana* KU-*su tašapak*(*ak*) "pour its *baḫrûti* by his anus" (41, 1, iv, 23). We are thus dealing with a tangible substance which can hardly be considered equivalent to Arab. بخر "to steam".

But E. was on the right track with "cold" (although incorrect), in *baḫ-ru*(*ra*) *ikkal baḫ-ru*(*ra*) *išatti* (16, 4, 12; 27, 10, 5; 53, 10, 6), *šikara dišpa baḫra tušašti-šu* "beer (and) honey *baḫra* thou shalt give him to drink" (80, 1, 15); *baḫ-ra ikkal u išatti* "he shall eat and drink *baḫra*" (34, 1, 4); *baḫ-ra ikkaluᵖˡ baḫ-ra išattuᵖˡ baḫ-ra ina muḫ-ḫi-šu te-ḳi . . .* "he shall eat *baḫra*, he shall drink *baḫra*, *baḫra* to his skull thou shalt apply" (51, 4, 4). In every case the reference is to preceding drugs.

Clearly it is not "cold", but "warm" or "hot", just as appears in *ummare baḫrûti ša ina gurari bašl*[*u*] "hot pots heated in *gurari*" (IV *R.* 58, 41, *b*, Del. *HWB.* 170). *Baḫru*(*s*)*su*, parallel to *rupšê*, may then perhaps mean "its hot (parts)", i.e. while it is still hot, the connection with Arab. بخر "to steam" then being obvious. *Rupšê* (akin to *rupuštu*, some form of regurgitation in the mouth) will be Heb. *repheš* "mire", i.e. dregs, sediment.

Baḫru is used as an adjective to certain drugs: "*ina mê kasî šikruti baḫrute* NU *patan išatti* "he shall drink in water of roses baked, hot, without a meal" (80, 1, 11).

Baḫâru is used as a verb (*tubaḫar*, 4, 6, 3; 14, 4, 5; 20, 1, 6; 65, 5, 10; 76, 2, 4; 84, 4, iv, 11; *lubaḫir*, 80, 7, 9), where the meaning "heat" suits excellently.

Although, therefore, the root must be connected with Arab. بخر "to steam" of a pot, the meaning cannot be exactly "steam", "vapour", nor does "distillation" fit it. "Steaming hot" must be correct, as it could hardly be used otherwise in poultices.

[2] *Idra*. This word is the base of *idranu*, a form of salt. See the text in Pinches, *PSBA.* 1909, 64. Salt was used for itch-scabs (Pliny, xxxi, 45).

[3] *Samanu*, presumably "scab" from the description (cf. ll. 13, 17), is a disease of the head (MA. 766). ᵘ*Samanam* = "a drug for *samanu*" (41, 3, 6; cf. *CT.* xiv, 41, Rm. 362, 6, *šammu sa-ma-ni* ZI *arat-zu ina šamni . . .* "a drug to keep away *samanu*, its juice in oil . . .").

it it ceases (?), not increasing (?),[1] seed of *arnoglosson* . . . , dust (?) of *diki* (?) (of) caper, dust of sesame, dust of *millet, dried doves' dung from a palm of the mountains . . . in hot [2] attar of roses thou shalt knead, press [3] on his head, let cool, bind on.

13. . . . If a man's head gets a scab, [thou shalt anoint (?)] him with dust from a limestone threshold of a house in [oil ?]. When therein **balsam . . . ,[4] seed of *arnoglosson*, dust of sesame which the base (root) . . . , bitumen (?), UŠ [5] of *millet, doves' dung, *ḫaṣabti*, *erû*-stone, seed of AŠ-plant, these nine drugs thou shalt bray together [and] anoint [his head] in "blood" of cedar; these drugs apply [6] thereto, bind on, and he will recover.

The word occurs in *samanam ša igari* "scab of the house-wall" (65, 5, 21; rub and anoint, for *guraštu*, (scabies, itch), 17, 1, ii, 4; cf. 7, 3, 3), like the "leprosy" of Lev. xiv, 34.

Mr. N. V. Sidgwick, F.R.S., suggests to me that this "scab of the house-wall", used externally for itch, is the efflorescence which appears on stable-walls, etc., which may be calcium nitrate or potassium nitrate. The latter, Pliny says (xxxi, 46), is found in Media, called halmyrax, used externally in ointment. A synonym is possibly *ṭâbat* EME.*šal-lim*, which I think must be the nitrous efflorescence on the desert.

[1] Difficult. GAL.BI *zir ᵘlišan kalbi* occurs again, l. 14. Is it the same in both? There is no room for *ʿarganu* here; but the character following *i-na-aḫ* may belong to this latter group. Cf. *SAI.* 1483, 1489.

[2] Cf. *mê kasî im-mu-ti* (Langdon, *PBE.* xxxi, 67, 10).

[3] ŠAR. Besides the ordinary uses of "bind" there are several other uses of this ideogram in *AM.*: (1, *a*) Br. 4333, *ṣarâḫu*, from the complements *aḫ*, *iḫ*. It is used of *libbiᵘˡ*, ŠARᵘˡ (50, 4, 13), ŠAR.ŠAR (40, 5, 9), ŠAR.ŠAR . . . (44, 7, 2), ŠARᵘˡ-*ḫu* (22, 2, 4; 42, 2, 1; 49, 3, 4; 57, 3 *r.* 1), ŠAR.ŠAR-*ḫu* (21, 2, 6); of *rîš libbi* (43, 6, 3, cf. *u-ṣar-*[*riḫ*], 87, 1, 8); cf. *enuma* NA *rîš libbi-šu* BIL *libbi-šu* ŠAR.ŠAR (39, 1, 4), *enuma* NA *ṣi-ri-iḫ-ti libbi iši-ma* (ib. 40), and BIL *ṣi-ri-iḫ-tu ša lib ênâᴵᴵ-šu* (10, 1, iii, 3); *ana ṣi-ri-iḫ-ti* BIL *libbi* ZI-*ḫi* (39, 1, 34). On the other hand, *libbiᵘˡ-šu in-nap-pa-ḫu* (52, 9, 4). The Arab. صرخ "be inflamed" is the meaning here; *innappaḫu*, lit. "be blown", i.e. "kindled". (1, *b*) But ŠAR-*aḫ* undoubtedly means "thou shalt blow" in *AM.*; it refers to an operation of blowing drugs into eyes (12, 2, 3; 92, 8, 1), ears (36, 1, 7, 8, 13, 16, *r.* 5; 38, 1, 4, 7), penis (61, 1, 4), urinary organs (59, 1, 22, 23).

(2) = *Zarâbu* "to compress", "press on" drugs (of head, eyes, etc.); ŠAR-*ab* (4, 1, 6; 5, 1, 15, 19; 8, 1, 9; 12, 11, 2; 65, 5, 9, etc.); *ina pu-ti* GAB ŠAR-*ab* (5, 5, 8); cf. *rîš libbi šu u-za-rab-šu* (48, 2, 1); *u-zar-rab-šu* (45, 6, 5).

(3) = *Ḳatâru*, from variants; "to fumigate," . . . *uznâᴵᴵ-šu tu-ḳat-tar* (34, 2, 2); *ina* BIL *uznaᴵᴵ-šu* ŠAR (35, 1, 5, 7, duplicate of 33, 1, 28, *ina* BIL *uznâᴵᴵ-šu tu-ḳat-tar* (and ŠAR in the line following); *ina* BIL ŠAR-*šu* (64, 1, 20, 28; 80, 6, 6).

[4] Does the GAL.BI (see note 1 above) belong to this clause? "When the **balsam swells (?)" "When it increases in the balsam (?)" Doubtful.

[5] UŠ is apparently the correct reading.

[6] MAR, read *eḳû* on the following grounds: *eḳû* occurs as *te-ḳi* (10, 3, 18, 19, 20, 22; eyes, 12, 8, 10); [^{tak}A]Z.ḪAR *ina ḫimeti* RAT *te-ḳi* (eyes, 9, 1, 10); *ina lipî* GIR.PAD.DU.GID.DA RAT *te-iḳ-ḳi* (eyes, 14, 1, 4); . . . *dišpi ḫimeti* RAT *ênâᴵᴵ-šu te-iḳ-ḳi* (18, 9, 9); *te-te-ni-iḳ-ḳi* (eyes, 9, 1, 6); *te-te-niḳ-ḳ*[*i* ?] (18, 6, 3); KU-*šu te-te-ni-iḳ-ḳi* (anus, 101, 3, 9); *te-ḳi-it ênâᴵᴵ-šu ša ḳat edimmi* (16, 3, 4). *Eḳû* = *lapâtu* "touch", and *saḫâru* "surround" (MA. 89); *lupputu* is used for rubbing (see my *Devils*, ii, 79, 177). From the above quotations we see that it was used in conjunction with curd, marrow, or honey, and of the eyes or anus. An examination of the eye-texts shows the extreme probability of its varying with MAR, and as, moreover, *SAI.* 4122 shows . . . MAR = *eḳû*, the equivalence is certain.

17. [If a man's head] gets [sca]b, the hay (?)[1] which is on the water, the dust of a pig-stye, pigs' dung, saffron (?) . . . , powder[2] of thorn sprouting on his building, half a root of . . . , almond-[oil], juice of tamarisk, juice of *vitex agnus castus*, flour of *gu-gal*, flour of *gu-du*, flour of roast corn . . .

20. [If a man's head] gets [scab ?], the hay (?) which is on the water . . .

No. 2. *AM.* 1, 3 (K. 8346), *top broken.*

2. . . . to his head . . .

3. [If] there is . . . in the body of a man, . . . [soot ?] of an oven thereon thou shalt bind[3] . . .

5. . . . white *ditto*-stone, black *ditto*-stone thou shalt bray, mix in curd, anoint his skull . . .

6. . . . thou shalt take its blood, anoint his skull; thou shalt bray *tašmê* of the house-wall . . .

7. . . . thou shalt recite the charm E.NU.ŠUB[4] ***kinîb kinib kinib šuḫ kinib*** . . .

8. [If a man's . . .] throbbing (?) . . . takes, thou shalt bray fir-gum, roses, and salt together, in water his head [wash] . . .

9. . . . in *kurunnu*-beer thou shalt wash, set it under the stars; in the morning . . . , he shall eat and recover: flax thou shalt reduce, bray, in oil and beer anoint, the under part (?) of thorns . . .

11. . . . and rose-perfume thou shalt bind: seed of fennel in oil thou shalt bray, his head [anoint] . . .: . . . thou shalt bind: *lolium* in beer let him drink, thou shalt put gum of *galbanum in his mouth, with water

[1] If *alapû* = *ᵘalapû*, it may be hay (*AH.* § 10, BY). But the lack of the determinative is curious; *alapâ* (again without determinative) is used 17, 1, 2. See *AH.* § 10, C.

[2] KU.KU = "powder"? It is used of *ᵗᵃᵏ*AZ.ḪAR (arsenic, 19, 6, 12); of *ᵗᵃᵏ*AŠ.GE.GE (19, 1, 6); of KA.AM.SI (ivory, 40, 5, 6); of trees (the powdered, dry bark, e.g. *telammaku*, 5, 5, 7; 40, 5, 6; *ᵗ*KU, 5, 5, 6; *ᵗkal marḫi*, 5, 5, 7).

[3] *Taṣaru*(*m*) (the form *taṣaru* occurring 75, 1, 26, 27, 28) would appear to be from Heb. צור "bind", "swathe", in spite of its form. Its use is almost the same as *ṣamâdu*; "four days *taṣaru*" (75, 1, 26); "seven days *taṣarum*" (44, 1, iv, 6); various drugs *tazak šamni tapašaš taṣarum* (61, 7, 1); *tazak ana pani* ŠUR *taṣarum* (39, 1, 5; cf. 72, 2, 12); *tabašal taṣarum* (41, 1, iv, 6); also 26, 2, 7; 42, 3, 12; 75, 1, iv, 21; probably 27, 5 *r.* 6.

[4] E.NU.ŠUB, constantly, as is well known, heading incantations. See Langdon, *PBE.* xxxi, 70, translating it "house not purified", and further *Journ. Soc. Or. Res.*, v, 2, p. 81. Cf. 65, 5, 23

of dates . . . : . . . [without] a meal let him drink: oil and beer he shall drink (in quantity) and in (?) ½ *ḳa* of beer-lees, ½ *ḳa* of . . . [from ab]ove downwards thou shalt massage,[1] bind on for ten days. Licorice-root, . . . , fennel in oil thou shalt bray, his head and all his members [thou shalt bind] . . .

16. [If a man]'s jaws burn, his head ditto (?) his face . . . him . . .

No. 3. *AM.* 1, 4 (K. 2615), *top broken.*

2. . . . [in] pressed [grap]es thou shalt knead, on a . . . skin spread . . .

3. . . . *lycium*, right horn of an ox, [left ?] horn of a . . . (?) . . . , *cannabis*, seven drugs . . . and thou shalt bind on his temples.

6–20 (*right half of lines containing a Sumerian incantation*).

21. [Charm against] the Hand of a Ghost.

22. [Ritual for this: a . . . thread] thou shalt spin, thou shalt pierce (and)[2] thread thereon seven male *šû*-stones[3] . . . , *crataegus azarolus* (?), in seven folds thou shalt fold it, seven and seven [knots thou shalt tie]; when thou tiest it thou shalt recite the charm, the folds . . .

No. 4. *AM.* 2, 1 (K. 2491 + 8356), *obverse, top broken.*[4]

1. Ritual for this (?): *ḫallutanâ* (?) of a GIR.ŠAL (?) . . . [therein thou shalt put *ḫipîti*]. The charm seven times thou shalt recite: in a cloth (?) thou shalt bray (?) . . . [seven knots thou shalt tie; while thou tiest] recite the charm, on his temples [thou shalt bind and he shall recover].

4. If a man's head contain water, the upper part foetor . . . [thou shalt set a . . . , and it will remove his water].

5. If a man's forehead[5] contain water . . . [thou shalt bind for seven days]; on the eighth day his forehead three times thou shalt . . . [bind].

[1] *Tu-maš-še* (?), or *tu-maš-šad* (?).

[2] KAK = *zaḳâpu*, Br. 5270; cf. *zikpu* point of a dagger.

[3] *tal-šû*. Langdon, *PBE.* xxxi, 63, 7, thinks it is "coral" comparing Ninurta Epic, *ASKT.* 81, 23, but doubtful. It occurs in *AM.* 102, 1, 22; 104, 1, 9.

[4] If my suggestion that this is duplicate of *CT.* xxiii, 37, 2 ff., and 24, 20 ff., be right, this is one of the series *Enuma* NA *muḫḫu-šu išâta ukal*, and obv. and rev. must be transposed.

[5] *Apputtu*, pointed out by Holma (18) = Syr. *appûthâ* "forehead", used antithetically to *zibbatu* "tail", *ZA.* xvi, 174, 11.

7. If a man's head contain liquid[1] . . . [thou shalt put].

8. If a man's head smells unpleasantly,[2] [*mušgarru*-stone, DAG.GAZ-stone, cinnabar, *marḫaši*-stone] KA.MI-stone, *ḫulalu*-stone, AN.ZA.[KAN]-stone [AMAŠ.PA.E-stone] these [eight "stones"] in a scarlet thread and a white thread thou shalt spin [on his temples bind, and he shall recover].

11. If a man's head . . . , fir-gum, [pine-gum, *suadu*, box (?), **ferula communis*] . . . *juniperus oxycedrus, artemisia*,[3] **ba[lsam, **sagapenum in ditto (??) thou shalt mix (and bind on)].

13. [If ditto] . . . in the suet of the kidney of a male sheep which has not been put in salt . . . [thou shalt bray in cedar-oil, spread on a skin, bind on his head].

14. [If ditto] . . . *galbanum, turmeric . . .

15. . . . head, white and black sulphur, *liquidambar male and female, *muzu*, . . . salicornia-alkali (?) . . . , human excrement,[4] these drugs together [thou shalt mix and apply to] his temples.

18. . . . head, *hyoscyamus*, sulphur, turmeric, . . . thou shalt crush, in "blood" of cedar thou shalt mix, on the fire [heat and apply].

20. [If a man]'s [head] . . . is sick, thou shalt bray *ṣaṣumtu, murdudû*, . . MU.UN.ṢIR (?) together, knead in rose-water, press on his head [bind, and for three days not take off].

22. [If a man's] . . . gives trouble, thou shalt mix[5] fir-gum, pine-gum, *hyoscyamus*, myrrh, gum of *galbanum . . . ox- . . . , spread (them) on a skin, press on, [bind on for three days].

[1] *Šeḫa*, Syr. *š'ḥâ* liquefactus est.

[2] *Ittenibaššum[ma]* from *ba'âšu*, not to be confused with "If a man's stomach *it-te-nit-ba-aš-šum*" (Kü. i, i, 19).

[3] There are two *siḫu*, one "white pine" (for which *ᵗišiḫu* is the more correct form), the other *ʿ* or *ᵛsiḫu*, *Artemisia* (*AH.* § 10, BA).

[4] *Gulgul*, common with *ameluti* in these texts. I thought at first that it was Heb. גֻּלְגֹּלֶת "skull", but obviously it is far more probably a collateral with Heb. גֵּלֶל "dung". Cf. *CT.* xxiii, 36, 58 [*gul-g*]*ul-li-šu it-tar-du enuma(ma) gul-gul-la-šu te-sir* KIL.A *ša gul-gul-li-šu* . . . In *SM.* ii, 662, human excrement is used to smear on eyes.

[5] But cf. *KAR.* 202, ii, 43.

25. . . . right and left three each . . .

Reverse, top broken.[1]

3. . . . [thou shalt pound (and) str]ain, in rose-water knead, [press, bind on, and not take off for three days ?].

4. . . . in beer-lees thou shalt boil, knead, [press, bind on, and ditto ?].

5. . . . their sweetness (?) thou shalt remove, in [mountain-honey, verdigris (?) of gold thou shalt bray, apply ?].

6. . . . thou shalt pound (and) strain, in rose-water knead, press, [bind on and not take off for three days ?].

7. . . . thou shalt pound (and) strain, in rose-water knead, press, [bind on and not take off for three days ?].

8. Thou shalt bray cantharides (?)[2] in honey, [apply ?].

9. ⅓ *ḳa* juice of *ma-[er]iš-ma-ra*[3]-plant thou shalt pound (and) strain, in rose-water [knead, press, bind on and not take off for three days ?].

10. Calcined lime in almond-oil thou shalt knead, press [bind on and not take off for three days ?].

11. LA *nigula* which has a *kibšam* (?), in oil (and) [copper]-dust[4] [thou shalt bray, apply ?].

[1] Is this a duplicate of *CT.* xxiii, 24, 20–25, 35 ?

[2] ŠAM *ša ib-ḫu*, in *AM.* about 30 times. Kû. iii, i, 24, ^{u}ŠAM *ša ib-ḫu* ^{u}ṢI to drink, in a series of single drugs, each followed by ^{u}ṢI, to be drunk for bile. Drunk alone as ecbolic in labour (67, 1, 16); is gloss to ^{u}KUR.KUR, *hyoscyamus* (19, 5, 3); once written *ša ib* ŠAM *ḫu* (57, 5, 13). Quantities, 2 *bur* (49, 6 *r.* 2); 1 shekel (6, 3, 7; 8, 1, 27; 13, 7, 4, 5); 3 shekels (9, 1, 16); 5 shekels (11, 2, 20). Used externally constantly with others, for eyes (8, 1, 19, 27, 3); 9, 1, 13, 16; 13, 7, 4, 5; 16, 1, 8, 23, 26; 17, 6, 5); wash head (64, 1, 37, with *storax, "Akkadian salt," **ammi . . .); for *kiṣirti* of lungs (49, 6 *r.* 2); apply anus (57, 5, 13, with mandrake only). Sayce (*ZK.* ii, 207) pointed out the word *ib-ḫu* as a worm in *VR.* 27, 24 (= *CT.* xiv, 8, *r.* 24), so that there is great probability of it meaning a drug from some lower order of animals. The question arises, can it be Cantharides ? The properties of the Cantharis beetle, sometimes confused with *buprestis* (Dioscorides ii, 145, 146), have long been known; but it is a very powerful drug, and it may be too strong for the identification necessary. At the same time, although a powerful vesicant, it is given internally with caution, as a diuretic and aphrodisiac; and its effect on the renal region produces abortion, which may explain ŠAM *ša ibḫu* as an ecbolic (*P.* 317; *EB.* v, 213). In *SM.* ii, 100, note especially that the juice of beetles is prescribed as *kohl* for eyes.

[3] A most pungent plant; see *AH. sub voce.*

[4] Cf. *CT.* xxiii, 26, 3.

12. *Juniperus excelsa* (and) *lolium* thou shalt pound (and) strain, in lees of *kurunnu*-beer [thou shalt knead, press, bind on and not take off for three days ?].

13. Mustard (and) *barḫuš* thou shalt pound (and) strain, in lees of *kurunnu*-beer [thou shalt knead, press, bind on and ditto ?].

14. Pine-gum, fir-gum, *galbanum, *lolium*, salicornia-alkali [thou shalt bray, apply ?].

15. Copper-dust [in . . . thou shalt bray, knead, press, bind on and not take off for three days ?].

16. . . . *lolium*, ⅓ *ḳa* of *usa*-beer . . . [thou shalt knead, press, bind on and not take off for three days ?].

No. 5. *AM.* 2, 3 (K. 13502), *top broken.*

2. . . . sulphur, *nigella* . . .

3. . . . thou shalt bray soot (= sal ammoniac)[1] . . .

[1] IM.KAL. It is better to discuss the longer group IM.KAL.GUG first. IM.KAL, meaning literally "strong wind", must signify "vapour" or something vaporized. GUG = *sâmtu, sându*, most noticeable in $^{ta\check{s}}$GUG, i.e. a red stone.

Now one of the most obvious "red stones" is the cinnabar, which gives vermilion paint. It must be different from $^{ta\check{s}}$AN.GUG.ME, following "gold, silver, copper", perhaps cornelian used in decorating the palace (Sennacherib, King, *CT.* xxvi, 23, 14).

But the "vapour of cinnabar" is mercury, well known to the ancients (IB. No. 1143 says mercury came from Azerbaijan). Cf. Pliny (xxxiii, 37 ff.), who says that minium (our cinnabar, according to Bostock), which is found in silver mines, comes from Carmania. He describes the method of obtaining hydrargyros from the "inferior" minium, either by pounding minium and vinegar, or else by putting minium into flat earthen pans covered with a lid, and then enclosed in an iron seething-pot, well luted with potter's clay. A fire is then lighted under the pans, and the flame kept continually burning by the aid of bellows; which done, the steam that is found adhering to the lid is carefully removed, being like silver in colour, and similar to water in its fluidity. He notes that minium is a poison.

Now IM.KAL.GUG is used: (*a*) in fumigating ears (33, 1, 31; 34, 5, 7; 35, 1, 7; 38, 2 *r.* 10) (cinnabar is used in application to ears in *SM.* ii, 114, 115); (*b*) brayed and applied, [eyes] (11, 2, 12) (cinnabar is used for eyes in *SM.* ii, 98); (*c*) as ecbolic in difficult labour, to be brayed and drunk in beer (67, 1, iv, 22). We may, therefore, see "mercury" in IM.KAL.GUG with reasonable probability.

Sântu, sându, the name for the "red stone", may have an echo in the Syr. *šânâ*, Pers. *šâdnaḥ*, Arab. شادنة haematites (Lane, *Dict. sub voce*), and still more probably in σάνδυξ "a bright red colour" (with -ξ termination, like so many words ὀπόπαναξ, στύραξ, etc.). Leclerc's note to IB. No. 1132 gives sandyx = minium.

What, then, is IM.KAL in our present text? Obviously it cannot be "vapour", since it is brayed. On my asking Mr. N. V. Sidgwick for his opinion about it, on the analogy of

4. If a man's head [has] an itching[1] . . . **ammi, *lolium*, . . . [in oil of] cedar thou shalt anoint.

No. 6. *AM.* 3, 2 (K. 2416 + 9224 + D.T. 215, with the addition of + K. 16402, pl. 46), *obverse, top broken.*

1. (+ K. 16402, ii, 1–3) [Ritu]al (?) for this. Her *mušadu*[2] in . . . Her *mušadu* thou shalt roll, into . . . thou shalt put; the charm seven times thou shalt recite . . . *šapilti* (?) of the *mušadu* . . .: a thread thou shalt spin, seven and seven knots [thou shalt tie: as thou tiest thou shalt recite the charm].

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3. (+ K. 16402, ii, 4–6) (*Sumerian charm and formula*). Recite the charm.

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6. Incantation: When the hair of a woman grows weak.[3]

" mercury " above, and suggesting that it might be some form of solidified vapour, he instanced the ancient method of obtaining sal ammoniac from the soot of camels' dung. The common fuel of Mesopotamia is, as is well known to anyone who has been there, dried cow-dung, or camel-dung; wood fuel is scarce and difficult to obtain on the flats. The soot in Egypt is carefully collected and sold to the sal ammoniac makers (*EB.* xxiv, 59).

IM.KAL = *aklu* (*SAI.* 6309), IM.KAL(AG).GA = *aštu, išikku, dannu, sušikku* (*SAI.* 6311–6314). It is used in *AM.* alone for head here; and (to be drunk), 26, 4, 7, duplicate of 64, 1, 11.

IM.KAL.LA (Kū. iii, iii, 13), brayed alone and drunk in cedar-oil and beer (cf. 27, 2, 16; 36, 1 *r.* 2), must be the " soot of LA " rather than IM.KAL with a complement.

[1] *Kuraru. CT.* xiv, 36, 81–2–4, 267, obv. 12 ff., is a list of drugs (the names now lost) called " *ᵘša-mi ku-ra-ri* ZI (= *nasaḫi*) " and " *ᵘša-mi ku-ra-aš-ti* ZI " " for taking away *kuraru, kuraštu* " (the same words, cf. *Urartu, Uraštu*). The treatment for *kuraru* of the head is to shave the head, anoint with fish oil (NI.KIL), etc. (5, 5, 10), the particular point to notice being that urine (ammonia) is to be plentifully used. Another long treatment is given in 65, 5, 9, washing the head being part. In 65, 5, 20 *samanam ša igari* (probably saltpetre) is applied (see p. 3, n. 3); and in 17, 1, ii, 1–6, among the drugs given for " when a man is full of *guraštu* " are fennel, **ricinus*, sumach, *samanam ša igari*, and *storax. In the same text ammonia (2, 3, 3) and sulphur (l. 2) at once indicate an itch; so also " if his body is full of itch ". " Baldness " is practically an impossibility, therefore; besides, it would hardly be good Assyrian to talk about " removing " baldness.

The text is similar to *KAR.* 202, i, 49, *Enuma* NA *ḳaḳḳadu ku-ra-ra . . . -šu* TUK *ultu* (?) GIG . . . *tâbtu uḫulu ḳarnanu ᵘšamranu ᵘninû saḫ-li-e šinat* (?) *imeri* (?) A (?).GEŠTIN.NA DAN.GA (A ?).GEŠTIN BIL.LAL *tar-bak taṣamid(id)*, etc.

[2] *Mušadu.* Cf. 43, 1, ii, 3: " if ditto, *ubani-ka mušadi* NIGIN *dišpi* SUD . . . , i.e. thou shalt wind a *mušadu* on thy finger, sprinkle honey . . ." *KAR.* 202, ii, 24, duplicate of l. 7: " if ditto, *mušadi-šu* TI-*ḳi*. It is perhaps " hair-ribbon ", " fillet ".

DUM.KID, as in DUM.KID.ŠE = *ša šapilti ša še-im.* It occurs alone, DUM.KID *ina išati tuṣaḫḫar(ar)*, 95, 2, iii, 6, and again 99, 3 *r.* 5.

[3] *Išaḫḫuḫ*, Syr. ܐܫܚ " grows thin, weak ". Cf. *KAR.* 202, ii, 27 (cf. *CT.* xxiii, 32, 8) " when a man *ud-da-at išû šarat ḳaḳḳadi-šu išaḫḫuḫ* "; 95, 3, 17; *išaḫuḫa*, 30, 12, 1. Cf. also *CT.* xxiii, 34, 22; 35, 48. Jastrow (*Trans. Coll. Phys. Philad.*, 1913, 379) " bent down ", not recognizing that it refers to the hair (his text being *šarat takab ḳaḳḳadi*).

7. (*Duplicate of KAR.* 202, ii, 24.) Ritual for this: thou shalt take her *mušadu*, plait[1] a cord,[2] *urṭê*[3] [of ?] a palm of the north[4] thou shalt spin; hair[5] of a white horse with (?) seven and seven knots thou shalt tie, in her hair thou shalt bind; the charm seven times thou shalt recite . . . for three days the back of her neck will hurt her; until her hair stops[6] thou shalt not take (it) off.

10. (ll. 10–12 charm.)

13. . . . by the seven times knotting may she be holy, . . . may she be pure! Let evil tongue be absent from her!

15. [Incantation]: to preserve[7] the hair of a woman's head.

16. Spin [a thread . . .], a PA-stone of its seven colours, *ianiba*-stone, a meteorite,[8] stone (?) of (?) Gutium,[9] copper, bronze . . . [thereon thou shalt thread]: seven and seven knots thou shalt tie: as thou tiest (them) thou shalt recite the charm; [bind them on her hair] and the falling[10] hair shall be stopped.

(*Rest mutilated.*)

[1] *Taṣapar*, Arab. صَفَرَ. Cf. 16, 5, ii, 6; 25, 6, ii, 10; 28, 3, 7; 35, 1, 12; 90, 2 *r.* 5.

[2] KU (= *lubuštu*) *man-tu*. Ebeling's text is correct (*KAR.* 202, ii); I misread *man* as *kur*, owing to some clay in the sign. Perhaps Syr. ܣܥܪܐ hair, or a cord.

[3] *Urṭê*, probably *ᵘurṭû*, the plant, which is spelt without the determinative also on 94, 2, ii, 16.

[4] Ebeling's text, *KAR.* 202, ii, 24, has no KU before *ᵍišimmari*, and ŠI before *iltanu*. The "palm of the north" does not bear fruit and is, of course, far less valuable than those which grow south of Tuz Khurmati. The fibres of the palm used for rope-making are taken from the integument which covers the tree between the boughs (Rhind, *Vegetable Kingdom*, 255).

[5] *Zappi* must be the Syr. *zappê* "hair"; 33, 1, 18; 38, 2 *r.* 2; of *imᵉʳsisi buḫalitu* "stallion" (as in the duplicate for our passage, *KAR.* 202, ii), 99, 3 *r.* 9. Especially "*zappu* of an ass of the right, *zappu* of an ass of the left, [*zappu*] of a *imᵉʳbakkarrû*, *zappu* of a white pig" (Myhrman, *ZA.* xvi, 177). *Zab-bi šaḫî*, 5, 3, ii, 8.

[6] For DU.DU-*za* of the hair, cf. *CT.* xxiii, 28, 26.

[7] *Kalî*, like *ikkalla*, l. 19, "keep, retain." Cf. *CT.* xxiii, 35, 39, and for the grammatical construction, *CT.* xiv, 36 *passim*.

[8] *tabziḳtu*, Syr. ܐܣܛܠܝܢ fulmen.

[9] *Gutitu*, cf. Br. 11148.

[10] *CT.* xxiii, 34, 31 and 35, 39 have *šartu* DU-*tu*.

AM. 4, 1 (K. 2416), *reverse, top broken.*

(*To turn grey hair black.*)

3. . . . gall of a black ox (*v.* gall of a snake), gall of a scorpion, gall of a pig, *punpul*[*la*] . . . , *suadu*, thou shalt reduce, bray; these five drugs in equal parts [thou shalt mix], . . . [which have been] buried (?) take up and together mix [1]; in the oil of a cypress of the cemetery . . . press on his [head], seven days anoint, and the grey hair [will turn black].

---

7. (Cf. 5, 1, 5.) If a man's head in his youth is full of grey hairs, to darken the grey hair [2] . . . into oil thou shalt put until they die [3] . . . bray, in oil of the cypress of a cemetery thou shalt mix, anoint . . . one hundred days thou shalt anoint . . . , the charm seven times thou shalt recite . . . , thou shalt pound, therein refined oil . . . [under] the stars thou shalt set it, (on) his head press it, bind on for seven days and he shall recover.

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14. (Cf. 5, 1, 13.) [If a man's head] . . . [thou shalt take] the head of a black raven,[4] the head of a stork [5] . . . these [drugs] thou shalt mix in oil, anoint his head . . . , . . . of his urine (?) thou shalt take, pine-gum . . . together thou shalt bray, in almond-oil thou shalt mix, put . . . and his hair shall grow.

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19. (*Three lines of charm, mutilated.*)

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22. [Incantation: When] the hair of the head is not healthy.

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23. . . . in *alappanu*-beer thou shalt put, heat in an oven, wash his head . . . leek-seed, *ḳilḳil*-seed [6] together thou shalt bray . . . , mix, anoint him, and there shall be no grey hair.

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26. . . . *nigella, hyoscyamus*, seed of poppy . . . [*biṣṣur* ?]-*atani*, these six drugs thou shalt dry, bray, . . . on her skull . . .

---

[1] *Tuḫasa*, Arab. حاس "mix", but it must be in a peculiar way. "The leaf of *solanum tuḫasa*" (15, 6, 5); "garlic (?) *tuḫasa*, and pour into his ears" (36, 1, 11); "the leaf of tamarisk *tuḫasa*" (9, 1, 32). Cf. 14. 3, 7 and 25, 6, ii, 13, where it varies with *RAT*.

[2] *Ana šarti piṣîti ṣullumu*, like *ᵘšа-mi dâmi parasi*(*si*) "drug for stopping blood" (*CT.* xiv, 36, 79–8, 22. 3), etc.

[3] Are we to supply here "bees", as in *SM.* ii, 691?

[4] Not "locust"; the "black" is enough to determine this. "The fat of a black raven" is used for the same purpose in *SM.* ii, 691. For these drugs for hair cf. *CT.* xxiii, 35, 38.

[5] RAḲ.RAḲ.ḪU must be the *raḳ-raḳ-ḳu* = *la-ḳa-la-ḳa* of *CT.* xiv, 6 *r.* 8.

[6] Possibly cassia, but more probably another plant having a black seed; see *AH.* § 10 AG.

No. 7 (*AM.* 3, 3, K. 10323 is a duplicate of IV R. 56, ii, 21 ff., the *Labartu*-series).

No. 8. *AM.* 3, 5 (K. 6224), *obverse, top broken.*

2. . . . with cedar-[oil] thou shalt anoint, the milk of poppies thou shalt reduce . . .

3. [If . . .] fire, thou shalt put . . . to his head. If [ditto] . . .

4. [If . . .] thou shalt mix [sulph]ur and . . . and anoint. If ditto, thou shalt mix sulphur in mountain-honey [and anoint].

5. . . . thou shalt bray **bellis*, anoint in oil. If ditto, thou shalt bray ***ricinus*, mix in fat, anoint. If ditto, thou shalt bray PI.PI-fennel, in oil anoint.

6. . . . thou shalt beat up fennel in cows' milk or cows' urine, wash his head (therewith), reduce *barḫuš*, bray (it), press it on his head, anoint with oil . . .

7. . . . poppy (?), opium-stick, thou shalt reduce, bray, press on his head, anoint with oil, bind on, (or), apply, bind on.

8. . . . thou shalt bray, mix in cedar-oil, and hair will grow.

9. . . . thou shalt bray, anoint his head, bray *gu-gal* flour, *gu-du* flour, chamomile, (and) knead them in rose-water, bind on: with ***ricinus* (and) alkali in hot water wash his head.

No. 9. *AM.* 3, 6 (K. 10549), mutilated, Col. ii, 4, " When the hair of the head is white," and 7, " That grey hairs should not be." For *tetemir* cf. *tetimmir*, 11, 2, 31.

No. 10. *AM.* 3, 7 (K. 4083). Cf. K. 6087 (61, 8).

No. 11. *AM.* 4, 3 (S. 88), catchline " . . . on his bed with him ".

No. 12. *AM.* 4, 2 (K. 9173), *top broken.*

2. . . . on his mouth thou shalt pour[1] . . .

3. . . . fennel-seed, lupin, **calendula*, . . . in the morning his head thou shalt wash [therewith, and he shall recover].

5. . . . poppy, dried roses in cedar-oil [thou shalt mix, and apply].

[1] Is there a parallel in *SM.* ii, 691, to prevent hair becoming grey " take the gall of a swallow, and pour two drops on the right side of his mouth, and one on the left "?

6. . . . in fire,[1] to his head [thou shalt put].

7. . . . thou shalt mix, cool, in cedar-oil (*v.* in curd) [thou shalt anoint].

8. . . . his head thou shalt wash, opium-stick . . .

9. . . . thrown away . . . (?) and his head . . . a frog[2] in curd thou shalt mix, anoint, [and he shall recover].

11. . . . [in] strong wine thou shalt mix, his head [bind].

. . . . . .

No. 13. *AM.* 4, 6 (K. 8160), *top broken.*

1. . . . hair of a *muḫ-tul-bi*,[3] . . . , *ulap kibti*,[4] . . .

2. . . . fir-gum, sumach, *liquidambar, kelp (?) . . . thou shalt heat and in a bandage on his temples thou shalt bind (them) [and he shall recover].

4. . . . ** meerschaum,[5] sulphur, kelp (?), *muza*-stone together [thou shalt bray, in] cedar-oil mix, in scarlet wool enclose, bind on his temples.

[1] Cf. *AM.* 3, 5, 3.

[2] NE.ZA.ZA must mean "frog". Without an adjective, 12, 2, 5; 30, 8, 8, 9; 30, 13, 7, 8; 36, 1, 4, 16; "yellow" 8, 1, 13 (its gall); 37, 10, 6; 87, 1, 2; 88, 2, 16; 94, 3 *r.* 4; 103, 1, 6. Cf. *SM.* ii, 663, for eyes, the blood of a yellow frog; 703, the same drug prevents hair growing. The tidal pools round Basrah are full of young frogs growing from tadpoles in mid-June. Cf. *KAR.* 204, 15, *Enuma* ditto, NE.ZA.ZA *ša lib*^{ab}*erî nâri i-ba-* . . . See Jastrow, *PRSM.* vii, 163.

[3] MUḪ.TUL.BI: an animal, Boissier, *Div.* 29, 6; *Doc. Ass.* 111, 6, etc.; Virolleaud, *Bab.* 1, 26. Its hair used (19, 2, ii, 4); in almond-oil anoint (96, 4, 9); its bone, in fumigation for ears (33, 1, 33).

[4] KU.NIK.IB.ŠU.LAL, apparently made up of two groups, KU.NIK.IB = *ulapu*, and ŠU.LAL = *kibtu* "wheat" (Hrozný, *Getr.* 62; cf. *AM.* 51, 10, 5). Bray with **ammi, sesame, human bones, sulphur, etc., as unguent (19, 2, ii, 7); cf. 29, 1, 3 (unguent, with 3 others); bind alone on temples, poultice, 20, 1, 34. Cf. 63, 2, 5; 94, 2, ii, 15. *Fumigate*, with various drugs, alkali, sulphur, bitumen, human bones, etc., 93, 1, 12; ears, with seed of tamarisk, *liquidambar, etc. (35, 1, 5; 38, 2 *r.* 2). Cf. 33, 1, 29, 30, 35; 34, 6, 6; 80, 6, 3; 99, 3, 18 *r.* 9, 12, 18; 103, 1, 6. (On *ṭiṭ ulapi* "mortar", see Thureau-Dangin, *RA*, 1914, 87.)

[5] I take *ru'ut nâri* "spittle of the river" to be the same as *meerschaum* = magnesite, or magnesium silicate. It must be some definite earth, just as *kibir nâri* is sulphur. Meerschaum is said to be used as fuller's earth in the Turkish Dominions (*PC.* xv, 63). The suggestion in Bostock, Pliny (xxxv, 53 ff.) is that silicates were more used in medicine in ancient times. Of the earths in Pliny, Chian earth is a cosmetic, Cimolite is an aluminous silicate "a white chalk dissolving in water", there being two kinds of Cimolian earth (Pliny, xxxv, 57), used externally for tumours, etc.

In *CT.* xiv, 9, K. 4373, *r.* v–vi, 4–7, it is included in the same group as sulphur and *ḫâpu* (twice). Does *ḫâpu* = Syr. ܚܦܦ "wash", with reference to fuller's earth?

6. . . . fruit of the "sea-tree", *muza*-stone, *ṣab*-stone, * liquidambar male [and female], kelp (?), cinnabar of Egypt, together in a skin- . . . on his neck thou shalt put, and [he shall recover].

8. The left horn of a kid which has been covered (?) [1] thou shalt reduce: the horn of a ram thou shalt reduce: *arušte*-powder [2] thou shalt . . . , . . . instead of powder (?), ** ammi, alum, caper, seed of tamarisk, . . . [* liquidambar] male and female, kelp (?), laurel-berries, lupin, * *calendula*, *imḫur*-[*ašrâ*] . . . , sumach, male mandrake-root, *muza*-stone . . . [the]se [drugs] together thou shalt bray, mix in oil, [apply, and he shall recover].

No. 14. *AM.* 4, 7 (81-7-27, 61), *top broken.*

1. . . . thou shalt continue the treatment, and he shall recover.

2. . . . that man "seeketh the Temple of Shamash" [3] and . . .; . . . pine-gum (?) thou shalt bray, wrap in a fleece, put it on his stomach . . . in spelt-bread let him eat . . . thou shalt continue the treatment and he shall recover.

7.[4] . . . [that man] "seeketh [the Temple of Shamash (or Sin, or Ninurta)]", and for seven months he shall see fair things: . . . pure . . . of Nipur (?) thou shalt apply to his head . . . for seven days continue the treatment, and he shall recover.

10. . . . sickness that man, remove (?), [that man "seeketh the Temple of Shamash (or Sin, or Ninurta)]" and shall see fair things: for his recovery . . . oil of cypress on his head thou shalt put . . . thou shalt continue the treatment and he will recover.

No. 15. *AM.* 5, 1 (K. 2532), *top broken.*

2. . . . leek, an old shoe, together thou shalt dry, reduce . . . lead, antimony,[5] salt together thou shalt mix, once, twice, (or) [thrice] . . . If

[1] ZU, opposed to the ordinary UŠ.NU.ZU? Cf. the similar text 96, 4, 1–6.

[2] For *aruštu* Del. *HWB.* 141 quotes Sarg. *Ann.*, 201, A.BAR *munammir arušti-šunu* "antimony (or plumbago) which polishes their *aruštu*".

[3] For this phrase cf. 34, 4 *r.* 5, 9, and 37, 2, 8. It may be a euphemism, especially the "seeing fair things" (madness, delirium?). Unless, of course, it means that he shall go to the temple for relief.

[4] Cf. 34, 4 *r.* 5, or 9, as part duplicate.

[5] A.BAR, long connected with Syr. ܐܒܪܐ "lead", cf. 1B. No. 13, *el-abar* collyrium, from ابّار. It came as tribute to Tiglath-Pileser I (v, 39) from Melitene. Noticeable is *uban* A.BAR ("a finger of A.BAR," 101, 3, iii, 13); cf. with this, 19, 6, 3, *ana lib* A.BAR *tanadi*

ditto alum (and) chamomile thou shalt bray, in cedar-oil mix, [apply to his head, and he shall recover].

5. If a man's head in his youth is full of grey hairs, to blacken the grey hair . . . a stork (?)[1] (a stork), or a . . . in the fire thou shalt boil, their dung thou shalt take, in oil [thou shalt mix] . . . the Charm "ŠAK.KI EN.NA" seven times . . . the Charm "ŠAK.KI EN.NA" thou shalt recite . . .

10. If ditto, thou shalt take a ram's horn, with bone of . . . in a fire "he" shall calcine (it), with oil . . . three days on his head thou shalt bind it . . .

13. (Cf. 4, 1, 14.) If ditto, the *ma-ereš-ma-lî* plant, the head of . . . , the head of a black raven, the head of a hawk,[2] the head of . . . together

ubanu . . . , 77, 6, 7 *ša* (?)-*pan-ti* A.BAR *ina* NI . . . , 49, 4 *r.* 8 NAM.SI.DI (= ? *nam-si-di*) A.BAR *teppuš*(*uš*). In 101, 3, 9 . . . A.BAR is applied to anus.

But the more common group is LIŠ.A.BAR (especially in eye-texts), which must mean antimony, collyrium, or properly the sulphide, antimonite, long used in Syria for darkening eyelids (Rutley, *Elements of Mineralogy*, 289). LIŠ = *idgurtu*, which latter word is used as an adjective to "mountains" (*idgurâti*, MA. 129), which must refer to the sharp peaks. This points to LIŠ = "needle" or similar, i.e. *idgurtu* = Syr. ܣܘܓܪܬܐ "great needle" (cf. 19, 6, 17, *id-gur-ti* NI . . .).

In LIŠ.A.BAR "needle(s) of antimony" we must see either (*a*) the needle with which the stibium was applied; cf. *SM.* ii, 118, "wrap a piece of wool round the top of a *kohl*-needle," to insert in the ear (or 194, similar), or the description of a girl painting her eyes (Chandler, *Travels*, ii, 140, quoted Smith, *Smaller Bible Dict.*, 396), "thrusting in at the external corner a bodkin which had been immersed in the soot"; or (*b*) more probably the peculiar elongated prisms of antimonite, found in masses having a columnar structure. It will be observed in the eye-texts that it is the LIŠ.A.BAR which is ground up.

LIŠ.A.BAR is prescribed if a man's ear is heavy: "after this a LIŠ.A.BAR in oil or . . ." (34, 1, 22) (cf. the use in *SM.* above); perhaps also to insert up the penis (62, 1, ii, 10); and . . . *ina* LIŠ.A.BAR *itti dišpi* ḪI.ḪI "in (with) antimony-needle(s) with honey thou shalt mix" (43, 1, ii, 11). But its chief use is for the eyes: LIŠ.A.BAR *ta-zak* (8, 1, 10) "antimony-needle(s) thou shalt bray, [apply] to his eyes"; . . . LIŠ.A.BAR *ina* NI *u epir eri* RAT MAR (11, 2, 21) "antimony-needle(s) thou shalt bray with oil and copper-dust, (and) apply" (also cf. 8, 5, 6; 8, 6, 3; 10, 3, 3; 12, 8, 13).

[1] *Igirû*. If the two diagonals following indicate a gloss, then *igirû* = "*laḳlaḳḳa* or . . .", i.e. the stork. The Heb. עֲנָוָר (obviously = *igirû*) is a migratory bird which chatters (Isa. xxxviii, 14; Jer. viii, 7), which is a description applicable to the stork. Sennacherib (*CT.* xxvi, 30, ll. 48, 57, ed. King) describes how he introduced the *igirû* into Nineyeh, to occupy the pools along with the wild pigs, describing them as having a far distant home.

The White Stork is common round Mosul. I do not remember the Black Stork there, but it is obvious from our medical text that a black bird is necessary, and it may well be that the *igirû* is the Black Stork (which is not a proper inhabitant) as distinct from the *laḳlaḳḳa*, White Stork (although it is true that the White Stork has some black on its wing). Other black birds in Mesopotamia are: (1) the Cormorant, which I have seen at times at Mosul, driven in from the sea; (2) the Black Glossy Ibis, but only at Birejik (see my *Pilgrim's Scrip*, 308), where it is a migrant. In *SM.* ii, 691, the "fat of a black raven" is the equivalent.

[2] *Issur ḫurri*, Zimmern (*Akkad. Fremdw.* 51), Arab. *ṭêr el-ḥurr* "hawk". Cf. hawk's blood, in *SM.* ii, 702, to make hair grow.

thou shalt reduce, bray, in good oil mix, on his head press, [bind on, and he shall recover].

16. If ditto, thou shalt catch a chamaeleon,[1] open its stomach . . . , return the . . . to its stomach; thou shalt wait (?) [2] . . . for seven days . . . ; the . . . from its stomach thou shalt take, dry, reduce, bray, [mix] in good oil . . . , on his head press; seven days thou shalt anoint (?), bind . . . "The grey hair shall become black"; while thou bindest him, the charm thus thou shalt recite . . .

(Perhaps add here No. 16, *AM.* 6, 4 (K. 13505), *top broken.*)

2. While thou bindest him, the charm thus [thou shalt recite] . . .

3. If a man's head [has] grey hairs (?, *paršumâte*) . . .

4. [If] a man's head [has] grey hairs (?) . . . thou shalt bray . . . licorice, in honey [mix and apply].

6. [If] a man's head has grey hairs (?) . . . *usa*-beer . . .

No. 17. *AM.* 5, 2 (K. 2471), *top broken.*

2. **Bellis* (and) *crataegus azarolus* (?), their (?) leaves thou shalt pound . . . 5 shekels of gum of *galbanum, 5 shekels of wax (honeycomb) into a pot . . . The gum (?) [3] which thou hast bound on thou shalt take off: after thou hast taken it off . . .

5. If a man has anointed himself with unguent which is not fresh (?),[4] and his head . . . thou shalt wash, dry; then *gulbana*-plant [5] thou shalt dry, reduce, [apply].

[1] *Âr ili* "sheen of god" (like the plant *âr kaspi* "silver sheen", i.e. anemone, *AH.* § 10, r). It is a synonym of *ḫarba-bibillu*, Del. *HBW.* 51, and it occurs again with *lummû* and *ḫammu* as equivalent of . . . MUL (*CT.* xiv, 2, K. 71, A, I, 32-34), Delitzsch thinking that in this latter occurrence it is a different word from the former. (Cf. *KAR.* 182, 1, 19.)

Ḫarba-bibillu is comparable to the Arab. حرباء "a chamaeleon", which is obviously the magical animal necessary here to change grey hair to black, and the Syriac at once shows how it can adopt Assyrian in Syr. ܐܪܝܠܐ "a lizard", i.e. from our *âr ili*. "Sheen of god" must refer to the chamaeleon's capacity for changing colour.

Cf. Weidner, *RA.* 1914, 119, *ḫar-ba-bi-lu*[*m*], and *CT.* xxii, 48, 8, *ḫar-ba-bi-li*. *Lummû* looks like "that which changes colour", from the Arab. *l'mâ* in viii conj. passive, "be changed" of colour of face.

[2] *Tetekib*, Heb. עָכַב "wait", or *teteḳib*, Aram. עְקַב "come late".

[3] A.DAN.

[4] The reading must be *šamni la ta-ki* ("oil not fresh (?)"); but *taki* can be referred to תוך "to be damaged". *Lataki* לאם "fit" is possible.

[5] I doubt *galbanum*, which should be √חלב.

7. If ditto, thou shalt bray **calendula*, in [oil mix, and apply it]. If ditto, thou shalt bray chamomile, in [oil mix, and apply it]. If ditto, thou shalt bray PI.PI-fennel, [in oil mix and apply it].

No. 18. *AM.* 5, 3 (K. 10655 + 10926), *top broken.*

The section I, 8–10, is duplicate of *KAR.* 202, i, 20, and should read *Enuma* NA *muḫḫu-šu* UD.DA-*at ênâ*II*-šu ibarrura . . .* GAB (?) *tabašal* ^{š}*buraši* ^{š}*ṣumlali* ^{š}*balulḫi saḫlê* GAB *šamaššammi* ^{u}*siḫi ina šizbi talâš*(*aš*) *taṣamid-ma ibaluṭ*(*uṭ*).

" When a man's brain is on fire (?), his eyes *ibarrura . . .* thou shalt boil wax (?): thou shalt knead in milk pine-gum, oleander (?), *galbanum, lolium, mucilage (?) of sesame, artemisia, bind on, and he shall recover."

Cf. *Ud-da-at* (?), 6, 9, 6, and *KAR.* 202, ii, 27, *Enuma* NA UD.DA-*at* TUK *šârat ḳaḳḳadi-šu išaḫḫuḫ*, the latter being duplicate of *CT.* xxiii, 32, 8, and Jastrow, *Trans. Coll. Phys. Philad.*, 1913, 18. Jastrow restores his broken text from elsewhere in *CT.* xxiii, with NE.NE (" *išatu uḫammaṭ* ") for UD.DA-*at*: it may be that we must read UD.DA-*aṭ* (= *iḫammaṭ*) here.

Ibarrura or *iparrura*, difficult. For *barâru* Jastrow suggested no Semitic comparison, although he gave a rendering. The verb *barâru* is a synonym of *palâmu* (Del., *HWB.* 188) = Aram. פְּלַם, Pa. " to cover with mud " (i.e. here matter, pus ?); while *parâru* might be Heb. פָּרַר, Pilp. " to quiver ". It occurs again, 97, 4, 30, duplicate of *KAR.* 184: *Enuma* NA ŠAG.DIB.BA TUK.TUK-*ši* PI^{II}*-šu išagguma ênâ*^{II}*-šu ibarrura* SA TIG-*šu ikkalu*^{pl}*-šu*, etc.

Cf. *CT.* xxiii, 23, l. 1 ff. (dupl. of *KAR.* 202, i, 1) *Enuma* NA *muḫḫu-šu išati ukal* SA.ZI ŠAK.KI TUK-*ma ênâ*^{II}*-šu idak ênâ*^{II}*-šu birratu*, and ib. 27, 12 [*Enuma* NA NE].NE TUK-*ma ênâ*^{II}*-šu ibarrura dâma ukalla.*

No. 19. *AM.* 5, 5 (K. 8074), *top broken.*

1. [If ditto ?], thou shalt pound chamomile, fat . . .

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2. To remove itching, fennel (?), UŠ.GUL[1] in lees (?) of . . . [thou shalt apply].

---

3. For ditto, thou shalt rub[2] chamomiles on the place, " black alum "[3] . . .

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[1] For UŠ.GUL cf. 17, 1, 2.

[2] *Takar* (√*kâru* ?). From the passages in which this occurs it must mean " to rub, smear ", e.g. *eḳir šinnišu takar-ma ibaluṭ* " the root of his tooth thou shalt rub and he shall recover " (36, 2, 11, cf. 4): " When *ênâ*^{II}*-šu* DUL-*ma* (= *katma*) *ukal ênâ*^{II}*-šu takar* thou shalt rub his eyes " (for dimness or cataract ?, 9, 1, 37): *adi dâmu* ŠI.GAB *takar* " thou shalt rub until blood appears " (25, 6, ii, 8): rub anus (53, 1, iii, 3; Kū. ii, iii, 48). Cf. כִּיֵּר " smear ".

[3] IM.IŠ.MI.KUR.RA.IM.IŠ.TAK.KUR.RA = *gabî* " alum " (Thureau-Dangin, *RA.* 1920, 28): IM.IŠ.MI.KUR.RA (opposed to IM.IŠ.UD.KUR.RA, 59, 1, 45) occurs again 59, 1, 29, to be drunk

4. For ditto, seed of leeks, ** *ricinus*, "black"-plant (*Xanthium strumarium* ?) together thou shalt bray, [apply].

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5. If a man's head gets an itch, thou shalt bray flour of corn, rub on . . . Thou shalt remove his (its) *ritrittu* (scab ?), wash in beer: powdered (bark) of box thou shalt apply, bind on . . . powdered (bark) of box, powdered (bark) of *elammaku*, powdered (bark) of the "all-unguent" tree . . . on the front thou shalt remove (?), thou shalt press, thou shalt reduce *suadu* (and) cedar (and) bray (them) . . . in rose-water thou shalt wash: the powdered (bark) of box, the powdered (bark) of *elammaku*, the powdered (bark) of the "all-unguent" tree . . .

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10. If ditto, thou shalt shave his head, thou shalt anoint (it) with fish-oil,[1] before he sleeps[2] thou shalt remove (it), the root of . . . , the root of *kumaḫu*, bed (dregs) of GAR.RIN.NA, lupins, seed of *ḳutrate* . . . together thou shalt bray, with cattle-urine thou shalt bind his head, in beer thou shalt wash, in rose-water thou shalt . . . , seed of *vitex agnus castus*, seed of mandrake, linseed, seed of sumach, PI.PI-fennel juice, . . . , turmeric, *saggilatu*-alkali, juice of *ma-ereš-ma-lî* plant . . . thou shalt dry, pound, (and) strain, in rose-water knead, again dry, pound, (and) strain, in beer . . .: for three days thou shalt not take (it) off; on the fourth day, when thou takest it off, in hot urine thou shalt wash . . .

No. 20. *AM.* 6, 1 (K. 11544), *top broken.*

2. . . . scarlet anemones thou shalt bray, [apply to] his head . . . [*e*]*ḳidu* without salt thou shalt bray, in milk thou shalt [knead] . . . [apply] the scarlet anemones to his head, . . . cool [it]; then the aforesaid *eḳidu* without salt . . .; [thou shalt bind] on his head in a bandage . . . thou shalt not take (?) his . . .

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8. . . . flour of spelt-corn in water thou shalt knead, bind his head . . .

---

9. . . . sulphur, salicornia-alkali, cantharides (?) . . . these [drugs] together in the shade thou shalt dry, pound, [strain] . . .

---

for dyspnoea, for which alum is also drunk, l. 24. Drunk in Lutz, *AJSL.* xxxvi, 80, l. 1. Its value appears to be *saḫ* (?) (*kit* (?), *lil* (?))-*mu* (*VR.* 27, 19, *e–f*). Pliny (xxxv, 52) speaks of several kinds of alum, the white and the black, both used for dyeing; but Bostock in his note (loc. cit.) quotes a Dr. Pereira, who says that Pliny did not distinguish alum from sulphate of iron, since he calls one white and the other black. Are we to consider IM.IŠ.MI.KUR.RA = sulphate of iron ?

[1] NI.KIL = *šaman nâni*, *SAI.* 3701.

[2] Cf. KI.NÀ, 47, 1, 1.

No. 21. *AM.* 6, 2 (K. 8008).

1. . . . which on the head of the sick man thou hast bound . . . [the charm . . .] UŠ.MU.E thereafter thou shalt recite.

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3. . . . which the opening of his mouth hath turned to favour, in the house wherein the charm hath been performed, in the (ditto) which the charm hath . . . (*ušširu* ?). The lord to [compose ?] his limbs,[1] the lord to learn [his disease] . . . [at] his bed a kid at the head of the sick man . . . "Take [the . . .] of a cow." . . .

No. 22. *AM.* 6, 3 (K. 9828 + 11868), *top broken* (cf. K. 13417, Pl. 12, and K. 13398, Pl. 41).

2. Thou shalt pound and strain . . .

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3. 10 shekels of [dried ?] powder of mucilage (?) of sesame . . .

---

4. 10 shekels of dried (?) powder of mucilage (?) of sesame . . . the first day thou shalt bind his breast, his head . . . thou shalt pour on his head, in the house . . .

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7. 1 shekel of cantharides (?), ½ shekel of . . . (Cf. 8, 1, 27.)

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8. ⅓ *ḳa* of seed of *pirḫi*, ⅓ *ḳa* of seed of beans (?) . . .

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9. ⅓ *ḳa* of ground meal, 1 (?) shekel of juice of . . .

(*Four lines broken.*)

15. ⅓ *ḳa* of juice of . . . thou shalt again dry . . .

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17. ⅓ *ḳa* of *supalu* (gum ?), ⅓ *ḳa* of . . .

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18. Salicornia-alkali . . .

(*For AM.* 6, 4, *see* 5, 1.)

No. 23. *AM.* 6, 5 (S. 414).

1. . . . thou shalt take out the hair (*or*, thou shalt thread (on) wool) . . .

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2. . . . thou shalt heat, grind, apply . . .

---

3. [* stor]ax (?) thou shalt bray, [apply] . . .

---

[1] Cf. my copies *CT.* xvi, 5, 181 ff., and translations, *Devils*, i, 19.

4. . . . in white (?) mountain-honey and water thou shalt wash and he shall recover.

5. . . . in oil thou shalt anoint, **calendula*, fennel . . . in water thou shalt wash his head, in oil anoint . . .

7. [His head] thou shalt wash in water (?) and bray fennel, anoint in oil.

8. . . . in mountain-honey thou shalt anoint . . .

. . . . . .

No. 24. *AM.* 6, 9 (K. 10212), *top broken.*

3. . . . thou shalt [boil], spread on a skin, [bind on and he shall recover].

4. . . . head, *arê*-(plant),[1] *kankadu*-(plant) thou shalt dry . . . [in] *barga*-oil mix, continuously [apply].

6. If a man his head . . . ,[2] and his body hurt [him], thou shalt dry . . . , pound, (and) strain; flour of *gu-gal*, flour of *gu-du*, flour of . . . in lees of beer thou shalt knead [and apply].

9. If ditto, dried (?) mucilage (?) of sesame, fir-gum, pine-gum . . . [thou shalt apply].

10. If a man's head get a pain, roses[3] . . . [If ?] his head take a pain, licorice-root . . . in the morning without a meal let him drink.[4]

No. 25. *AM.* 7, 3 (K. 10562), *top broken.*[5]

1. If ditto, vinegar,[6] *billu*-wine . . .

[1] Cf. 39, 1, 38, *ᵛa-ri-e.*

[2] *Ud-da-at* (?) (or *ud-da-šir* (?)). See No. 18.

[3] Cf. 64, 1, 15 . . . *su a-ḫi-iz kam-ti kasî zir ᵛšimri* . . .

[4] Cf. 64, 1, 4.

[5] Joined to K. 7953, pls. 64–65, after this was in type.

[6] *Ṭâbatu*, A.GEŠTIN.NA. Küchler (83) and Del. *HWB.* (298) "water mixed with wine". But the ideogram really means "water (liquid) of wine"; water with wine would have very little value in the medical receipts in which A.GEŠTIN.NA is prescribed. My own view is that it represents *vinegar, acetic acid* in its simple form. Vinegar occurs more than a hundred times in *SM.*, and yet we have as yet identified no proper word for it in Assyrian. Nothing, as far as I know, has been compared to the Syr. *ḫallâ*, but conclusive is BIL.LAL = *ṭâbatu* and *enṣu* (*SAI.* 3157, 3156; Meissner, *MVAG.* 1913, 2, 20, 17), the latter being obviously חוֹמֶץ "vinegar".

In a case where the patient voids urine "like that of an ass", followed by blood, the physician is to introduce two (or three) shekels of gum of *galbanum mixed with ⅓ *ḳa* of A.GEŠTIN.NA up the penis (66, 7, 18). This mixture is made for the good reason that acetic acid is a powerful solvent of gum-resins (i.e. * galbanum) (*PC.* xxvi, 342). A similar small

2. Again, river-bed (*or*, lees of beer ?) . . .

3. If ditto, scabies thou shalt cover (?) . . .

4. If ditto, vinegar, *billu*-wine . . .

5. If a man's head . . . river-bed (*or*, lees of beer ?) . . .

B. Diseases of the Eyes.

No. 26. *AM.* 8, 1; *AM.* 12, 8; *AM.* 20, 2 (K. 2570, etc.), *top broken.*

(*a*) . . . thou shalt reduce, bray . . .

(*b*) [If a man's eyes] . . . its leaf, *salicornia*-alkali thou shalt bray, in rose-water knead, [bind on]: . . . bray, beat up in fat, in equal parts bray, [mix] in curd, [apply].

(*c*) If a man's [eyes] are sick and matter (?) is secreted on his temples, [thou shalt spread] tanners' verdigris[1] on vellum (?), on [his eyes] bind: bray copper-dust, arsenic,[2] yellow sulphide of arsenic, mix in curd, apply to his eyes . . .

(*d*) When a man's eyes are sick and are inflamed, thou shalt bray dust of a copper saucepan in curd, apply to his eyes; a bronze blade . . . in water thou shalt wash . . . once, twice, thrice apply to his eyes, *lolium* in beer-dregs thou shalt knead, apply.

(*e*) [If a man's eyes] . . . [in a (?)] saucepan thou shalt reduce, pound, bray in sweet curd.

2. When thou doest . . . apply to his eyes.

quantity is used (41, 1, iv, 21). A.GEŠTIN.NA is prescribed with other drugs for using on the head (65, 5, 8) (cf. vinegar externally for the head as a medium with other drugs, *SM.* ii, 41, 63, 64, 65). It is also used for the temples (20, 1, iv, 39). It is boiled in a pot, and then many plants are introduced into it to make an enema (49, 6 *r.* 6). It is used in a mouth-wash (28, 7, 7) (cf. *SM.* ii, 187, etc.); for toothache (28, 1, 4) (cf. *SM.* ii, 186). Note 50, 3, 4, "once, twice (or) thrice the lolium . . . which vinegar has not destroyed he shall eat" (*ša* A.GEŠTIN.NA *la šul-pu-tu ikkalᵘ*). "Wine-water" could hardly have the strength necessary to "destroy" anything, whatever the object of the verb may have been here. (Cf. 66, 7, 22, [A.GEŠTIN.NA.D]AN.GA *tuballal e-nu-ma* TAK (= *ušalpit* ?)-*šu*, etc.) "Strong vinegar," A.GEŠTIN.NA.DAN.GA, is still more indicative: it is a medium for 25 drugs and aromatics (84, 1, ii, 4); in 57, 5 *r.* 3, ⅓ *ḳa* of alkali, ⅓ *ḳa* of "strong vinegar", 5 shekels of salt, 5 shekels of **ammi, boiled in *kurunnu*-beer and with oil added to it, is to be poured on (up ?) the anus.

[1] "Shoemaker's vitriol," *SM.* ii, 114.

[2] See l. 23.

3. ***Ricinus* thou shalt bray, cook in beer . . . pour in his eyes; thou shalt bray **bellis*, bind on. Thou shalt bray *pirḫi*-plant (and) bind on: *artemisia*, *sagapenum in milk knead, bind on; raw[1] kidney-fat in the morning thou shalt put . . . an antimony-"needle" thou shalt bray, appiy to his eyes.

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6. If his eyes are sick and are dim,[2] pine-cone-gum . . . in water thou shalt wash, bind: fruit of the poppy, *storax . . . [thou shalt apply].

8.[3] If a man's eyes are sick and for many days will not open, in the fire (of his head his eyes are full of film, his head) thou shalt press, three times a day dough thou shalt cool, (seed of **bellis* thou shalt reduce, in *pûru*-oil) an antimony-"needle" thou shalt bray, (and) [apply] to his eyes.

---

11. If a man's eyes are affected with dryness,[4] he shall rub[5] an onion, drink it in beer, [apply] oil to his eyes . . .

---

12. Powder of date-stones[6] thou shalt reduce, bray, knead in rose-water, bind[7] on, before a meal let him . . .

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13. Thou shalt disembowel a yellow frog,[8] mix its gall in curd, apply to his eyes.

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14. 5 (?) *bur*[9] of [*gu*]-*gal* flour, 6 *bur* of powder of roses, 5 shekels of lolium (in) rose-water . . . thou shalt bind on his temples and eyes.

---

15. Thou shalt bind on . . .; where the seed of the . . . -plant has gone in he shall touch (?)[10] and . . . three times "..." he shall say.

---

[1] *Sâmu* "red".

[2] For the use of *ma u*, cf. 20, 2, 7.

[3] Dupl. of *KAR.* 183, 11 ff., whence the restorations in round brackets.

[4] *Tabila(m).* E. is probably right in his translation "dry" (xiii, 5). It is used of *storax when applied to the eyes (8, 1, 28; cf. 92, 8, 9), to ears (36, 1, 18): of GIŠ.GIL (= tannin), applied "dry" to eyes (16, 1, 4): various cereals, including lolium, applied "dry" to the head (16, 1, 6): of drugs to be applied "dry" to the head (64, 1, 38): cf. 13, 6, 10; 18, 4, 5; 28, 7, 10. "If a man coughs 'dry', (and) does not spit" (81, 3, 4; cf. 81, 1, ii, 22).

[5] *Uḫašša*, doubtless, if not Syr. ..., Af. "to rub", some form similar to it. Cf. 105, 1, 18, [*šû*]*mu* ŠAR *ina eli uḫašša*. The remedy of drinking a raw onion in beer would presumably induce tears.

[6] For this receipt cf. *SM.* ii, 663.

[7] *Tukappat*, Heb. *kâphath* "bind".

[8] Cf. *SM.* ii, 66, 3.

[9] It is doubtful whether the passage in l. 27, with its variant *šiḳlu* "shekel", gives us much help in deciding the amount of the *bur*: "½ *bur*" is clear; in "½ shekel" the "half" is not written so clearly, but seems probable.

[10] Very doubtful rendering: for *eḳû* see p. 4, n. 6.

17.[1] If a man['s eyes are sick, **ammi, *hyoscyamus*, rose]s, lolium, *maštab*-plant, fat of *opopanax, seed of pine, gall (?) [of the tanners] [2] . . . in curd, suet of the kidney of a male sheep, wax (?) of honey thou shalt include,[3] apply to his eyes.

19.[4] If a man's eyes [are sick, * storax in curd thou shalt bray and apply:] [If a man]'s [eyes] are sick, cantharides (?) in curd thou shalt bray (and) apply.

20.[4] If a man's eyes are sick [alum in curd thou shalt bray and apply: If a man's eyes are sick, *lol*]*ium* (?) in milk thou shalt boil (and) apply.

21. If a man's eyes are full of blood, and day and night not . . . and the middle of his eyes is red, his eyes being dim,[5] thou shalt reduce roses . . . [in] cows' [milk (?)] or in the milk of a harlot [6] thou shalt knead (and) bind on. In the morning thou shalt take it off, and bray arsenic [7] in curd,

[1] Part duplicate of Scheil, *RA.* 1921, xviii, 1, 6.

[2] *Kammu* (*ša aškapi*): Kü. iii, ii, 18: cf. 19, 6, 8, 10, 13, externally for eyes (note the peculiar form of the ideogram): for eyes (10, 4, 6), ears (34, 1, 28). It cannot be the "sumach of the leather-workers" of *SM.* ii, 487 (the Assyrian for "sumach" is ^{u}LID.GAB, *ṣapru*, *AH.* § 10, BG); but gall is used for both eyes (frequently) and ears in *SM.* (ii, 112). The oak-galls are the source of tannic acid, (a styptic) which is a staple commodity of the hills of North Mesopotamia (see my chapter in Woolley, *Carchemish*, ii). What seems to be conclusive is 12, 4, 6: *ina lipî kalîti alpi ṣalmi kima kam-ma*, i.e. "in the kidney-fat of an ox black as gall". Cf. *šuḫta ša aškapi*, 12, 8, 5, and its duplicate, 20, 2, 5.

For the sign *aškapu* see Christian, *Vienna Oriental Journal*, 1911, 425; De Genouillac, *OLZ.* 1908, 381; Meissner, *OLZ.* 1911, 385.

[3] *Tuštabbal.*

[4] Duplicate Scheil, *RA.* 1921, xviii, 1, p. 6.

[5] DUL-*ma*, presumably *katma*(*ma*), since *dulmu* appears to be out of the question. *Ênâ*^{II}-*šu* DUL-*ma u-kal* (9, 1, 37): . . . *u* DUL-*ma ta-za-ak* (62, 3, 14): *ênu* DUL-*ma išakkan*(*an*) (?) (13, 3, 5): . . . *-su-u* DUL *na-di-* . . . (95, 4, 3). Cf. *kat-mu* (22, 3, 1, 2).

[6] *šal*ŠAMKA (or, ŠAL.ŠAM.KA). *Šamkatu* is another form of *šamḫatu* (MA. 1058), and presumably this is the same. Cf. l. 33 (curious); 12, 3, 5; 13, 2 *r.* 9; 13, 6, 13; 57, 10, 5.

[7] *tak*AŠ.ḪAR (probably to be read thus instead of ^{u}AŠ.ḪAR). Before discussing this (which, as AZ.ḪAR on *CT.* xiv, 8, obv. 14 = ŠIM.BI.ZI.DA * SIG.* SIG) it will be well to discuss ŠIM.BI.* SIG.* SIG of l. 28 (Br. 5185, 5186) first.

Consider the following equivalences:—

ŠIM.BI.* SIG.* SIG	= *lêru*, *šîpu* (Br. 5185, 5186).
ŠIM.BI.GUŠKIN	= *lêru*, *šîpu* (Br. 5187, 5188).
IM.ŠIM.GUŠKIN	= *šîpu*, *šindi ḫuraṣi* (*SAI.* 6297).
IM.GUŠKIN	= *šîpu* (Br. 8487).
ŠIM.GUŠKIN	= *šîpu*, *lêru*, *damatu* (Br. 5198–5200).
IM.ŠIM.TAK.IŠ	= *lêru*, *šindi ni-* . . . (*SAI.* 3548).

From a comparison of these, and from analogy with *šindi ḫuraṣi*, we may reasonably see in ŠIM.BI.* SIG.* SIG an original equivalence *šindu arḳu*.

Pinches was nearly right when he said that *šindu* is "probably not 'spot' but certainly 'mark'" (*JRAS.* 1898, 444, quoted MA. 1072): "an eight-year-old brown donkey without

his eyes *rapadi*-plant [1] on a bronze blade [2] [thou shalt anoint ?]. A thread

šindu on it" (MA. ibid.) certainly indicates "paint" when it is considered alongside of the quantities "four *mana* of *šindu*" and "sixteen *mana* of *šindu*" of the contracts (quoted Del., *HWB.* 674). Cf. Langdon, *OLZ.* 1909, 112, and Dougherty, *Shirkûtu*, 84.

Šîpu and *lîru*, the equivalents of *šindi ḫuraṣi*, are considered as "Goldleiste", "Goldlehm", "Goldpaste" by Del., *HWB.* 645, and Jensen (*KB.* vi (1), 510), both reading *šîbu*. (Cf. Neb. iii, 30, *šîbi šaplanu ʿerini zulûlu*.)

But we can go a good deal further in the identification than "Goldleiste". The word, on the analogy of قُصّ "smeared", معصا *pasta*, Syr. ܡܨܐ "liquid gum", must be *šîpu*, not *šîbu*: *lîru*, its synonym, is doubtless Heb. רִיר, Arab. رَال "slaver", etc.

We have therefore to define a drug not remote from "gold paint" or "gold leaf", used in *AM.* as (1) ŠIM.BI.* SIG.* SIG for eyes (l. 28 and l. 31), and read, as I suggest, *šindu arḳu* "green (yellow) paint"; (2) ŠIM.BI GUŠKIN "gold paint", also for eyes (with *ḫîlu* of copper and ^{tak}AŠ.ḪAR, 12, 8, 6; 20, 2, 6); 15, 4, 2; for mouth (externally, with *nigella*, **ammi, *eruca*, alum, and *storax (36, 2, 3); and even drunk (for pain ?) with thyme, *supalu*, lolium, *nigella*, etc., in oil and *kurunnu*-beer (16, 4, 5).

The eye-medicines and mouth-washes of *SM.* ii, 90, 94, and 176, 181, 187, 188, contain that drug which easily suggests itself here, i.e. *arsenic*; it is even drunk in *SM.* ii, 412, 463, etc. As a paint it is the yellow trisulphide of arsenic, a natural product of Kurdistan (F. Rutley, *Elements of Mineralogy*, 285), which provides the yellow dye or paint known as *auri pigmentum* or orpiment.

This is so close to σανδαράκη (cf. Pliny, xxxiv, 18) that it is obvious that the Greeks borrowed the Assyrian words *šindu arḳu* "green (yellow) paint". There is the slight objection that σανδαράκη in Diosc. and Pliny appears to be more the *realgar* or red (and orange) sulphide than the yellow orpiment. Yet from Bostock (Pliny, xxxv, 22, note) it would appear that Pliny speaks of the pale or massicot, as well as the red, and it may well be that a confusion arose between the two kinds, especially as σάνδυξ = "the red stone" (*sându*, cinnabar). We may therefore consider that ŠIM.BI.* SIG.* SIG and ŠIM.BI.GUŠKIN are the yellow sulphide of arsenic, the "yellow arsenic" of *SM.*, and as equal to *šindu arḳu* = σανδαράκη.

We can now turn to ^{tak}AZ.ḪAR. There is no doubt about the equivalence ^{tak}AZ.ḪAR = ^{tak}AŠ.ḪAR: the former occurs 10 times in *AM.*, the latter 6 times. It is used externally, chiefly for eyes (here in curd, and 16, 1, 21); 9, 1, 24; 12, 4, 5; 12, 8, 6; 15, 4, 6; 19, 6, 12 (in powder); 20, 2, 6; 50, 1, 3 (eyes (?)); for cheeks (or beard, 26, 8, 9); for ? 57, 10, 2. For some urinary trouble, brayed alone in curd, or with salt in curd, for [introduction by penis], 62, 1, ii, 6, 7.

AZ.ḪAR, as we saw above, = ŠIM.BI.ZI.DA *SIG.*SIG, evidently closely connected with ŠIM.BI * SIG.* SIG. ŠIM.BI.ZI.DA in *SAI.* 3546 = *guḫlu*, i.e. *koḥl* (Zimmern, see MA. 215). We have thus a "green *koḥl*" used for eyes chiefly, but also for urinary trouble, called AZ.ḪAR, AŠ.ḪAR, closely allied to yellow sulphide of arsenic, but not quite the same, as it is used side by side with it in the same prescription (12, 8, 6). But it is obviously some form of arsenic. It can hardly be the *ghâr* in the Arabic "realgar", since the latter (red sulphide of arsenic) does not comply with the "green" demanded. It is more probable that AZ.ḪAR, AŠ.ḪAR (a foreign, borrowed word, since there is some doubt about the sibilant) is comparable to Arab. *ḫaḍira* "be green", and even to ἀρσένικον, ἀῤῥένικον, both curious words.

[1] For this plant see *AH.* § 10 R. *Rapadu* is the name of a disease.

[2] GIR UD.KA.BAR. GIR is properly a dagger, razor, or sickle (an edge or a point): e.g. in 11, 2, 7, ". . . take out and GIR UD.KA.BAR *ḫimeti tapašaš* thou shalt anoint a bronze blade with curd"; 10, 3, 31, . . . *ina dišpi* UD GIR.UD.KA.BAR „ (= MAR) ". . . in white honey a bronze blade thou shalt apply"; 12, 8, 7, ". . . thou shalt apply to his eyes: a bronze blade . . . once, twice, (or) thrice thou shalt apply to his eyes." All the above are eye-texts: 70, 9, 4 is uncertain.

of blue [1] wool and of white wool thou shalt spin, twist, bind on; his temples on both sides . . . thou shalt take out, and his eyes . . .

26. If a man's eyes are changed [2] to blood, seed of *eruca*, mountain-honey, yellow sulphide of arsenic [3] thou shalt mix [and apply].

27. 1 shekel of cantharides (?), ½ shekel of *storax, ⅛ "Akkadian salt" [4] [thou shalt bray, apply to his eyes].

28. Thou shalt apply to his eyes dry *storax: therein yellow sulphide of arsenic. . . . Thou shalt bray the dung of a lizard [5] in marrow of "long bone" (thigh), without a meal [thou shalt apply].

30. 5 *še* of *storax in oil thou shalt bray, apply to his eyes; cantharides (?) in curd [thou shalt mix and apply].

31. If a man's eyes are full of blood, thou shalt bray yellow sulphide of arsenic in curd, apply . . .[6]

32. Thou shalt mix **bellis* and lupins in equal quantities, [thou shalt apply them] in curd (and) *liquidambar.

[1] *Ḳannu*. It is opposed to "white" wool here; on the other hand, in *ADD.* No. 954, 1–2, the contrast is between it and *ṣalmu* (GIG), both also of wool. *Ṣalmu* is used of flowers (e.g. the anemone, *AH.* § 9 P, ll. 145, 146), but also of the "black-headed race". *Ḳannu* would appear to be connected with Syr. ܩܢܘܢ *κνάνεος* (perhaps *uḳnu*, Jensen), or more probably with Syr. ܨܒܥܐ *isatis tinctoria*, woad. The connexion of *urapadi* in this prescription may offer some explanation (in its connexion with "blue wool") for *AH.* § 9 B, l. 167 [*ura*]-*pa-di* = *uša-mi uḳ-na-a-ti*.

[2] *Šunnu'*, either Heb. שָׁנָה "are changed", or perhaps, like *šinîtu* "dyed garment" (MA. 1076), from Heb. שָׁנִי = "dyed scarlet".

[3] Doubtless for ŠIM.IŠ.GUŠKIN. See p. 24, n. 7.

[4] MUN.EME.ŠAL.LIM, i.e. "Salt of Akkad", as distinct from ordinary salt (*ṭâbtu*), mountain-salt (*ṭâbat šadi*, Kü. i, i, 31), and *ṭâbat Amanim* (cf. 65, 5, 17) "Salt of Amanus", supposed to be ammoniac or rock-salt.

MUN.EME.ŠAL.LIM, 22 times in *AM.*: used for eyes (here: 8, 5, 10; 8, 7, 3; 9, 1, 13, 34, 38; 11, 2, 44; 16, 1, 8, 10; 17, 4, 7); for yellow teeth (31, 6, 7); ears (35, 2, 12); wash head (64, 1, 37); in cough (by letting a piece dissolve in the mouth, followed by pig-meat soup, beer, and honey (80, 1, 12); in fumigation (99, 3, 8).

Various salts occur in *SM.* The most likely equivalent for MUN.EME.ŠAL.LIM appears to be either (1) "Armenian Salt" (cf. IB. No. 381, "Armenian nitre") or (2) "Indian Salt" used for eyes (ii, 89, 98) and for cleaning teeth (ib. 190) (but "Cappadocian Salt" for ears, ib. 115). The probability is that it was called "Akkadian Salt" because it was especially that efflorescence of salt which appears on alluvial soil (certainly common in S. Babylonia), and probably comparable to the Indian Salt, *Réh*, an efflorescence on the surface of the ground, of sodium carbonate, sodium sulphate, and sodium chloride (Sir G. Watt, *Commercial Products of India*, 51), over large tracts of desert; or even potash, also from an efflorescence (ib. 972).

[5] "Dung of a lizard," for eyes, *SM.* ii, 101, 660.

[6] K. 2570 has: *maštakal*-plant (and) *tarḫu*-plant in equal quantities thou shalt mix, [apply].

33. Thou shalt bray (and) apply BAL-"stone" in saliva: . . . in milk of a harlot (?) [thou shalt mix and apply].

34. Blood from a pig's heart [thou shalt pour] into his eyes . . .

(Pl. 9, 1.) Col. ii.

3. . . . (some) drug against pain . . . in rose-water thou shalt wash . . . thou shalt apply.[1]

6. If a man . . . [in] oil thou shalt bray, apply.

7. If a man . . . thou shalt apply.

8. If ditto . . . dry into his eyes thou shalt blow.

9. If a man's eyes . . . *arno*[*glosson*] (and) **bellis* in ground meal thou shalt mix, in wine [thou shalt apply].

10. *Storax . . . , arsenic in curd thou shalt bray, apply.

11. [If] a man's eyes are sick . . . thou shalt bind . . . arsenic in curd thou shalt bray, apply.

13. . . . cantharides (?), "Salt of Akkad," **ammi, . . . these drugs together thou shalt rub,[2] in *ṣilute* . . . with rose-water thou shalt cover, (and) apply to his eyes.

16. 3 shekels of cantharides (?), . . . shekel of . . . mountain-honey thou shalt bray, apply to his eyes. This is a drug for twenty days.

17. Charm: . . . of life [is freed] . . . is freed, People in . . . ? Recite the charm.

20. Incantation: [If a man]'s eyes are full of blood.

21. Ritual for this: root of the plant . . . (and) *ellu* thou shalt bind (together), 14 knots thou shalt tie; as thou tiest thou shalt recite the charm, bind on his forehead.

[1] *Taṭippi*, Syr. *ṭ'phâ*, *Pa. imposuit* (*medicamentum*).

[2] *Tamarrak*, מָרַק "rub" (?). Also 97, 1, 3, *tuṣaḫḫar tamarrak*: 10, 4, 6, various drugs *eštenîš tamarrak*: 95, 2, ii, 13: 52, 1, 8, two forms of asafoetida *tamarrak*. *Umarrak*, E. (xiii, 11) correctly "let melt", but incorrect in his transliteration *marâqu*. It is obviously from Heb. מָרַךְ "to melt". "He shall put a piece of 'Akkadian Salt' in his mouth and *umarrak*" (80, 1, 12): she shall put *allankaniš* in her mouth and *umarrak* (67, 1, iv, 23).

22. Charm: ? Recite the charm.

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23. Incantation: [If a man]'s eyes are full of blood.

---

24. Ritual for this: thou shalt bray arsenic in curd (and) put it into his eyes.

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25. Charm: Sound front, sound back, smitten front, smitten back . . .[1] Flesh multiplieth flesh, blood produceth[2] blood, dung[3] createth (?)[4] dung! Perform, O Gula, the high Charm of Life! Let them bring nigh the cataplasms,[5] (which) thou hast arranged (and) grant recovery![6] Recite the charm E.NU.ŠUB.

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29. Incantation: If a man's eyes are full of blood.

---

30. Ritual for this: 3 *še* of salicornia-alkali, 3 *še* of gum of ***andropogon*, 3 *še* of lizards' dung thou shalt bray together,[7] knead in goats' milk, bind on his eyes.

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31. If a man's eyes are sick and full of blood, unguents (only) irritating (?)[8] the blood, blood (and) tears coming forth from the eyes, a film[9] closing over the pupils[10] of his eyes, tears turning to film, to look oppressing him: thou shalt beat leaves of tamarisk, steep them in strong vinegar, leave them out under the stars; in the morning (i.e. on the morrow) thou shalt squeeze (them)

[1] About one line unintelligible to me, somewhat similar to 11, 1, 3.

[2] *Inaṣab*, Syr. ܝܨܒ, especially to lay, produce (eggs).

[3] *Sânu*, Syr. ܣܪܓܢܐ, human excrement.

[4] *Uḳannan*, doubtful. I can trace nothing probable in Semitic; the assumption is that קֵן "nest" is at the base of it. Can it be connected with *ḳinnatu*, Arab. قِينَّة *podex* (Christian, *Vienna Oriental Journal*, 1912, 390)?

[5] For *ṣimditi* cf. *ṣindi ša ḳat edimmi*, 99, 2, iii, 4, and *napšalti u ṣindi*, 102, i, 35.

[6] For this use of *balaṭ* without termination after *bulṭi* (= *bulluṭi*) cf. my *On Traces of an Indefinite Article*, 27.

[7] This use of alkali, gum, and lizards' dung is parallel to the mention of flesh, blood, and dung in l. 26. See *AH.* § 10 c, 2.

[8] *Ul-ta-ta-ni-'*. If the form is correct, it must be like *uk-ta-ta-ṣar* (MA. 427), either from *šanû* "to change" or *enû*, Heb. עָנָה "to afflict", etc.

[9] GIŠ.MI, i.e. probably a film like conjunctivitis. Cf. 13, 2 *r.* 3, 4; 18, 6, 4.

[10] AN.KAL, of eyes here and 8, 6, 5; 13, 2 *r.* 11; 18, 6, 7. There is no reason to suppose it the same as *irri ênâ^{II}-šu*, 16, 1, 2. It is also the name of a stone, 7, 1 *r.* ii, 7; *SAI.* 376. Is this the blue schist used for eye-pupils in the Tell Ubaid lions (Hall, *Proc. Soc. Antiq.*, 1919–20, 32)?

in a helmet: white alum, *storax, "Akkadian Salt," fat, cornflour,[1] *nigella*, "gum of copper," separately [2] thou shalt bray: thou shalt take equal parts (of them), put them together; pour (them) into the helmet (in) which thou hast squeezed (the tamarisk); in curd and *šuniš*-mineral thou shalt knead (it), (and) open his eyelids with a finger (and) put it in his eyes. (While) his eyes contain dimness, his eyes thou shalt smear, and for nine days thou shalt do this.

38. If ditto, myrrh, *storax, "Akkadian Salt" through a bronze tube [3] into his eyes thou shalt blow.

39. If a man ditto, "gum of copper" (and) *storax thou shalt bray, through a bronze tube into his eyes thou shalt blow.

40. If a man ditto, *mint and *storax thou shalt bray, through a bronze tube into his eyes thou shalt blow.

41. Charm. O clear eye, O doubly clear eye, O eye of clear sight! O darkened eye, O doubly darkened eye, O eye of darkened sight! O eye of sleepy (?) sight, O eye of . . . [4] sight, O eye of evil sight! O failing [5] eyes, O painful [6] eyes, . . . eyes, like the slaughter of a sheep . . . [like ?] hay (?)

[1] KU *ka-a*. Also 80, 7, 7 (Rm. ii, 162), E. (xiii, 18) leaving it untranslated, the remedy thus made being a kind of gruel with alcohol, doubtless to produce a healthy perspiration. It must surely be "flour of *kû*", as in Hrozný (*Getr.* 64, quoting King, *Magic*, 12, 30) *banû šeam u ki-e* "creating corn and *kî*", the latter obviously parallel to "corn".

[2] *A-ḫi-nu-u, a-ḫi-en-na-a*. Various drugs *a-ḫi-en-na-a* . . . (43, 1, 9): "red wool and white wool *a-ḫi-en-na-a* NU.NU separately thou shalt spin" (10, 1, iii, 15). Various drugs *aḫinû* RAT "separately thou shalt bray" (9, 1, 34): also cf. 49, 1, ii, 6; 8, 3, 2. Del. *HWB.* 40, and E. (xiii, 8 "gleichmässig") are certainly wrong; it has been correctly derived from *aḫu* "side", but how it can mean "gleichmässig" I am at a loss to understand. Clearly the adverbial use from "side" must be "aside": the very quotations in *HWB.* show this, *mâta aḫi-ennâ nizuz* (V *R.* 1, 126) "we will divide the land into separate portions, apart" (not "unter uns"): the god Nusku repeats the word of his master to Ea *aḫi-ennâ* "aside, apart" (not "hinüber"). The passage about spinning the wool separately (contrasted with 11, 1, 1, *ešteniš*), the red and the white bound on separately to each eye, is conclusive.

[3] MUD: cf. E. xiii, 17, MUD.A.BAR = *appu abaru* a tube of *abaru*.

[4] DA.A.

[5] *Abû, apû*, applied to eyes: 10, 1, iii, 9, 21; 11, 1, 33; 12, 9, 6; *ip-pa-a*, 14, 1, 5; *tab-ba-a*, 10, 1, iii, 10; referable to Syr. ܐܒܝ *defecit, caruit, extinctus est.*

[6] *Ašû*, referable to Arab. أسى "be afflicted". Jastrow (*Trans. Coll. Phys. Philad.* 1913, 375) suggested the correct meaning, but without a Semitic comparison. It is applied to headache, *ašû* RI.RI (26, 1, 6) (i.e. *muḫḫuṣu* or *šumkutu*) "throbbing pain": cf. 6, 9, 11; 19, 4, 3, 6; to eyes (9, 1, 42; 10, 1, iii, 9, 22; 11, 1, 17, 33; 16, 1, 17, etc.). It can seize on a man (16, 4, 8; 55, 8, 4). It is applied as a description to numerous plants (*CT.* xiv, 29, K. 4566 *passim*), sumach, **calendula*, root of *arnoglosson*, licorice-juice, ***ricinus*-seed (berries), mustard, *hyoscyamus*, *galbanum, *carduus marianus*, fir-gum, pine-gum, etc., i.e. "drugs for pain". In *AM.* 16, 3, 13, *miṣu* ^{u}*ašî* in a receipt, probably = "some anodyne." 16, 4, 2: . . . ^{v}*ašî parasi(si)*, "a drug for stopping pain."

thrown away, like a cup of sour wine (vinegar) thrown away. . . . [Of these twain] Nergal between them a boundary [hath set] . . . The charm is not . . . (?)[1]: it is the charm of Ea [and Marduk] . . . [the charm of Nin-aḫa-kuddu] the mistress of charm; Gula, [quicken the] recovery, thy gift (?). Recite the charm.

(Pl. 10, 1.)

48. [Incantation for a] Sick Eye.

49. . . . as thou tiest it thou shalt recite the charm, on his eye . . .

50. [Charm.] (As in 9, 41–42.) . . . of Gula . . . "[Whom shall I send to the daughter of Anu] of Heaven,

Col. iii, 1.

that they may bring me their ewers of *ḫulalu*, their basins of bright lapis, that they may gather me the waters of the sea, the broad ocean, whereon no woman in her courses hath descended,[2] wherein no separated woman hath washed her hands, that they may cast them for me and cool the blazing fire in his eyes?"[3] The charm is not . . . (?), it is the charm of Ea and Marduk, the charm of Damu and Gula, the charm of Nin-aḫa-ḳuddu, the mistress of charm. O Gula, quicken the recovery, thy gift (?)! Recite the charm.

[1] I am still uncertain whether my old translation of this passage (*PSBA*, 1908, 149) is as far out as critics say. It is now customary to leave it untranslated. Langdon (*PBE.* xxxi, 56) gives the various forms in which it occurs, and *ia ut-tu* (which omits *un* at the end) is fairly common. But the Malay parallel in my *Semitic Magic*, xlviii, is suggestive.

[2] Variant *urrušti la uširru* (*CT.* xxiii, 3, 8).

[3] Part duplicate of my copy of SA.GAL.LA, *CT.* xxiii, 2, 5: added to by E., xiii, 135. *D(t)allu* (E. "Türverschluss" wrongly, comparing *BSGW.* 70 (1918), 5, p. 27 [1]) is a vessel, as has long been known (MA. 1159). Br. 2579, DUK (= *karpatu*). DAL = *dallu* = *naman* . . . The water is to be gathered up, and hence "door-locks" are ridiculous. *Karpatu* is the cup (or basin ?) into which the water is poured: *dallu* I take to be the modern Ar. *delleh*, a spouted copper jug, now used for coffee (see my article *JRAS.* 1923, 240, No. 15). Moreover, it varies with *ḳannu* in l. 20 below. This word is probably connected with Arab. قَنِينَة "a phial".

Ḫarištu (varying with *urrušti*, *CT.* xxiii, 3, 8) is from *ḫarâšu* (MA. 341; Del. *HWB.* 293) "hold back", i.e. the woman held back under restriction, *tabu*, Heb. עָצוּר: Ebeling translates *urrušti* "eine Schmutzige".

Musukkatu is from the similar root סכך (*HWB.* 497, originally "shut up", Arab. سَكَّ) will equally mean a woman segregated. Ebeling has understood it correctly as a menstruating woman, but his "wortl. 'sehr schlimme'" from *msk* is impossible for *musukkatu*.

Ḫarištu has a special section (V *R.* 18 *c–d*, 19–21, MA. 342), and there is even a *ḳannu ša ḫarišti* (V *R.* 15, *d* 47).

The woman and maid "with unwashed hands" (see my *Devils*, ii, 139, and *Semitic Magic*, 117 ff.) are always a source of pollution, the reference being euphemistically to menstruation.

6. Incantation for a Sick Eye.

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7. Ritual for this: this (is) for red wool, a thread thou shalt spin, tie seven knots, as thou tiest (them) recite the charm, bind on his sick eye.

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8. Charm (*as in* 9, 41–42). O failing eyes, O painful eyes, O eyes sundered by a dam of blood[1]! Why do ye fail, why do ye hurt? Why hath the dust of the river come nigh you, (or) the spathe of the date-palm whereof ye have chanced to catch the pollen which the fertilizer hath been shaking? Have I invited you, Come to me? I have not invited you, come not to me, or ever the first wind, the second wind, the third wind, the fourth wind cometh to you! Charm.

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14. Incantation for a Sick Eye.

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15. Ritual for this: red wool, white wool separately thou shalt spin: seven and seven knots thou shalt tie (in each): as thou tiest, thou shalt recite the charm; the thread of red wool thou shalt tie on his eye which is sick; the thread of white wool thou shalt tie on his eye which is whole, and he shall recover.

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17. Charm (cf. 9, 41–42).

Of these twain the daughter of Anu between them hath built a wall; the one will not move in accord with its fellow. Whom shall I send to the daughter of Anu of Heaven, that they may bring me their ewers of *ḫulalu*, their basins of bright lapis that they may gather (the waters)[2] and bring (them) to the failing eyes, the painful and troubled eyes? Recite the charm.

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23. Incantation for a Sick Eye. Ritual for this: as before.

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24. Charm. O clear eye, O doubly clear eye, O eye of clear sight! O painful[3] eye, O doubly painful eye, O eye of painful sight! A pair, they are one eye, yet a mountain is set as a bar between them . . . (on) their surface a knot is tied, (on) their under parts a wall is built. . . . What hath been their wind, what not their wind . . .? What hath been their windgust, what not their windgust?[4] Wind of the face, dimness of the face, cloud . . .

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[1] *Pur-sit dâmi šu-te-iṣ-li-pa-a-tu*, parallel to *pur-si-in-di da-a-mi šu-ḫar-ri-a-tum* on 11, 1, 33, بحر "cleave".

[2] As before, Col. iii, l. 1?

[3] See *SAI.* 8609, *eṣû*, and 8612 *ašâtu*.

[4] i.e. the wind bringing dust into the eye: cf. 11, 1, 11.

30. [Incantation] for a Sick Eye. [Ritual for this : as before.]

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31. [Charm] (*as in* 9, 41–42).

(*Pl.* 11, 1.)

1. [Ritual for this :] red wool (and) white wool thou shalt spin together, seven and seven knots thou shalt tie : as thou tiest, thou shalt recite the charm, bind on his temples and he shall recover.

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3. [Charm :] Sound [front], sound back, smitten front, smitten back : thou art a true hero, thou a weak reed.[1] Recite the charm.

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4. Incantation for a Sick Eye. Ritual for this : as before.

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5. (*Unintelligible formula.*) Recite the charm.

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6. Incantation for a Sick Eye. Ritual for this : as before.

---

7. Charm. The eye of the man is sick, the eye of the maid is sick, the eye of the man and maid who shall heal ? Thou shalt send, they shall receive. Pure palm-fibre[2] thou shalt chew[3] in thy mouth, twist with thy hand, bind the man or maid on their temples ; the eye of the man or maid will recover. Recite the charm.

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10. Incantation for a Sick Eye. Ritual for this : as before.

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11. Charm. In Heaven the wind blew and brought blindness[4] to the eye of the man : from the distant heavens the wind blew and brought blindness to the eye of the man. Unto the sick eye it brought blindness ; of this man his eye is troubled, his eye is pained. The man weepeth grievously for himself.

Of this man, his sickness Ea hath espied and (said) " take pounded roses, perform the Charm of the Deep, and bind the eye of the man ". When Ea toucheth the eye of the man with his holy hand, let the wind which hath brought woe[5] to the eye of the man go forth !

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[1] Like 9, 1, 25.

[2] KU.LIB, i.e. either the " clothes + heart " or the " flour + heart " of the palm.

[3] *Teḫipi*, lit. " destroy." There is no authority for " chew ", but it seems the obvious rendering.

[4] Better Syr. ܟܣܝܐ than ܟܣܐ " poison ", which is probably *šammu* " drug ".

[5] *Uddubu*, Heb. אדב " grieve ".

32. Incantation for a Sick Eye.

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33. [Charm.] Failing [eye], painful eye, eye pent by a dam of blood[1]; the [twain] weep before their mother Mami: ["Why hast thou tormented (?)] us, and on us bound the pain, the blood, and the wind?" Recite the charm.

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36. [Incanta]tion for a Sick Eye.

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37. [Ritual for this: red wool (and)] white wool thou shalt spin separately, fold them in the middle in a fold, bind [the red wool] on his eye which is sick, the white wool on his eye which is sound, and he shall recover.

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39. (*Charm*): Recite the charm.

---

41. Incantation: If a man's eyes are set (?).

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42. . . . a thread thou shalt spin, double it twice, tie seven knots. As thou tiest (them), thou shalt recite the charm, bind on his temples, and he shall recover.

(*Pl.* 12, 1.)

1. [Charm. O] seed-corn . . . , why hast thou borne heat to me? Like a *šuršuru*[2]-bird flying to the edge[3] of the river, in the streets it hath met me. [O . . .] chopped straw (?)[4], why hast thou borne to me a throbbing (winking) like a star, . . . me like a flame, before flint and steel reached (overcame) you? [The charm is not . . . (?)] . . . it is the charm of Silig-mulu-šar, of Marduk: it is the charm of Nin-girimma, mistress of charm . . . thou knowest and I bring. Recite the charm.

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50. [Incantation for] removing blood (?), chopped straw (?), and blindness[5] of the eyes.

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[1] *Pursindi*; the phrase obviously parallel to 10, 1, 10. *Parâsu* = "to stop, hinder."

[2] *Šuršuru*, Arab. شُرْشُور, a small, unidentified bird. *Iṣa*, עוֹף.

[3] Cf. Heb. חֹד, used of sword-edge, *Pr.* v, 4.

[4] See Del. *HWB.* 287 for quantities measured by *biltum* (not *gur*, as dates); cf. Ex. xv, 7 (inflammable). Is. xl, 24 (windborne). *Zunnani*, from *zanû* (cf. *bunnanu*) or *za'ânu*?

[5] *Nisma*, from *samû* "be blind": or *manma* "anything (?)".

51. . . .[1] a gentle (?)[2] fragrance descended on the earth . . . and thou didst make seed-corn[3] to sprout: [ripen]ing (?)[4] [brought] harvest,[5] harvest [brought] the ear, the ear [brought the threshing]. Sin [harves]ted, Shamash gathered; when Sin had harvested and [Shamash] gath[ered] . . . Shamash and Sin threshed,[6] and the chaff [spread] motes [abroad].

[Incantation for removing] chaff from the eyes.

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57. [If a man's eye] is full of . . . , lolium, flour of roast corn, in beer thou shalt knead, bind on.[7]

No. 27. *AM.* 8, 2 (K. 10495). Col. ii.

1. If ditto, *storax, *solanum . . .
2. If ditto, thou shalt dry the head of a lizard . . .

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3. Thou shalt reduce *kak ti ti lu*? . . .

---

4. If a man's eyes ditto, and . . .

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5. The brain of a *kul-tim* . . . in equal parts thou shalt mix . . .

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6. The young of a raven[8] . . .

[1] The following is a very tentative translation of a difficult passage containing several new words.

[2] *Alalu*, presumably an adj. in agreement with *bašamu*, Syr. ܒܫܡܐ. *Basamu*, properly "balsam" in Assyrian, apparently with its presumed earlier equivalence to בֹּשֶׂם "sweet smell". Are we to see in this the equivalent of אֵד, Gen. ii, 6?

[3] *Ḫabburru*. Cf. Langdon, *PSBA.* 1914, 31, "seed-corn." It must be something of the kind. Langdon, *AJSL.* xxviii, 228, "If the seed-corn be not sound, it will not produce the green shoot and create seed." From Del., *HWB.* 268, it should mean "husk". Is it בַּבְּרוֹת "corn"?

[4] [*Ḫam*]*mannu* (?), from *ḫamâmu* = *eṣidu* (MA. 323).

[5] *Kiṣru*, קְצִיר "harvest".

[6] *Išiṣaḫimma*: the Arab. شَاصَ = "rub"; שִׁיצִי = "finish". The word appears elsewhere in Assyr. (MA. 1096); the meaning "rub" appears to apply here to the method of rubbing out grain by threshing-sledges, as is done to this day in the Tigris valley. Cf. Aram. שִׁיצָנִיתָא, a kind of wheat (?). *Mirḫu* is similarly the "rubbed" thing, Heb. מֹרַח, the Arab. مَرَّحَ being to rub with oil, or clean wheat of its husks or dust. *Lilâ*, presumably from *lalû* "be full".

The passage describes the growth of the wheat which has produced the dust or chaff which has entered the sick man's eye. The gradual tracing of the history of the offender is common in Assyrian medicine (cf. the Legend of the Worm in the Tooth).

[7] First line of next tablet, i.e. K. 2500, 16, 1.

[8] Cf. 11, 2, 33.

Section of the History of Medicine.

President—Dr. ARNOLD CHAPLIN.

A Manuscript of the "Christianismi Restitutio" of Servetus,[1] placing the Discovery of the Pulmonary Circulation Anterior to 1546.

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EVER since William Wotton's "Reflections upon Ancient and Modern Learning" appeared in 1694,[2] it has been well known that the earliest *printed* description of the circulation of the blood is that of the pulmonary circulation in Servetus's "Christianismi Restitutio," printed at Vienne in 1553. This striking description has been often quoted. From time to time, however, various more or less elaborate attempts have been made to claim actual priority of discovery for others. Thus Achille Chéreau, Librarian of the Faculty of Medicine of Paris, even went so far as to assert boldly that Servetus was a mere "copiste"[3] of Realdo Colombo, whose book was not published until 1559, six years after that of Servetus. Needless to say, Chéreau could adduce no real reason to justify this extraordinary charge, based in fact on mere unwarranted assumption. Chéreau was promptly answered by Charles Dardier.[4]

[1] The best and most accessible English account of Servetus based on original documents is that by Sir William Osler, *Michael Servetus* (Oxford University Press, 1909, and *Johns Hopkins Hospital Bulletin* for January, 1910). It was read at the Johns Hopkins Hospital Historical Club on May 10, 1909, and at the Oxford Summer Extension Course on August 10, 1909. The present writer has treated the subject of Servetus in *Contributions to Medical and Biological Research, Dedicated to Sir William Osler, in Honour of his Seventieth Birthday*, N.Y., 1919, ii, 767-777, espec. 772f. A serious mistake in the printing may be corrected here: the Latin note printed in the text on p. 774 is of course Murr's own note to "illato*" and should have followed the line ending "1790 8 mai." Also the words "a. 1786 . . . illato," added later by Murr, should follow "exemplo" I find that Murr's rare reprint of the "Christianismi Restitutio" of Servetus was listed for the first and only time in the second Mess-Katalog for 1791 ("Allgemeines Verzeichniss der Bücher welche in der Frankfurter und Leipziger Michaelismesse des 1791 Jahres . . . neu gedruckt oder . . . wieder aufgelegt worden sind . . . " Leipzig, Weidmann, p. 259, under "Fertig gewordene Schriften") as "*Serveti*, Mich., Christianismi Restitutio, 8 maj., MDLIII," without stating that it was a reprint, and without naming any place, printer, publisher or agent. Probably the date of the Mess-Katalog accounts for the similar wrong date given by the *Jena Allg. Lit. Ztg.* for November 20, 1792, mentioned in my "Servetus Notes," p. 774. Both ignored the minute "1790" at the end.

[2] P. 230, quoting the "circulation" passage from a transcript supplied by Charles Bernard. The Postscript to the Preface in the second edition, 1697, explains that Bernard had obtained it from Abraham Hill, who had himself transcribed it from Bishop John Moore's transcript (now in Cambridge University Library) from the book formerly at Cassel. Mosheim's "Anderw. Versuch einer . . . Ketzergeschichte." 1748, p. 254, says that Servetus "war schon hundert Jahre verbrannt, da dieses Stück seiner Verdienste um die Arzneykunst und Naturlehre erst der Welt geoffenbaret ward. Thomas Bartholin ist, so viel ich weiss, der erste der es entdeckt hat," and in the margin he refers to "Bartholin. Anatom. reform., p. 594," but I have been unable to find in Batholinus any mention of Servetus.

[3] "Servet n'en est que le copiste souvent infidèle, parfois maladroit, toujours mystique," Chéreau's "Histoire d'un Livre, Michael Servet, Paris, 1879, p. 30, previously in *Bull. de l'Acad. de Méd. de Paris*, 1879, viii, 785.

[4] Appendix to "Michel Servet . . . par Henri Tollin, trad. par Mme. Picheral-Dardier." Paris 1879; *not* in the German original.

The similarity in the statements by Servetus and Colombo need not be denied.[1] It is also probable that Colombo and the other Italians may never have seen or even heard of the passage in the rare theological work of the heretic Servetus. But if the great idea could occur to Colombo and others independently of Servetus, why could it not occur to Servetus independently of Colombo? Here Chéreau's logic, such as it is, breaks down completely, and he finally takes refuge in a strange quotation from the theological bibliographer Schelhorn, to the effect that Servetus was practically insane, and in his own conclusion "ce n'est pas le bourreau qu'il fallait à Servet, mais bien le médecin."

Chéreau (p. 32) emphasizes the fact that Colombo's dedication of his book to Pope Paul IV (1555-59) expressly states that it had been begun many years before ("quod abhinc multos annos inchoaveram"), but there is apparently nothing at all to indicate in any way *when* the circulation passage was written, or the discovery made by him.

On the other hand, Chèreau is obliged to mention the inconvenient fact that the "Christianismi Restitutio" of Servetus was circulated in manuscript in 1546, and that some such manuscript appeared successively in the sales of the libraries of du Fay (1725), Count d'Hoym (1738) and the Duke de la Vallière (sale 1784), and that Tollin states that he himself had actually seen it. Chéreau (p. 39) says "Nous ne savons par qui il a été alors acheté, ni dans quel cabinet il est passé. Tollin assure *l'avoir vu* . . . Nous l'avons, nous, cherché en vain."

Without repeating the other references in my printed "Servetus Notes" I may say that this elusive manuscript had been officially listed in print by Delisle in 1871. Moreover, in 1878, the Rev. Alexander Gordon, the chief living authority on Servetus, printed in the *Theological Review* (July, p. 417f.) a careful collation of the circulation passage based on a transcript made by the Rev. S. A. Steinthal. In this article Mr. Gordon states that the MS. in question "contains what answers to pp. 92-247 of the printed text. Of this portion it presents no mere draft, but a copy of the prepared text, that would have been printed had Calvin returned the manuscript. Some passages which occur only in this recension are in the author's best manner. The circulation-section exhibits fewer various readings than any other part; showing that it was fully matured in the original draft from which the recensions of 1546, and that printed in 1553, were successively prepared."

This, then, is the most interesting and important manuscript, of which, after various elaborate but unsuccessful attempts, I finally succeeded in obtaining the photograph shown *for the first time* to the Royal Society of Medicine. Later I was able to examine the original in the Bibliothèque Nationale (MS. Latin 18212).

It is written on paper (ca. $7\frac{1}{2} \times 5\frac{1}{2}$ in. in size) and consists of 73 leaves or 143 pages, besides the preliminary leaf. Pages 36, 67, 70, 102 and 103 are blank. The "circulation" passage is on pp. 8ff. The dark-blue morocco binding bears the well-known arms of the famous bibliophile, Count d'Hoym. There are no other marks of ownership or indications of the history of the manuscript, except du Fay's Latin note on the fly-leaf (printed in the du Fay,

[1] Cf. Charles Singer's "The Discovery of the Circulation of the Blood," (London, Bell, 1922), p. 37f. (Classics of Scientific Method), and W. Preyer's note in Tollin's "Die Entdeckung des Blutkreislaufs durch M. Servet" (Jena 1876), p. 39 (Preyer's Samml. Physiol. Abhandl.).

&c., catalogues[1] and in De Bure's "Bibliographie Instructive"). This note states that it had formerly belonged to the Basel bookseller Cælius Horatius Curio, and that it is an autograph first draft or sketch of the "Christianismi Restitutio." Tollin ("Entdeckung," &c., 1876, p. 33) thought that the writing was that of Curio himself. Certainly it is not that of Servetus, though it was evidently transcribed in the 16th century from the 1546 draft, corresponding to the pages in manuscript in the Edinburgh incomplete copy of the original printed 1553 book. Hence it is of great importance as "setting back the date of the formulation of Servetus's discovery by at least seven years" (Gordon). It well deserves printing accurately, or better still, reproduction in facsimile.

The authorities of the Bibliothèque Nationale recently granted me the unusual privilege of carefully examining their famous copy of the original printed "Christianismi Restitutio" itself, and of comparing it with the description of it in the 1783 La Vallière catalogue. Thus I found that Colladon's manuscript "Eorum quæ in impurissimo hocce opere continentur Index" on two leaves at the end, is in fact merely a list of the headings of the books or chapters into which the printed volume is divided. The volume is marked in various places, and various passages are underlined. On a leaf facing the title-page is written in calligraphic imitation of print: "Ex Dono | Eruditissimi & Amicissimi | Doct. Ric. Mead | Magnae Brittaniae Regis | Medici Primarij. | De Boze." Apparently this has not hitherto been printed, though the La Vallière catalogue prints in full Mead's Latin note on the fly-leaf.[2]

At the Bibliothèque Nationale I saw also the Paris de Meyzieu (1779)—La Vallière copy of the suppressed 1723 London reprint of the "Christianismi Restitutio." It bears no mark of ownership whatever, but there is no reason to doubt the statement in the La Vallière catalogue that it was Mead's copy[3]—which I find described in the "Bibliotheca Meadiana," London 1755, p. 238, as: "Servetus de Trinitate, Liber partim impressus, partim scriptus, 2 vol. 4to." (sold for £8 18s. 6d.), the title-page, contents, and pp. 253-1050 (corresponding to pp. 293-734 of the original) being in MS., transcribed from the printed original.

In my printed "Servetus Notes" I stated that such documents relating to the seizure of this London reprint as I could find in the Public Record Office did not refer to Mead even indirectly. In fact, original documents there signed by Samuel Palmer and Isaac Dalton, respectively, show that Palmer printed the first five sheets only (i.e., pp. 1-20; the book being in twos, on half-sheets) from copy received from one *Gysbert Dummer* (a Dutchman), to whom he sent the sheets for correction; and from the appearance of the corrections he supposed them to have been made by one Patrick; and that Dalton printed the rest, employed by Dummer. Part of the copy was brought to Dalton from Palmer's house by Peter Paris, a Frenchman, who composed from it in Dalton's house. Dalton carried the sheets for correction to Patrick in Hand Alley. It was this Patrick who first called the attention of the authorities to the

[1] La Vallière catalogue, 1783, I, no. 912: "Forsan ipsius authoris autographus, codex hic manuscriptus, qui fuit percelebris bibliopolæ Basiliensis Cælii Horatii Curionis, uidetur prima conceptio (ulgo l'esquisse en termes de peinture ou le dessein) libri ualde fumigerati Michaelis Serueti a Joan. Caluino cum ipso Serueto combusti cui titulus: Christianismi Restitutio," &c.

[2] Mead's note really reads of course: "Vid, la lettre . . . præfixam suæ editioni" *not* as printed; and the date at the end of it should read: 1740. Thus Mead must have presented the book between 1740 and 1745, when it appears in de Boze's own rare quarto catalogue of his library.

[3] Dr. George C. Peachey kindly calls my attention to the fact that Mead certainly never used a bookplate. See his new handsome and valuable edition (London, Kimpton, 1923) of Macmichael's "Gold-headed Cane."

reprinting, perhaps as soon as he found it too heretical. He kept a school in Moor-fields then, and he is probably identical with the classical scholar and lexicographer, Samuel Patrick (1684-1748) at one time usher at Charterhouse. Thus Des Maizeaux was right when he wrote to Mosheim (l.c., p. 373), on July 30, 1730, that it was " un Hollandais " who " entreprit de faire imprimer ici un gros Recueil d'Ouvrages de Servet," though probably only a reprint of the " Christianismi Restitutio " was really contemplated.

Section of the History of Medicine.

President—Dr. ARNOLD CHAPLIN.

The British Medical Arrangements during the Waterloo Campaign.

By Colonel H. A. L. HOWELL, C.M.G., R.A.M.C. (Retired).

THE news that Napoleon had escaped from Elba, landed in France, and, with a rapidly increasing army, was marching on Paris, caused all Europe to spring to arms. The armies of the allied nations collected and threatened the invasion of France by all her frontiers. When Napoleon reached Paris he found an armed force ready to join him of about 224,000 men, which would enable him to take the field with an effective of about 155,000 [1]. He knew that his enemies could not rapidly combine against him, that the Russians and Austrians would not be ready to take the field before August. His only chance was to attack the enemy armies and defeat them in detail. His nearest opponents were the Anglo-Allied troops in Belgium, and the Prussian army based on Cologne. It was obviously necessary to deal with these first, and, if possible, by surprise. He provisioned and garrisoned the French fortresses on the Belgian border, and behind these, concentrated his army on the line Maubeuge to Philippeville, within fifteen to twenty miles of Charleroi and the Prussians [2].

Let us now consider the state of the British army under Wellington. At the end of the Peninsular War he commanded a highly efficient army, but, in 1815, this army had already been broken up. Many veteran battalions had been sent to take part in the war in America. The army had been reduced. Some infantry second battalions had been disbanded, and the remaining battalions and the cavalry regiments cut down to a peace strength. The axe was also applied to the medical department. Most of the medical staff of the hospitals were placed on the retired list, or on half-pay, while battalions lost their second assistant surgeons. The Ordnance Medical Department was also reduced.

To replenish the ranks of the regiments the militia was drawn upon, and many militiamen fought at Waterloo, still wearing militia uniform. Only the Guards and the 52nd Regiment were at full strength. Only four battalions were over 700 strong, many were under 600, and some under 400 [3]. Wellington's army therefore included a very large number of inexperienced and insufficiently trained soldiers, but they were stiffened by many veterans and the Peninsular spirit infused their ranks. Wellington's description of his army in a letter before the battle was: "I have got an infamous army, very weak and ill-equipped, and a very inexperienced staff" [4].

By June, 1815, Wellington's army numbered about 106,000, of whom 14,000 were cavalry, with 196 guns. Of these, 12,000 were garrisoning various places. About 34,000 were British, the rest being made up of the King's German Legion, Hanoverians, Brunswickers, Dutch and Belgians [5]. The 1st corps, under the Prince of Orange, had headquarters at Braine-le-Comte, a second corps, under Lord Hill, had headquarters at Ath, while a strong body of reserve troops were round Brussels, under Wellington. The cavalry were round Ninove and Grammont. This army watched the frontier from Nieuport and Ypres to Binche, just west of Charleroi [6].

The British had two lines of communication with England, one by Antwerp and the Scheldt, the other by Ostend, with water communication by canal as far as Ghent, which facilitated the carriage of supplies.

On Wellington's left, that is to the east, the Prussian army of 117,000 men, of whom 12,000 were cavalry, with 312 guns, guarded the rest of the Belgian frontier. Its headquarters were at Namur, and its line of communication ran through Liège to Maestricht and Cologne [7].

Sir Andrew Halliday, then a Surgeon to the Forces, who wrote an account of the campaign, says that Wellington's force was so placed that it could concentrate at any spot in the sphere of operations in twelve hours [8]. James says the Prussians could assemble at headquarters in twelve hours, or the whole army at any spot in twenty-four hours.

Such was the position before active hostilities began.

Let us now, for a time, leave the scene of operations and consider the British Army Medical organization of that date, and try to form some idea of how it was used in time of war. There were no Field Service Regulations in those days so our knowledge must be gathered from scattered sources, from army lists, warrants, and casual references in military and medical writings of the time [9].

In 1815, the medical services of the British Army consisted of (1) an Army Medical Department, (2) an Ordnance Medical Department, and (3) a separate establishment for the Household Troops.

The Army Medical Department consisted of some directing or administrative officers, the hospital or medical staff, and the regimental medical organizations.

During the Peninsular War, until the Walcheren campaign, the Army Medical Department was directed by a medical board consisting of the Surgeon-general, the Physician-general, and the Inspector of Hospitals. As a result of the ineptitude shown by the board in connexion with the disastrous Walcheren campaign, the board was abolished, and, in 1810, a Director-general appointed, with two principal inspectors as his assistants. John Weir was the first Director-general and held the appointment until June, 1815. Three days before Waterloo, Sir James McGrigor, who had been principal medical officer in the latter part of the Peninsular War, became Director-general, with one Principal Inspector, Sir William Franklin, as assistant. The two sat "for nearly twenty years at the same table" (McGrigor). Their office, at the time of Waterloo, was in Upper Brook Street and not at the War Office. McGrigor, perhaps the greatest of our Directors-general, had little to do with the medical arrangements at Waterloo, until after the battle. The other administrative officers were the Inspectors of Hospitals and the Deputy Inspectors of Hospitals.

The general hospitals, of which there were only five or six in England were run by a separate medical establishment, consisting of physicians, staff

surgeons, hospital assistants, purveyors, deputy purveyors, apothecaries, hospital mates and dispensers. All were commissioned officers, except the mates and dispensers. *Purveyors and deputy purveyors* had charge of the provisioning of the hospitals and kept the hospital accounts. Originally, they were better paid than staff- and regimental-surgeons and were selected from their ranks, but at the time of Waterloo they were no longer medical officers, but appointed from persons versed in accounts.

Hospital assistants were the junior physicians and surgeons in the hospitals. They had formerly been called hospital mates for general service and were commissioned. Another class of hospital mates still existed at the time of Waterloo, but these were warrant officers only and may be regarded as probationary medical officers expected "to improve themselves towards a due qualification for admission as commissioned officers" (War Office Circular). Later on, hospital assistants were designated staff assistant surgeons. *Apothecaries* were medical men selected from the assistant surgeons and hospital mates. Many were promoted to the rank of physician. They appear to have acted chiefly as clinical clerks to the physicians. *Staff surgeons* were surgeons, not belonging to regiments, who were employed on the staff of a general in the field, or in a hospital, or garrison. They were usually promoted from the ranks of hospital assistant, apothecary, and regimental surgeon. *Physicians* were the *élite* of the medical profession in the army. They were required to hold either an Oxford or Cambridge degree and to be Licentiates or Fellows of the London College of Physicians. Later, the degrees of any University in Great Britain or Ireland were acknowledged as qualifications. For a long time most of the physicians were appointed directly from the ranks of the civilian medical profession. As they were better paid than regimental and staff surgeons, who had difficulty in attaining the rank, much discontent arose. In 1811, it was directed that physicians were to be chosen from regimental and staff surgeons who possessed university degrees.

Officers of hospitals were designated as "to the forces," thus, "physician to the forces," "surgeon to the forces," "hospital assistant to the forces," "apothecary to the forces." This arose from the fact that they were originally appointed to forces in the field. When the war was over they were placed on half-pay, being called up again, if required, when a fresh war broke out. Sufficient of them were retained in peace time to officer the few general hospitals at home. Thus, in 1807, there were only seventeen physicians, and all but two were on foreign service. In the same year there were sixty surgeons to the forces, and all but seven were overseas.

The regimental medical personnel was distinct from the medical staff. Originally each cavalry regiment and infantry battalion had a surgeon; later, there was added a surgeon's mate. The surgeon held the King's commission, but the surgeon's mate, who was often a fully qualified medical man, was a warrant officer. Surgeons were usually promoted from the ranks of surgeons' mates. When the war with France broke out many regimental surgeons' mates left the regiments and went to the newly-formed hospitals as hospital mates where their pay was higher and their position better. The difficulty in obtaining regimental mates was so great that in 1796, by Royal Warrant, their title was changed to assistant surgeon, they became commissioned officers and were given better pay. As a result of war experience an extra assistant surgeon was appointed to each battalion of over 500 men. So at Waterloo we find most battalions of infantry and each cavalry regiment had a surgeon and two assistant surgeons. It may be mentioned here that medical

officers at this time held only relative rank and had no disciplinary powers over either the attendants in hospital or over their patients [9].

Wherever a regiment went, in peace or war, a regimental hospital was opened for treatment of the sick and wounded of the regiment. Barracks had small regimental hospitals. On the line of march, at the halts, a house would be commandeered by the surgeon and converted into a hospital. Sir James McGrigor describes [10] the procedure during the Peninsular War, as follows:

> "The divisions of the army, composed of from eight to fifteen or sixteen regiments, under the command of a lieutenant-general, were each of them under the medical superintendence of an inspectorial officer" [Guthrie tells us this was usually a deputy inspector of hospitals], "to whom the surgeons reported, and who regulated all the medical concerns of the division. It was his duty to see that, however short a time a battalion or a corps rested in one place, a regimental hospital was established; indeed as they carried with them medicines, bedding, stores, and all the materials of a hospital, a regiment might be said to have its hospital constantly established even on the march. It was frequently established in the face of an enemy, and nearly within the reach of his guns. When the regiment halted, after getting the men under cover in some building, and constructing chimnies, the first object was to make bedsteads, getting at the same time additional mattresses of straw, rushes, &c. It was really surprising to see with what rapidity this was done; so much were regiments in the habit of it, I found the hospitals complete in everything, and the men most comfortably lodged in a few days after a regiment had halted. In short, by making each regiment constantly keep up an establishment for itself, we could prevent the general hospitals being crowded; much severe and acute disease was treated in its early and only curable stages, and no slight wounds or ailments were ever sent off from their regiments; by which means the effective force of the army was kept up."

I may say that McGrigor carried out this method of keeping sick and wounded with the regiment against the wishes of Wellington, who was all for rapid evacuation of sick and wounded. At the same time, as McGrigor shows in his autobiography, under Peninsular conditions it answered well. On the day of battle the regimental surgeon and one assistant surgeon followed the corps into action, applied first-aid to the wounded and superintended their removal. The second assistant surgeons joined the senior surgeon of the brigade or a staff surgeon who, with them, established a collecting or dressing station for the wounded, or a field hospital [11]. Guthrie, after his long experience of war, stated that "it is quite impossible for a regimental surgeon to be out of fire, if he do his duty; and a medical staff officer can scarcely be out of cannon shot." Guthrie also tells us that, at the beginning of the Peninsular War, at the battle of Roliça, the post laid down for the regimental surgeon during the action was seven paces in rear of the colours. During sieges the surgeon's post was at the gorge of the trenches. During the assault on a fortress his post was at the foot of the breach [12].

In the old wars the artillery trains going on service had surgeons appointed to them as well as to their depot at Woolwich. As the artillery increased the ordnance medical staff was increased, thus forming under the Master-general of the Ordnance a distinct medical department. We need say little about it except that it had, when Waterloo was fought, a director-general of its own, and the other ranks were surgeon-general and inspector, assistant surgeon-general and inspector, resident surgeon, surgeon, apothecary, assistant surgeon, and second assistant surgeon. At Waterloo the ordnance medical department was represented by four surgeons, three assistant surgeons, and ten second assistant surgeons [13].

The Household Troops also had a separate medical establishment. The

senior surgeon of each regiment of Foot Guards was given the style of surgeon-major, and the other surgeons were called battalion surgeons. Each battalion had three assistant surgeons. At Waterloo only two assistant surgeons were with each battalion, and the senior surgeon (Curtis) was given the acting rank of deputy inspector of hospitals, and placed in charge of the First Division.

The principal medical officer in the Waterloo campaign was Inspector of Hospitals, afterwards Inspector-General Sir James Grant. Under him were nine deputy inspectors and one acting deputy inspector. Five of these were appointed to the medical charge of divisions, two were directing the hospitals at Brussels and Antwerp. Deputy Inspector J. R. Hume was physician to the Headquarters' Staff and personal physician to Wellington, as he had been in the Peninsula. Deputy Inspector John Gunning was designated "Surgeon-in-Chief to the Army serving in the Netherlands." Deputy Inspector Stephen Woolriche was appointed to take charge of the field duties in the event of a general action and in this capacity was principal medical officer at the battles of Quatre Bras and Waterloo. There were also nine Physicians. One was appointed to the staff of the Third Division. How the others were employed I cannot trace, but probably they were on the staff of the general and field hospitals. There were thirty-two Staff Surgeons. Some were on divisional staffs. Eleven were at Waterloo, one was at Quatre Bras; the others were, presumably, at the hospitals. Five Hospital Assistants and one Apothecary were at Waterloo; the rest of the medical staff, consisting of sixty-five hospital assistants, five hospital mates, one purveyor, seven deputy-purveyors, four apothecaries and ten dispensers, were on duty in the hospitals [14].

The British army at this time had no ambulance system corresponding with that in the French army, where Larrey had introduced his "flying ambulances" for the rapid removal of the wounded, and Baron Percy had later formed a corps of surgeons and trained stretcher-bearers, with waggons very much on the lines of a modern field ambulance. In our army when a man was wounded his comrades removed him to the rear, or, if the action was brisk, he lay wounded on the ground till the fight was over. It is estimated that at one period of the Battle of Waterloo 10,000 men were absent, having gone to the rear with wounded. No regularly trained men were provided for the removal of the wounded or to assist the surgeons either on the field or in the hospitals. Wounded were removed from the field by the bandsmen or by their comrades to the first line of surgical assistance and afterwards transported to the hospitals of the second line, field hospitals, or to the general hospitals in return carts of the commissariat, in hired country carts or in spring waggons. At this time the wheeled transport of the army was managed by a corps designated the Royal Waggon Train. It included a number of spring waggons for the carriage of the sick and wounded. Two of these were supposed to be detailed to each corps. These waggons were four-horsed, could carry only two wounded lying down and seven or eight sitting up, and were very heavy and so broad that they blocked the roads. The stretchers in use consisted of a pair of poles with canvas between. They had no traverses [15]. Each regimental and staff surgeon was allowed a horse or mule with a pair of field panniers and a pack-saddle for the carriage of his field medical stores [16]. From a reference in Guthrie's "Commentaries," I gather that there were depots of medical stores on the lines of communication from which the hospital and regimental medical supplies were obtained. The attendants in the hospitals were not trained men. They were men from the regiments, detailed as occasion arose

for duty in the hospitals. While so employed they were called hospital orderlies.

To sum up the medical position when the fighting began: With each cavalry regiment and infantry battalion was a surgeon and two assistant surgeons; with the headquarters' staff, on the lines of communication, in field and general hospitals were the medical staff. The hospitals were not yet ready, but were in process of formation. The General Orders of Wellington note day by day, until just before Waterloo, the arrival of additions to the medical staff of the hospitals. McGrigor hurried more over from England after the battle. The fact is Napoleon struck before our army was ready.

Let us now return to the field of operations. Napoleon's concentration being completed, on the morning of June 15 he crossed the Sambre at Charleroi. Ziethen's Prussian Corps was opposed to him and fell back fighting on Ligny, where the Prussian Army was collecting. On the same day Wellington issued orders for the concentration of his army at Quatre Bras. This place was held by 10,000 Dutch-Belgian troops. Many of Wellington's troops had a long way to march, and the result was that during the next day, when the battle occurred, regiments arrived at intervals one after the other. His cavalry was late in arriving. As part of his attack on Ligny, Napoleon sent Marshal Ney with 47,000 men and 120 guns to occupy Quatre Bras, and then turn on the Prussian right flank. The fight began at 2 p.m. on June 16 [17]. Ney's troops greatly outnumbered the Allies at first, but, during the afternoon and evening more and more British troops arrived, and, in the evening Wellington was able to take the offensive and drove Ney back to Frasnes. The French had the superiority in guns and cavalry and made most effective use of them during the day. The British-Allied loss in killed and wounded was about 5,000, one half of whom were British. Houses near the cross-roads were used as dressing and collecting stations for the wounded, especially an inn, with a yard and outbuildings at the north-east corner. At one time the French cavalry charged through Quatre Bras and got into the inn-yard where they killed some stragglers and wounded. Assistant Surgeon Stewart, of the 92nd, who was dressing a man behind a house, had his bonnet cut in two and a lance run into his side [18]. Dr. Denecke, physician to the staff of the Third Division, was also wounded and his horse too. Our troops bivouacked for the night at Quatre Bras. In the meantime Napoleon had attacked and defeated the Prussians at Ligny with heavy loss, and the Prussians had retreated, not, as Napoleon expected, along their line of communication to Namur, but northwards to Wavre. Wellington now decided to retire along the Quatre Bras road through Genappe to Waterloo, and issued orders for the wounded to be collected and evacuated on this road. An officer of the artillery [19], who was present, wrote: "We left the enemy nothing but our killed—our wounded we brought off on cavalry horses, except such as could not be found in the standing corn." An officer of the 92nd [20], who was wounded, says he was carried in a waggon, but the motion was so painful that he got out and walked to Genappe and afterwards to Brussels. The same day, June 17, "waggons filled with wounded began to arrive in Brussels. The streets were filled with the most pitiable sights. Numbers of wounded who were able to walk were wandering on every road" [21]. The day was sultry and in the evening a thunderstorm with very heavy rain came on. Genappe was that night full of wounded. The retirement was leisurely; the British and Allied army completed its march and bivouacked for the night at Waterloo, on the position it was to hold next day.

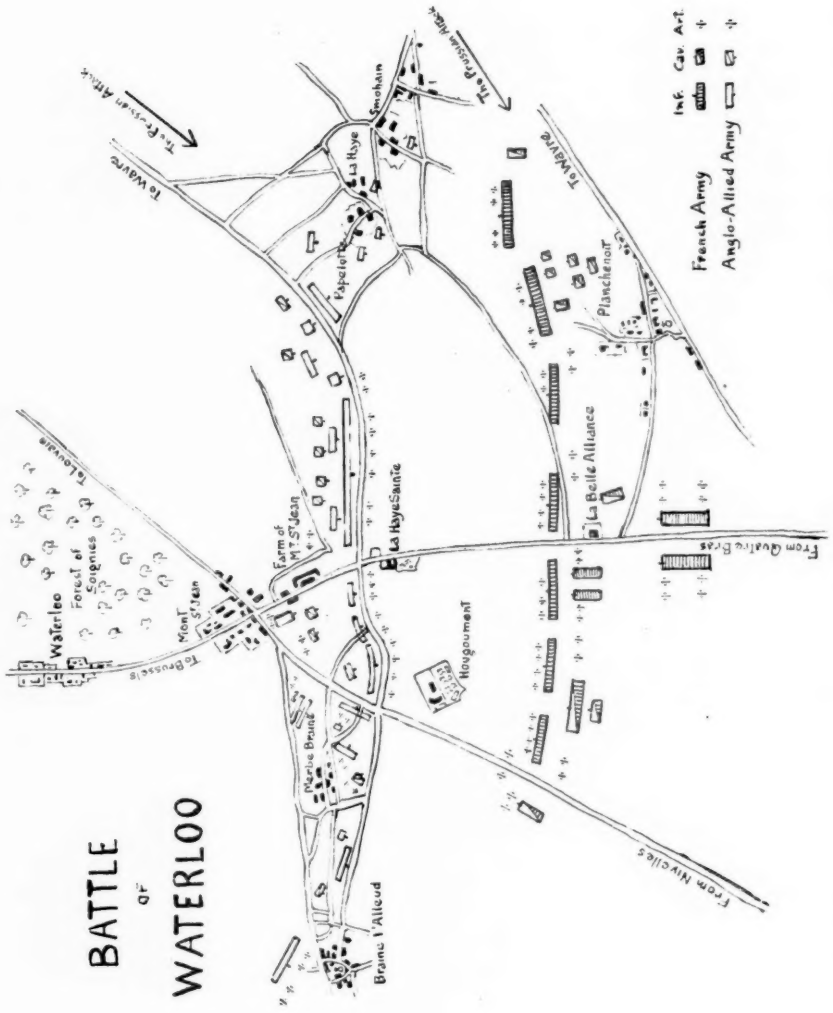
The morning of Waterloo showed the rival forces drawn up, each along a ridge astride the Genappe-Waterloo road with low ground between them (see Map p. 46). The ridge occupied by the British was traversed by a road which facilitated movement. In front of the right of the Allied position was the château and farm of Hougoumont, in front of the centre was another farm, La Haye Sainte. Both were held by Allied troops. At the centre of the French line was another farm, La Belle Alliance. Either owing to the heavy rain making the ground soft and hindering movement, or from lack of artillery ammunition, Napoleon delayed the fight until just before noon. Napoleon had more guns and the battle began with artillery fire and a French attack on Hougoumont. This was to have been only a preliminary of the battle but the defence was so obstinate throughout the day that a large part of the French infantry was kept engaged there and the château was still uncaptured. During the fighting at Hougoumont the wounded were placed in the chapel and in a barn. These were set on fire and some of the wounded were burnt in the barn [22]. Seventeen hundred killed and wounded were afterwards collected within the enclosure at Hougoumont. At 1.30 p.m. a vigorous attack was made on the British left between La Haye Sainte and Papelotte. The fighting was severe and the French repulsed with heavy loss. From 4 to 6 p.m. Ney directed assaults against the British right centre, west of the Genappe road. The Prussians now began to come up on the French right, and Napoleon was obliged to detach troops of the Guard to meet them. Ney's cavalry charges had little effect against the British infantry squares. Between 7 and 8 o'clock Napoleon ordered a general assault on the British position. In spite of the desperate valour of the Old Guard the French were driven back, Wellington's army advanced in turn to the attack and the battle was over. The Prussians took up the pursuit of the retreating French. Wellington's army bivouacked for the night on the field.

The Anglo-Allied army at Waterloo numbered 49,700 infantry, 12,400 cavalry, 156 guns: the British being 15,200 infantry, 5,800 cavalry, and seventy-eight guns. But 18,000 men were at Hal and Tubize, eleven and eight miles from Waterloo, guarding Wellington's right flank and these did not take part in the battle. Absent, sick, and wounded were shown as 3,240 men, but 1,000 of these were present at the battle. The French had 52,600 infantry, 14,900 cavalry and 266 guns.

The Allied casualties amounted to about 13,000, the Prussian 7,000, and the French 30,000. The French losses in the week between June 15 and 22 numbered 55,000, including 7,500 prisoners, or 43 per cent. of Napoleon's army [23].

The British, including the King's German Legion, had 1,759 killed. The wounded were about 6,000. Of these, 856 afterwards died of their wounds. The missing numbered 1,600, most of whom had left the battlefield with wounded, and afterwards rejoined, but 353 men were not afterwards heard of.

As to the nature of the wounds: sword and lance wounds were practically confined to the cavalry and were much more frequent amongst the French wounded. All suffered from gunshot. The British troops were at first exposed to the French artillery fire. Wellington retired then to the reverse slope of the hill only bringing them forward as required. Even while waiting in reserve on the other side of the hill the French cannon balls and shells did great damage among our ranks. Now, as to the situation of, and the work done by, the medical officers during the action, Assistant Surgeon Gibney, 15th Hussars, wrote [24]:—


BATTLE
OF
WATERLOO
French Army
Anglo-Allied Army
Inf. Cav. Art.
The Prussian Attack
The Prussian Attack
To Wavre
To Wavre
Smohain
La Haye
Papelotte
Planchenoit
La Belle Alliance
La Haye Sainte
Farm of Mt St Jean
Mont St Jean
Waterloo
Forest of Soignies
To Bruxelles
To Louvain
Hougoumont
Marbe Brune
Braine l'Alleud
From Nivelles
From Quatre Bras

"At first the medical officers remained at the positions occupied by the regiments at the beginning of the battle and the wounded found their own way back to them. As these positions were exposed to the enemy's fire they were directed to take up their quarters in the village of Mt. St. Jean. They found the village full of wounded."

The inhabitants had deserted the villages of Mt. St. Jean and Waterloo, behind our line, and, after the battle, the houses were taken over by the surgeons, and wounded officers were carried to them, each regimental surgeon keeping his own wounded together. Other villages and hamlets in the neighbourhood were also filled with wounded, for instance, the wounded of the 52nd regiment were taken to Merbe Braine as well as to Waterloo [25]. About one-third of a mile behind the fighting line was the farm of Mt. St. Jean, and this, according to Cotton, "was the chief hospital or the head-quarters for the medical staff." Simmonds [26] says "the farm of Mt. St. Jean was the collecting station for the wounded." An eye-witness stated that "the three rooms in the lower part of the house, nay, even the stables and cow-houses were filled with wounded British officers." A cavalry officer [27], visiting the field next day, wrote that

"the wounded have remained all night on the spot where they were hit, and from their number and want of means of immediately carrying them away, I fear some will have to remain out a second night. The weather is now fine and if they are dressed and supplied with a little water and bread, they will not take any injury. Many of the enemy must remain out a second night, and some a third."

As a fact the last British wounded were not removed from the field until the Thursday following the battle and, according to Sir Charles Bell, the French wounded were still being collected from the woods at the beginning of July.

An artillery officer says that, before the battle, Wellington cleared the road to Brussels, but many writers say that, after the action, the road was choked with scattered baggage, broken waggons, and dead horses. It was impossible for the wounded to be brought from the field.

"The way was lined with unfortunate men who had crept from the field. In every village and hamlet for thirty miles round wounded soldiers were found wandering; the wounded Belgic and Dutch stragglers exerting themselves to reach their own homes" [28].

The day after the battle most regiments sent out strong fatigue parties to collect the wounded and get them under shelter in care of their surgeons. Captain Hay, of the 12th Light Dragoons, says the wounded men were moved to the sides of the roads. At Waterloo

"the wounded were in hundreds without any covering, lying on every spare piece of ground. These the medical men were attending. The medical officers were quite worn out with the incessant duty they had been called upon to perform for three successive nights, and still there appeared no end to their toil."

At Brussels he found a wounded officer of his regiment who had not yet been seen by any medical officer. He took him to a large convent where every space of ground was covered with wounded soldiers. "In one long broad room, with a table down the centre, were lying twenty or thirty poor fellows, under the operations of the doctors' knives." The people of Brussels rose to the occasion and threw open their houses to the wounded, their doors being inscribed with the number of wounded taken in. Carriages were sent to the battlefield with refreshments for the wounded and to bring back wounded soldiers. Captain Hay says also that, on the 23rd, he saw "several patrols

of Prussians shooting their own and the French wounded who were beyond recovery" [29]. On the third day after the battle a field hospital was opened at Mt. St. Jean. Wellington's army had pushed on towards Paris, taking with it a large part of its medical personnel. Later on, when things had been got into better shape at Brussels and Antwerp, a large number of the hospital staff were sent after the army to Paris.

On June 20 Wellington issued a general order which ran :—

"In order to preserve order, and to provide for attendance in the hospitals at Brussels, the Commander of the Forces desires that one officer, one N.C.O., and three private men, for 100 sent to hospital, wounded in the late actions of the 16th and 18th instant, may be sent from the several regiments to Bruxelles to-morrow, and place themselves under the orders of the commandant there. No regiment need send officers and men for more than 100 men, and in case any regiment has not sent more than fifty men to the hospitals, such regiments will send only one non-commissioned officer and two men to take charge of them." — (Wellington's Dispatches.)

The best account of the arrangements for the care of the wounded after Waterloo is contained in John Thomson's "Report of Observations made in the British Military Hospitals in Belgium, after the Battle of Waterloo," published in Edinburgh in 1816. The report was made at the instance of the Director-general. Thomson was a Professor of Surgery in the Edinburgh College of Surgeons and Regius Professor of Military Surgery in the University of Edinburgh. He had formerly been a surgeon to the forces. Accompanied by the principal medical officer in Scotland, Dr. Somerville, he arrived in Brussels on July 8. The hospitals there were under the direction of Deputy-Inspectors of Hospitals Gunning and McNiel. He says that although many of the wounded had been left on the field for days they did not appear to have suffered in any respect from the exposure. The French wounded were sent, some to Brussels, others to Termonde, but the greater part to Antwerp. Six general hospitals were established in Brussels and found accommodation for 2,500 wounded. These were known as the Jesuits', Elizabeth, Annonciate, Orpheline, Nôtre Dame, and Gens d'Armerie Hospitals ; the last being set aside for French prisoners. Remittent and intermittent fevers were common amongst the French. At Termonde Dr. Perkins, a British medical officer, was in charge, and had under his care 250 French wounded. A double tertian fever was very common.

At Antwerp there were five hospitals, the Minimes, Facon, Augustines, Hôtel du Nord, and the Corderie. The Corderie was a large building intended as a rope-walk for the naval arsenal. It was fitted up for the French wounded, and was nearly a quarter of a mile long, with accommodation for 1,000 wounded. Other French wounded were placed in the Jesuits' Church, and in a suite of public rooms known as the Sodalité. The French wounded at Antwerp were under the care of the surgeons of the town, superintended by a few British medical officers. Deputy-Inspector-of-Hospitals Higgins was the principal medical officer, and was greatly praised for his arrangements. The wounded in Antwerp, however, did not do so well as those at Brussels, hospital gangrene being only too common. Remittent and intermittent fevers were also frequent. This was attributed to the low-lying situation of Antwerp. Thomson and Somerville were of opinion that "hospital sore" or gangrene was a disease endemic in Belgium. By August 7, owing to the numbers who had recovered or had been sent to England, three of the hospitals in Brussels had been closed and all the English wounded in Antwerp collected in the Facon. French

convalescents had been evacuated to Dunkirk, so that only 400 remained in the Corderie. Hospital gangrene was now common in all the wards. It was not of severe type, and in Antwerp was associated with little constitutional disturbance. Thomson did not think it contagious or conveyable to one case from another. Incised wounds were most common amongst the French, the head and shoulders being most frequently affected. The French surgeons packed the incised wounds with lint, and thus healing was by granulation; the English drew the edges of the wounds together by means of adhesive plasters. Stitching, we are told, was at this time almost abandoned. Gunshot wounds were, however, the most common. Amputation had been performed on the field both by English and French surgeons during the battles of June 16 and 18. Altogether some 500 amputations were performed after the battles, one-third being carried out before fever had set in. Thomson says the mortality depended a great deal upon the time at which amputation was done—the delayed operation being most fatal. In a return, I find that the amputations on British wounded numbered 216.

The wards in the hospitals were described as large, airy, and well-ventilated, and there was an abundance of good food. As far as possible cases were collected in separate wards according to the class of wound from which they suffered.

Guthrie, the greatest surgeon the Peninsular War had produced, and who was afterwards three times President of the Royal College of Surgeons, was on half-pay when Waterloo was fought. He offered to go to Belgium for three months but was refused, the D.G. wanting to employ him for six months. Ultimately, he went to Brussels on his own account and acted as consulting surgeon. He did brilliant work. He amputated at the hip, successfully, removed a bullet from the bladder, and tied an artery in the leg through the calf muscles. He was the first surgeon to use the lithotrite.

Sir Charles Bell, also acted as a voluntary consultant. He was in Brussels from June 29 till July 9. His letters, published in 1870, by his wife, contain vivid descriptions of his work in Brussels. Writing on July 1, he says: "It was thought we were prepared for a great battle, yet, there we are, eleven days after it, only making arrangements for the reception of the wounded." He found the French wounded the most neglected and took on the performance of all the capital operations on the wounded French. For three days he operated from 6 a.m. till 7 p.m. He was a clever artist and made sketches of what he saw at Brussels. Some of these sketches, presented by his widow, are now in the Royal Army Medical College.

Of the many who recorded their memories of Waterloo most refer with praise and respect for the work done by the surgeons. The most scathing things were said by old Peninsular veteran surgeons, like Guthrie, who declared that nothing could have been worse than the state of the hospitals when he arrived. There appears to have been a lack of dressings. Lieut.-Colonel Tomkinson, speaking of the efforts of the ladies of Brussels to supply old linen for the wounded says:—

"To judge of this assistance we should attend a large hospital after a general action, where, for want of linen, the same is used week after week and the very bandages put round wounds to cure them are doing more injury than good. Many surgeons came from England for the practice, and on things being arranged, no wounded could be better attended to.

"The wounded of a British Army generally receive more attention than those of other nations. The French system is to run great risks with a man's life in hopes of

saving a limb, from knowing that a soldier without leg or arm is incapable of service, and probably a burden to the State. With us, the practice is possibly too much in favour of hasty operation."

Army surgery was, however, admitted to be years ahead of that in civil life, owing to the great opportunities afforded the surgeon during the Peninsular War [30].

Wounded not likely to return to duty were sent to England, via Ostend, in August and September. In November, orders were issued for all British troops to evacuate Ostend, Brussels, and Antwerp, except a battalion which was left to take care of the hospital. Many wounded remained in Brussels until early in 1816. The remaining British wounded appear to have been then sent to the York General Hospital, then standing where part of Eaton Square now is, and there Guthrie had two wards and lectured for two years on the cases. To this hospital young medical officers were sent for instruction in their medical and surgical duties.

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Section of the History of Medicine.

President—Dr. ARNOLD CHAPLIN.

Egyptian Medicine under the Copts in the early centuries of the Christian Era.

By WARREN R. DAWSON.

WITH the passing of Nectanebo II, the last native Pharaoh, ancient Egypt, in its most characteristic sense, came to an end. In the last six centuries before the Christian era the country passed successively under Persian, Greek and Roman dominion. Thus, in these centuries, a strong obtrusion of alien forces and ideas rapidly spread over the face of a country whose social, political and religious traditions had remained, in all essentials, unbroken since their inception more than three thousand years earlier. This sudden cleavage with old ideas, imposed by peoples of quite different mentality from the Egyptians, naturally paved the way for the acceptance of Christianity, and as the foreign influence increased, so the hold of the old customs and religion weakened. Greek elements pervaded the old national gods, and the religion of Ptolemaic times, although outwardly the same as ever, was in reality undergoing a profound change. It was during this period of atrophy that the descriptions of Herodotus and other classical historians were drawn up. They often give us an erroneous impression of the Egyptians, which has only been removed during the last century, by Champollion's recovery of the lost language of the hieroglyphs—a discovery which has put us into touch with first-hand information from the contemporary native documents.

When at length the inhabitants of the Nile Valley forsook the religion of their forefathers and embraced the Christian faith, we know them no more as Egyptians, but as *Copts*. The history of Christianity as the national religion of Egypt is of short duration—some two and a half centuries only—from the Edict of Theodosius, in A.D. 381 to the Mohammedan conquest in 640; although the faith has survived amongst part of the population to this day. The literature of this period and of the next few centuries is very largely religious, purely secular compositions being quite rare. The language in which these works are written—Coptic—is the last stage of derivation from the ancient language of the hieroglyphs. It is written in Greek characters (with a few signs derived from Egyptian *demotic* to express sounds unknown in Greek) and containing a large admixture of Greek words.

Amongst their rare non-religious books, some fragments of medical literature have come down to us from the Copts. Our principal source of information is a fine papyrus discovered in 1892, at Meshaikh, near the ancient Lepidotonpolis, and now in the French archæological institute at Cairo. This papyrus is in good condition, except at the beginning, and measures about 2½ metres in length. It contains 237 prescriptions, and was written about the ninth

century A.D. The document has recently been published *in extenso* with a facsimile, translation, commentary, and a valuable introduction, by M. Emile Chassinat.[1] In the library of the Vatican are two leaves of parchment, which once formed part of a book; these contain forty-five prescriptions.[2] A third medical papyrus was discovered in 1887, at Deir-el-Abiad, and published by Bouriant;[3] this is likewise only a fragment, and has eleven prescriptions. The remainder of our material is a series of tiny scraps now dispersed amongst several collections, i.e., the British Museum,[4] the John Rylands Library at Manchester,[5] and the Berlin Museum.[6]

This collection of texts, small as it is, provides us with sufficient material with which to study the continuation of medical practice from the ancient sources of Pharaonic times after Greek, Arabic and other influences had appeared on the scene. The medical knowledge of the ancient Egyptians was considerable, although magic played a prominent, and in fact predominating, part in it as is evident from a study of the Ebers, Hearst, Kahun, Berlin and other medical papyri. They had, moreover, some knowledge of practical surgery, as the Edwin Smith papyrus demonstrates. We must now inquire the extent to which the Copts adopted or modified their inheritance of medical knowledge.

In its general form the Cairo papyrus is similar to the ancient papyri. That is to say, it is a varied and unsystematic collection of prescriptions for the treatment of affections of various parts of the body, and it further resembles them in that there are generally many duplicate remedies prescribed for the same complaint. We are, however, better able to understand it than the Ebers and other ancient papyri, for the Coptic language never completely died out and is far less obscure than the ancient Egyptian. We are thus able to identify practically all the drugs, whereas in Ebers the great majority are unintelligible to us, and comparatively few can be identified with certainty. Moreover, the Coptic papyri contain a large number of Greek and Arabic words which still further aids their intelligibility. The same applies to the maladies. Those described in Ebers are often only to be understood, if at all, from the context, as the exact translations of the words themselves are unknown to us. A great deal of light will doubtless be thrown on the obscurities of the ancient papyri when they are re-studied in conjunction with their Coptic and other derivatives, but up to the present no scholar has undertaken this task.

We will now glance over the Coptic literature itself. In the following remarks the Cairo papyrus is always referred to, except when specific mention is made of the other documents.

Of the 237 prescriptions, nearly half deal with affections of the eyes. The great majority of these are merely indicated as "Ointment for the eyes," "Collyrium for the eyes," &c., no specific malady being named, although in many cases implied. Others again are for cataract (13 prescriptions), dimness of vision (18), amaurosis (2), abscesses (3), inflammation (3), trachoma (13), &c. Most of these prescriptions differ but little from those for which no specific malady is named, and many bear a great resemblance to those set out in Ebers. A few specimens may be given:—

[1] *Un Papyrus Médical Copte*; Cairo Inst. Français d'Archæologie Orientale, *Mémoires*, t. xxxii, 1921.

[2] Zoëga: *Catalogus Codicum Copticorum*, pp. 626-630.

[3] *Fragment d'un livre de Médecine en Copte thébain* in the *Comptes Rendus de l'Acad. des Inscr. et Belles-Lettres*, t. xv (1887), pp. 374 *et seq.*

[4] W. E. Crum: *Catalogue of Coptic Manuscripts in the British Museum*, London, 1905, pp. 255-6.

[5] W. E. Crum: *Catalogue of Coptic Manuscripts in the John Rylands Library*, Manchester, 1909, pp. 52-60. H. R. Hall: *Coptic and Greek Texts*, pp. 64-66.

[6] *Koptische Urkunden aus den Kgl. Mus.*, Berlin, t. i, pp. 24-25.

No. 11.—A good powder for the eyes. Armenian borax, 10 obols; white lead, 2 ob.; pepper, 1 ob.; ginger, 1 ob.; verdigris, ⅔ ob.; starch, 2 ob.; sal-ammoniac, 1 ob. Pound them well, strain through a fine sieve; apply them to the eyes which are dim, they will become quite clear.

No. 41.—A good powder for all the maladies of the eyes. Saffron, ½ drachma; *mahrematini*,[1] 1½ dr.; collyrium of Lycium (? antimony), 1 dr.; acacia, 1 dr.; myrrh, 1 dr.; aloes, 1 dr.; opium, 1 dr.; dried ox-gall, ½ dr. Make into a powder and employ for all maladies of the eyes.

No. 42.—Cataract and film. Goat's gall and woman's milk; apply them.

Human milk, and especially the milk of a woman who has borne a male child, is frequently prescribed in Ebers and other ancient papyri.

No. 83.—Powder for all affections of the eyes. Burnt bronze, 8 dr.; schist, 8 dr. Pound them, make them into a powder, and use for the eyes morning and night.

The two ingredients of this prescription are borrowed from Greek medicine. Burnt bronze (*χάλκος κεκαυμενός*) and schist (*λίθος σχισθείς*) are frequent in Greek medical papyri.[2]

Ointments to prevent the growth of the eyelashes after they have been pulled out in cases of trachoma are numerous both in this and the ancient papyri. In a country like Egypt, where eye troubles abound, it is natural that they should occupy a prominent place in the medical works. We will now pass on to some other cases.

No. 114 is an infallible cure for earache. It works so quickly that the physician is advised to collect his fee beforehand!

Ear which suffers acutely. Opium, calf's fat, milk: melt them down together, warm them, and apply to the ear. The pain will stop instantly. But do not administer this remedy to a man until you have received your fee.

There are only two other prescriptions for the ears in the papyrus: one of them uses hyssop, and the other gum ammoniac dissolved in the milk of a woman who has borne a male child. (Nos. 173 and 206.)

Four remedies for the teeth are given, one for "teeth which suffer," is a mixture of red and yellow vitriol and alum (No. 130), another is a powder (No. 178), and the other two refer to extraction.

No. 151.—A tooth which has to be extracted with forceps (lit. iron): hellebore of good quality and gall; apply on the region of the cheek where the molar which you wish to extract is, you will be astonished!

In No. 184, in order to prepare a tooth for extraction, a preparation of the juices of various plants, including acacia and wild rue, is to be put on the roots of the tooth and left there for a moment. "Then take the tooth between your finger and thumb; it will come out quickly." A considerable number of medicaments for the gums are prescribed, but the two formulæ dealing with extractions are of interest, because there is no evidence in any mummies of the Pharaonic period yet examined, of any operative treatment for the teeth, although dental caries and alveolar abscesses were very common.[3]

In contrast with the ancient papyri, our manuscript does not contain many remedies for maladies of the stomach and intestines. We find prescriptions for pains in the stomach, for wind, bile, sickness, and for emetics and purgatives. Intestinal worms, again, feature very largely in the older papyri, but have only

[1] An unidentified drug. The word appears to be of Arabic origin.
[2] e.g. *Tebtunis Papyri* (2-3 century A.D.), No. 273 (pt. ii, pp. 22-23).
[3] See Elliot Smith and Dawson: *Egyptian Mummies*, London, 1924, p. 159.

five remedies devoted to them in the Coptic papyrus. No. 110 gives two prescriptions in which blue vitriol is used, one a draught, the other a suppository. No. 111 is curious :—

"Another: lettuce seed pounded: make the patient drink it in warm water, he will vomit the worms."

No. 166.—"Anyone who has worms in him. Camomile, mastic, rue, wine: pound them, mix with wine and let him drink of it."

In the Ebers papyrus a considerable amount of attention is devoted to diseases of women, and the Kahun medical papyrus deals solely with this topic, but only four prescriptions are to be found in the Cairo manuscript (Nos. 24, 123-125). Three of these deal with pain in the womb, and the fourth with closing of the womb. The remedies are applied as pessaries or by direct application with a piece of wool dipped in the preparation, and in one case the patient has to sit over the mixture, a method often prescribed in the Kahun papyrus. The Deir-el-Abiad papyrus has prescriptions for pains in the breasts.

Two prescriptions are for an infantile malady, apparently hernia (Nos. 230-231).

The skin and scalp affections with which our papyrus deals include melanoderma (22), herpes (158, 164), itch and lichen. One or two instances may be given :—

No. 38.—Head of a small child affected with itch. Sycamore latex and Ethiopian salve: apply; the mischief will cease.

Sycamore "milk" or latex is often employed in the ancient papyri.

No 60.—Head affected with itch. Cress seed, mustard pounded with vinegar. Apply.

No. 61.—Another: Acacia flowers and white of egg. Apply.

No. 127.—Purulent itch (or scab): Roast some fennel, grind it well with bitter vinegar until it has suitable consistence: anoint the patient with it. Leave it to dry, then wash off with warm water: the affection will cease. If you also apply this remedy to a creeping ulcer, it will dry it up.

A considerable number of prescriptions is given without mention of application to any particular part of the body. Thus we have ointments, salves, emollients, caustics (139, 147) and hæmostatics. There are also prescriptions for abscesses and boils, sores, cuts and bites, ulcers, gangrene, scars, swellings, &c. There is no mention in the Cairo papyrus of such maladies as fever or insomnia, but cummin, ground with moistened rue, taken for three days, is prescribed for fever in one of the Rylands fragments,[1] and one of the British Museum papyri has a prescription for inducing sleep.[2]

No. 139.—Powder which cauterizes quickly: Iris, 4 dr.; vetch, 4 dr.; flakes of copper, 2 dr.; aristolochia,[3] 4 dr.: roasted resin, 16 dr.; incense, 2 dr.; white vitriol, 20 dr. Pound them well, make into a powder: apply.

No. 115.—Plaster for any pain: Pine resin, 1 lb.; verdigris, 1 dr.; calf's marrow, 16 dr.; pork fat, 2 dr.; a sufficient quantity of vinegar. Cook them in a new pot: apply; the pains will disappear.

No. 197.—Ulcer, which spreads and remains open: Wax, 1 oz.; santal, 2 oz.; unsalted pig fat, 1 oz.; decoction of cut-open mouse, 1 oz. Cook them together: employ them for the ulcers.

[1] Crum: *Cat. Coptic Manuscr. John Rylands Library*, p. 52.
[2] Crum: *Cat. Coptic Manuscr. Brit. Mus.*, p. 255.
[3] Dioscorides, lib. iii, cap. v.

The medicinal use of the mouse from prehistoric to modern times I have already dealt with elsewhere.[1]

No. 144.—Gangrene: Orpiment, 4 dr.; realgar, 4 dr.; white vitriol, 4 dr.; quicklime, fine honey. Use for it, and it will cure.

No. 25.—In order to open an abscess: Blue vitriol, 4 dr.; white vitriol, 4 dr.; flakes of copper, 4 dr.; gum, 4 dr.; diphryges,[2] 4 dr.: pound them with vinegar; make into a dusting-powder, apply it and it (the abscess) will open.

No. 187.—White plaster for treating persistent wounds, and bites of dogs or men; it is very good: Burnt lead, 8 oz.; wax, 2 lb.; oil, 1 lb.; pine resin, 2 lb.; sweet wine, 10 measures; melt them over the fire; apply.

Human bites were thought by the ancients to be very serious. The Ebers and Hearst papyri devote several prescriptions to their treatment. Oribasius has a special chapter on the subject[3]; Pliny and the mediaeval medical writers also refer to it.

The above examples will give some idea of method of treatment employed by the Copts. The papyrus, in addition to the subjects already indicated, provides prescriptions for the urinary and excretory organs, and for the spleen, but contains nothing for such important parts of the body as, for example, the heart and lungs.

With regard to the *materia medica*, Greek and Arabic influences are manifest. The ingredients are, generally speaking, appropriate and rational; and though the book is by no means purged of magic, this occupies a secondary instead of a primary place as in the ancient medical papyri. We do not find many of such bizarre elements as the brain of a silurus-fish, the blood of a bat, the dung of a lizard or the fat of a snake: the ancient papyri abound with such materials. The mouse, however, has survived, likewise the goat, the wolf, the hyæna, and other animals. One prescription mentions the urine of a bat, and another the brain of the otter, but the greater number of the medicaments are of mineral or vegetable origin. Very many of them are borrowed from the Greeks, and are written in the papyrus with their Greek names.

Spells and incantations are entirely absent from the Cairo papyrus, although they occur in other Coptic medical writings.[4] The patient has generally to rely on the treatment prescribed by the physician without magical or divine aid; to a very few prescriptions the words are added "he will be cured, if it please God," or "he will recover by the power of God."

The author of this papyrus was, perhaps, discriminating in his choice of the recipes he wrote down from the various traditional sources from which he must have gathered them. One or two of them appear to be original, for instance one of his remedies (No. 117) he calls "a great remedy on which I have worked myself, with my father," and again (No. 122) he says "a great collyrium on which I have worked with my father, great is its virtue." The prescription No. 65, a comprehensive remedy for diseases of the spleen, for a crooked body or limbs, for gravel, for calculi, and to promote menstruation, is stated to be a cure "which we found written in the books of the ancients." I can find no trace of Hippocratic medicine.

The book is a collection of prescriptions solely and provides no information

[1] W. R. Dawson: *The Mouse in Egyptian and Later Medicine* in the *Journ. Egypt. Arch.*, x, pt. ii, 1924.

[2] Dioscorides, lib. v, cap. 119; Pliny, *Nat. Hist.*, lib. xxxiv, cap. 37.

[3] Πρὸς ἀνθρωποδήκτους, *Euporistes* III, 71.

[4] e.g., W. E. Crum: *Cat. Coptic Manuscr. John Rylands Lib.*, pp. 52-55.

as to diagnosis (the diagnosis is always assumed) nor any clinical or surgical directions. Prescription No. 70, which deals with black bile, is very similar to, and manifestly influenced by Avicenna,[1] Dioscorides[2] and Alexander Trallianus.[3] Evident as are the traces of Greek influence, the author of the papyrus did not apparently have any actual Greek writers before his eyes, or if so, he did not slavishly copy them. He was not a mere scribe, but was a practical physician, for his own comments are sometimes added to the prescriptions he copied. Thus he says in Prescription No. 26, "we have experimented with this remedy and found it perfect," and in No. 109, "I have experimented with [this powder] and found it perfect, it is without equal in efficacy," and again (No. 80) "a powder experimented upon by ourselves. We have tried it and have found it useful for all maladies of the eyes." It may be mentioned that the scribe who copied the Ebers papyrus occasionally makes similar comments.

It is unfortunate that the author has not handed his name down to us. That he was a doctor is evident, and the frequent use of the first person plural is explained by the passages in which the writer refers to his father, who must also have been a doctor.

APPENDIX.

The Instrument-Case of a Coptic Surgeon.

In the Cairo Museum is a wooden object which is elaborately fitted to make a portable instrument-case. It was discovered at Erment, the ancient Hermonthis, and most probably belongs to the early centuries of the Christian era, although there is no evidence upon which a definite date can be fixed. It must, therefore, be vaguely assigned to some period between the sixth and twelfth centuries. The instruments themselves are missing from the case, but the details of its construction show that they must have been of considerable precision.

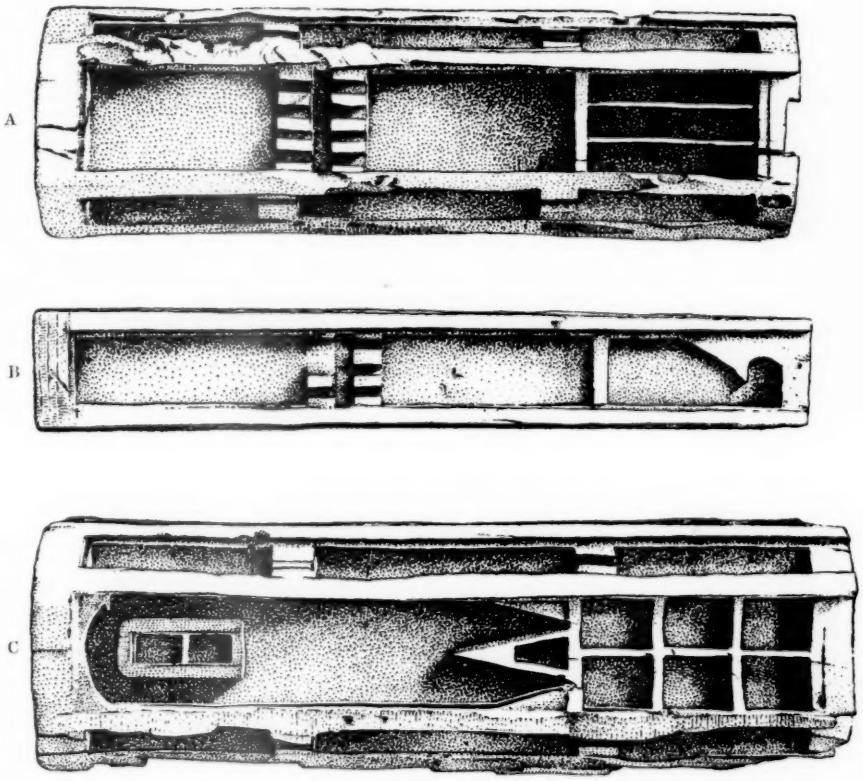
The instrument-case was described and figured some years ago in a purely archæological journal,[4] and is therefore probably unknown to many who are interested in the early history of medicine.

The box consists of a solid wooden core, octagonal in section, with alternate long and short sides. Each face of this core is fitted with countersunk partitions and divisions, each accurately shaped to contain instruments and small packets of drugs. The objects, once in position, were held in place with sliding panels of wood, accurately fitted in grooves (see fig., p. 57). Two of the wide faces, on opposite sides, instead of simple slides had covers made of thicker wood, themselves countersunk and fitted, and closed with sliding lids, the whole sliding face inwards to form a cover to the corresponding partitions on the central core. A transverse groove across the diameter of the core carried a lock-bar which held these two side panels in position, being pushed in to retain them, and drawn out to release them.

The case is made of the wood of some coniferous tree, and is 35·2 cm. long; the long sides in section measure 64 mm., and the short sides 25 to 28 mm.

It will be noticed that the greatest ingenuity is shown in accommodating the maximum number of objects in the smallest possible space. The nature of

[1] Book III.
[2] *Euporistes* II, 3.
[3] Book VII, cap. 8.
[4] G. Daressy: *Annales du Service des Antiquités de l'Egypte*, t. x, Cairo, 1909, pp. 254-257 and 2 plates.



A Coptic Surgical Instrument Case.

(A) One face of the instrument case fitted to receive scalpels, &c.
(B) Inner aspect of the sliding panel fitting on to A.
(C) Face opposite to that shown in fig. A fitted for reception of forceps, packets of drugs, &c.

some of the instruments can clearly be made out from the shape of the receptacles. Thus in one section there is accommodation for eight scalpels, four in each direction, the blades of which lie two by two in the grooves, and theirhandles in the square hollows on either side of these grooves. The short faces on either side of this could each carry two smaller scalpels. In another section there is the place for a spatula, perhaps with a shaft sliding into the handle so as to lengthen or shorten it. In another panel the small square partitions were doubtless used to contain little bladders or bags of dry medicines, whilst in the space below a large pair of forceps was placed. The short sides adjoining this face were fitted for longer scalpels two in each direction. The multiplicity of knives or scalpels is an interesting feature and denotes considerable knowledge and skill in surgery.

Section of the History of Medicine.

President—Dr. ARNOLD CHAPLIN.

THE "HSI YÜAN LU" OR "INSTRUCTIONS TO CORONERS."

Translated from the Chinese

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THE office and functions of coroner, in the modern sense of the term, were known to the Chinese many centuries before "Crowner's Quest Law" was quoted in *Hamlet*.

It was while stationed at Ningpo, in 1873, that I first heard of the *Hsi-yüan-lu*. I found that a copy of this work, here translated, was always carried to the scene of an inquest by the high territorial official on whom the duties of coroner devolved. I also found that inquests were held on the living, when dangerously wounded, as well as on the dead. In the latter case, to move or disturb in any way a corpse, before the coroner had seen and examined it, would lead to the most serious consequences for such reckless interference.

I became sufficiently interested in this phase of Chinese civilization to proceed to a careful study of the text of the *Hsi-yüan-lu*, and thence to translation, for which I was repaid by the possession of some acquaintance with the system of medical jurisprudence in ancient China.

Cambridge, 1923. HERBERT A. GILES.

THE "HSI YÜAN LU," OR "INSTRUCTIONS TO CORONERS."[1]

ORIGINAL PREFACE.

The *Hsi Yüan Lu* dates from the reign Shun Yu [A.D. 1241-1253] of the after Sung dynasty, and was compiled by a Commissioner of Justice named Sung Tz'u from the *I Yü Chi* by Ho Ning, Duke of Lu under the Chin dynasty and his son Mêng, aide-de-camp to the Heir apparent under the first Sung dynasty; also from the *Nei Shu Lu* by an unknown author of the Sung dynasty, and from various other books. Being subjected for many generations to practical tests by the officers of the Board of Punishments, it became daily more perfect and more exact.

[1] Lit., "Record of the Washing away of Wrongs."

The work now published by the Board was compiled from the *P'ing Yüan Lu* by an unknown author of the after Sung dynasty, from the *Wu Yüan Lu* by Wang Yü of the Yüan dynasty, and the *Hsi Yüan Lu Chien Shih* by Wang K'ên-t'ang of the Ming dynasty, and is strictly adhered to by all engaged in the investigation of criminal cases. Yet although in general use among officials, good editions are rarely to be met with in booksellers' shops. A collection of corroborative cases have lately been supplied by Wang Yu-huai of Wu-lin, an additional commentary by Sub-Prefect Yüan Ch'i-hsin of Kueichi, and coloured punctuation by Prefect Chang Hsi-fan of Yüan-ho, making a thoroughly intelligible and complete work. As, however, the blocks are unfortunately kept in Kuangsi, copies are seldom seen in the South (Kuangtung);[1] wherefore, Chung Hsiao-t'ing, a secretary of the Privy Council, from Chiang-tu, desirous of rendering it widely known, has, after careful revision, brought out a new edition.

I, the writer of this Preface, finding that both the summary and supplement of Book V in the original work were comprised in the above-mentioned corroborative cases and additional supplement; also, that the appendices *Pao Chien P'ien* and *Shih Hsiang Pi Lu* were a lot of doggerel rhymes in a vulgar style and of no practical value for reference—had just expunged them one and all, when I happened to come across, on the table of Hsü Shih-hua of Hai-chow, an Imperial Professor, a work entitled *Tso Li Yao Yen* or "Important Counsels to Government Officers," by a Fukien magistrate Yeh Yü-p'ing, commented upon and explained by Chu Hsing-chai of Yün-chien, a head Censor, to which was added a series of twelve additional articles from his own "limited experience." The language being terse and perspicuous, easy to understand and easy to act up to, and of special value to public servants, I substituted it for what I had just expunged, as an appendix to the Hsi Yüan Lu: that all officials purchasing copies might not only derive benefit as regards the investigation of cases, but, further, have their attention called to the fundamental principle of physical and moral government. Thus, an honourable discharge of their duties manifesting itself among officials and a system of virtue and decorous behaviour growing up among the people, the dead would be without wrongs and the living in the enjoyment of happiness—a consummation unceasingly hoped for by Yeh and Chu, and devoutedly wished by Judge Sung!

Done at Huai-pei, in the 9th moon of the 23rd year of the reign Tao Kuang (1843), by T'ung Lien, a Sub-Prefect of the Salt Gabelle.

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[1] *Query*, North.

BOOK I.

CHAPTER I.—GENERAL REMARKS ON INQUESTS.

There is nothing more important than human life; there is no punishment greater than death. A murderer gives life for life: the law shows no mercy. But to obviate any regrets which might be occasioned by a wrong infliction of such punishment, the validity of a confession[1] and the sentence passed are made to depend on a satisfactory examination of the wounds. If these are of a bona-fide nature and the confession of the accused tallies therewith, then life may be given for life, that those who know the laws may fear them, that crime may become less frequent among the people, and due weight be attached to the sanctity of human existence. If an inquest is not properly conducted, the wrong of the murdered man is not redressed, new wrongs are raised up amongst the living, other lives sacrificed, and both sides roused to vengeance of which no man can foresee the end.

In important cases, where death has not already ensued, all will depend on the energy of the coroner in proceeding instantly to personal examination of the victim—noting down the wounds, their position and severity—and fixing the death-limit,[2] in the hope that medical skill may effect a cure; so that in case of re-examination after death with a view to obtain a [different] verdict, the unpleasantness of dissection may be avoided.

Where death has already resulted from the wounds there is still greater need for promptitude ere decomposition sets in, and while as yet it is easy to note the severity and size of each wound. For while you are delaying the body is beginning to decompose though to guard against the infliction of false, or the tampering with real wounds is a principle of the highest importance. No, the coroner and his assistants should hasten to the spot with all speed, that the guilty parties may have no time to concoct schemes for evading the consequences of their crime.

If death has just taken place, first examine the top of the head, and the back, the ears, the nostrils, the throat, &c., and all places where anything might be inserted, on the chance of finding a sharp-pointed instrument of some kind or other. If nothing is found, proceed to examine the body in the regular way.

Should it be a case where examination of the bones is necessary, first carefully interrogate the relatives of the deceased, neighbours, and the accused, bidding them state clearly who struck whom with what weapon, and in what part of the body, the deposition of each being taken down in writing. Then, with your assistants, petitioner, and accused, proceed to the spot where the body lies, and examine it as the law directs. Mark what wounds are on vital spots, whether on the trunk or extremities, whether skin or flesh wounds, whether penetrating to the bone, their colour, size and shape; whether inflicted by the hand or foot, or by some weapon; their severity and appearance, whether recent or old. All this must be carefully elucidated and the prescribed form filled up with your own hand, not by the assistants. Do not, deterred by the smell of the corpse, sit at a distance, your view intercepted by the smoke of fumigation, letting the assistants call out the wounds and enter them on the form, perhaps garbling what is of importance and giving prominence to what is not, adding or subtracting as they see fit. Moreover, deaths from self-strangulation, throat-cutting, taking poison, burning, drowning, and the like, produce very different results, and only the most minute investigation of every little detail will ensure a complete and reliable verdict. If this be neglected, the assistants will intrigue and make light of the whole affair; the culprit will devise some means for escaping punishment, the relatives of the deceased will appeal against your decision, mischief-makers and bad characters will seize the opportunity, and the end of it all will be that your verdict will be discredited, another Coroner deputed, and dissection of the corpse be a matter of necessity, an outrage on the dead, an inconvenience to the living. These are the evils attendant on a dilatory or perfunctory discharge of your duties.

[1] In order to prevent rich criminals from procuring substitutes.

[2] Translated "period of responsibility" in the Penal Code. The term here used, and explained Chapter II *ad init.* was adopted only for brevity's sake.

In all cases where the circumstances are unusually suspicious, extra care must be taken to make the fullest inquiries. For instance, where death has taken place within the death-limit from wounds the cicatrices of which are not distinct, if there be any appearance of illness, it should be asked whether deceased was in the habit of employing doctors, quacks, or the like. Many die from disease, but you are not likely to find this out if you do not ask. It will not, however, do to trust to the testimony of a single person, or to be anything but cautious in employing others to make inquiries lest you defeat your own purpose.

In all cases of death where the relatives of the deceased, not having been absent, have not petitioned till more than a year has elapsed, or where it is not the relatives who file petition but some outsider, or where the more important facts are relegated from the body of the petition to the accompanying statement of circumstances in detail; in all such instances no hasty measures should be taken.

In all cases where requests are preferred, even by near relatives, that examination may be dispensed with; it will be necessary to satisfy yourself that there *actually is a corpse*[1] at the place mentioned before granting the petition.

Care must be taken that neither your own assistants, nor any person mixed up in the case, give notice beforehand to the four next-door neighbours and assist them in absconding; arresting only more distant neighbours, old men, women and children, thereby acting up to the letter of their duty in producing *some one*. Also that the accused conceal no important witness or bona-fide evidence for his own ends, and that he produce no witnesses, either friends, dependants, or tenants and the like, whose evidence at the inquest is false.

Any delay in securing the weapon which caused death will be taken advantage of by the accused's family for concealing the same. Your case would be thus rendered incomplete, and disastrous consequences would ensue. The weapon should be secured at once for future comparison with the wounds as to length and breadth, in order to complete the case.

At all inquests the first thing to be done is to cause the body to be identified by relatives and neighbours. If, however, it is in too advanced a stage of decomposition, they must be examined as to the colour and shape of the clothes deceased had on, if there were any marks on them, and also what scars or marks he might have had on his body. Their answers must be taken down in writing and signed before the examination begins.

The report on an inquest should show where and in what position the body was found, as also each article of clothing it had on; whether there were any tattoo marks, traces of acupuncture or cauterization; whether any limbs were found broken before death; whether hump-backed, having one leg shorter than the other, or bald; the colour of any marks on the body; the presence of tumours, swelled legs, and such like complaints—all these should be carefully noted in case of a further examination being held, or, where deceased's name was unknown, the body being claimed hereafter by relatives, in which case their statements could be put to the test. When criminals die in prison, those points must be attended to with more than usual minuteness.

The object of re-examinations is not to verify a previous verdict, but to guard against any injustice being done. If, therefore, any Coroner thus appointed, losing sight of the importance of human life, allow the least shadow of a wrong to remain unredressed, either from fear of offending the officer who presided at the previous examination, or from unwillingness to release the accused who may happen to be wealthy, from anxiety to suit the report to the wishes of superior officers, or from an endeavour to make the present resemble past cases, and so get quit of his duty as soon as possible—his crime will be greater in degree than that of the Coroner who held the first inquest. Rather let every Coroner deputed proceed with caution and justice to make the most careful investigation possible; for the object in examining a corpse is to arrive at the unvarnished truth, and where it is a case of a wounded man whose life is trembling in the balance, the slightest private bias should not be thrown into the scale.

[1] In order to prevent false accusations with the object of extorting money, followed up, when the money has been paid, by petitions for "no examination."

In cases of severe wounds on vital spots where death was instantaneous or resulted within three days, and where petitioner and witnesses can swear to the weapon and the part struck, it will only be necessary to examine such wound, thus sparing the dead all superfluous handling, and preventing injustice being done to the living. For men, in their passage from youth to manhood, either by falling down and striking against something, or being thumped and rubbed for pains, from the accidental breaking of a boil, carrying heavy weights, or knocking against hard things, cause their blood to be arrested in its course. Where the wounds are slight and of recent date, the bone will be of a red colour, which will pass away in time. Where the wounds are old and severe, the bone will be dark blue in colour, and will remain so to the last. It frequently happens that petitioner and witnesses mention a particular blow behind the ear as the cause of death, whereas on examination the body is found to be covered with wounds, and the higher authorities cancel the inquest as inaccurate. An officer who, in adding circumstances to make his report tally, were to mention wounds on both sides of exactly the same size and colour, would be met, if death resulted from blows, by the question: "Had the accused a similar weapon in each hand, and was there no difference in the force of each blow dealt?" Sometimes after a drunken brawl at night it will be confidently asserted that such a man struck in such a part. Now when the combatants themselves do not know where or remember how many blows each struck, is it likely that witnesses could? Generally speaking, where several people are engaged in a mêlée, in adjudging punishment you should look to the origin of the fray; in examining wounds, the important point is to ascertain their exact positions. Do not, however, adhere too closely to rules, as thereby injustice may be done; neither act with indecision or you will assuredly commit some blunder.

Where several persons strike blows it is difficult to determine which was the fatal blow. If on the body of the deceased there are two mortal wounds, both inflicted by the same man, he alone is responsible. If they were inflicted by different persons, one only pays the penalty of death, the most severe being held to be the mortal wound.

Where there are several wounds, fix on one as the mortal wound.

It is common for wounds to be set down by the accused as self-inflicted by deceased knocking in various ways against things, to the great hindrance of a proper elucidation of facts and settlement of the case. Knocking against things at rest is called *k'o*; running up against things in motion is called *chuang*. Such wounds would be confined to the front of the body, the forehead, &c., and being self-inflicted would not be severe; at any rate, not severe enough to cause death. Wounding oneself behind by running backwards against anything is quite out of the question. Where deceased has been knocked down and has wounds on the back of his head, back, or ribs, inasmuch as he may have been violently thrown down by the accused and have died from the effects of the fall, it will be necessary to observe carefully whether the body was lying on its back or its belly, and to note the severity of the wounds. These must not be hastily entered as wounds received by knocking against things, &c., or a miscarriage of justice will be the result.

Chapter II.—General Remarks on Examining Wounds and Fixing the Death-limit.

Murders are rarely the result of premeditation, but can be traced in the majority of cases, to a brawl. The statute which treats of wounding in a brawl attaches great weight to the death-limit, which means that the wounded man be handed over to the accused to be taken care of and provided with medical aid, and that a limit of time be fixed, on the expiration of which punishment be awarded according to circumstances. Now the relatives of a wounded man, unless their ties be of the closest, generally desire his death that they may extort money from his slayer; but the accused wishes him to live that he himself may escape death, and therefore leaves no means untried to restore him to health. This institution of the death-limit is a merciful endeavour to save the lives of both.

Cases of battery should always be reported by the *ti-pao* or head man before anyone else, and any neglect on his part should be punished severely. Where death

has ensued, near relatives of the deceased, such as father, brother, uncle, sister, wife, or child, who saw the corpse undressed on the day the wounds were inflicted, should be interrogated as to the age of the deceased, the month, day, and hour, at which the wounds were received, the weapon used, and the part struck. Their replies will give you, for instance, an oblique wound in such and such a spot, of such and such a length, or the circumference and diameter of any round wounds there may be, whether livid or red, swollen or not swollen, whether or not the skin or bone was broken, and what witnesses there were, all of which must be entered in due form in the accusation. Where death has not ensued, summon and examine the head man, and if the wounds are really grave, do not let the sufferer be brought up for inspection lest death might result from the exposure of his wounds, but go at once to the place yourself, on horseback or in a sedan, with a few attendants only, and proceed to examination, noting the wounds and fixing a death-limit. Accused should also be instructed to take the wounded man to his own home and see that he is properly treated. Meanwhile the case stands over. On the day death takes place, and formal application is made for an inquest, the evidence, &c., taken at the previous examination should be gone through and the wounds identified; all mortal wounds being carefully re-examined if there is the slightest cause for suspicion. Care in the early stages may be the saving of the lives of many who are mixed up in the case. If it is a trumped-up affair, make it clear that it is so, to warn people from like attempts in future. If it is boná-fide, take down the evidence and confession at once, to prevent any advances being improperly made towards you. See that your assistants, petitioner, accused, and the witnesses are all present, and make them sign the necessary declarations to that effect. Then, in summing up and awarding punishment, be still more careful that every point has been brought out clearly, and that there are no doubts in your own mind; for by these means, not only will justice be done to the living and to the dead, but you will prevent the cancelling of your inquest, long months and years of delay, and the implication of many persons. Where the petitioner has failed to report a case [of wounding] and to apply for the fixing of the death-limit, petitioning only when death has taken place—except in cases where it was instantaneous or within three days, in which instances the inquest may proceed as usual—it will be necessary to be very much on one's guard against counterfeit wounds. If, before reporting a case to the proper authorities, the opportunity is seized for carrying the corpse to the door [of the accused's house] seizing on his goods and wounding people, this second case must be settled as well as the first and a death-limit fixed according to circumstances. Should a death-limit be partly in one month and partly in another, you must observe whether the first is a long or a short month: there must be no carelessness about the limit of life and death. In a word, the examination of a body should be performed speedily and without making light of it; the examination of bones should be performed carefully, and without needlessly breaking them. If the report of an inquest is rejected by the higher authorities because confused in detail of the circumstances, investigate the circumstances only; if they complain of discrepancies as regards the wounds, then examine the wounds; but do not take any unnecessary steps.

There are vital spots and mortal wounds. The top of the head, behind the ear, the throat, the pit of the stomach, &c., belong to the former class; death soon takes place. Other ordinarily vital spots are the back of the head, the forehead, the chest, &c. Mortal wounds are where the flesh is livid, the skin broken, there is a deep gash, bones broken, brains coming out, blood flowing, &c. Where a mortal wound is given on a vital spot of the kind first mentioned, death will result in three days; on an ordinarily vital spot, in ten days. If a slight wound has been given on a vital spot, or a severe wound on a non-vital spot, though death ensue within the limit, yet other circumstances should be taken into consideration and allowed to influence your verdict. This applies still more forcibly if death occurs after the death-limit.

The death-limit is the most important point of all. Immediately on a case being reported, make a personal examination of the wounds and fix the limit accordingly. If death ensues from improper medical treatment, make a calculation of the time, so as to determine whether within or without the limit. Also take a note of the time at which the wounds were received, that your case may be clear and complete.

Chapter III.—Form for Entering Wounds.

[Sixteen vital spots are enumerated on the front of the body, and six on the back; thirty-six non-vital spots on the front, and twenty on the back. These are supposed to be shown in the accompanying diagrams.]

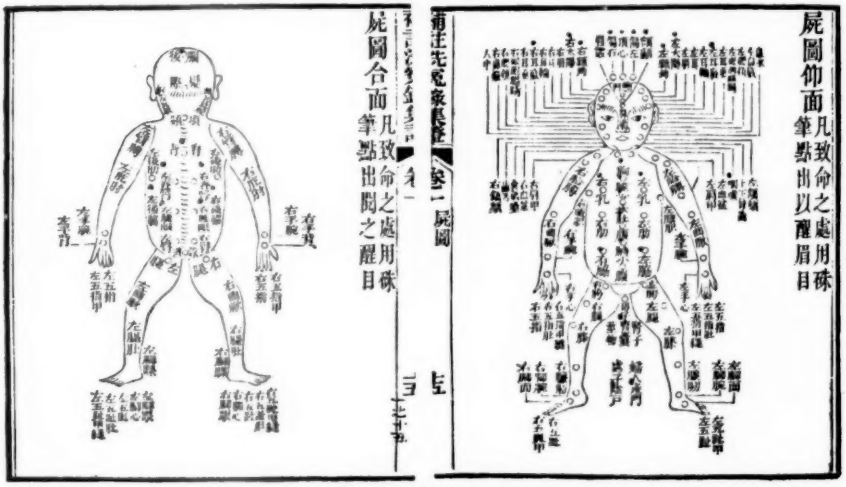


Fig. 1.—For examination of wounds and fixing the death-limit. The black dots indicate the the vital spots of the body.

Chapter IV.—Examination of the Body.

Where a corpse has to be examined prepare for use plenty of onions, red pepper, salt, and white prunes, also grains and vinegar, in case the wounds are indistinct or invisible. Take besides an earthenware basin, with implements for breaking, powdering, &c.

Do not be deterred by the smell and above all do not let the assistants intercept your view of any part of the body, as this would be a very serious hindrance.

Only on the discovery of a mortal wound can you insist on the relatives of deceased and the accused being present at the proceedings. On no account allow them to come too near for fear of damaging the body.

Begin to examine from the head downwards. Measure the length of the hair, noting if any has been pulled out or cut off. Look under the hair on the top of the head, and between that and the forehead, to see if there if any other cause of death, as a burn or a wound from a sharp-pointed weapon, &c. Examine the forehead and temples with the same view. Then the eyebrows, eyelids, and eyes. Note if the eyes are open or closed; if the latter, open them to see whether the pupils are perfect or not. Examine the cheek-bones to see if there are marks of a blow from a fist; the cheeks, whether tattooed or not, or whether such marks may have been obliterated. In the latter case, cut a slip of bamboo and tap the parts, when the tattooing will reappear. Note if the ears have been bitten, grasped by the hand, cut or wounded; if the nostrils or any part of the nose have been pierced by any sharp instrument; and if the lips are opened or closed. Count the teeth, and observe if the tongue is protruding. Also examine the jaws. Into the throat insert a silver needle, and note if it is black when withdrawn. Examine the outside to see if it is swollen, and if there are any wounds which might have produced death. Feel with your hand the windpipe and gullet; examine the shoulders and arm-pits, for a severe wound in the last-

mentioned parts would cause death. Examine the arm, wrist, hand, and fingers, down to the quick of the nails. These are not vital spots in themselves, but were the bones broken, or the quick pierced, and not properly cured, death might result. Examine the chest, breasts, pit of the stomach, belly, ribs, &c., &c. These are nearly all vital spots. Examine the thighs, knees, shin-bones, ankles, instep, toes and toe-nails. The same remark applies here which referred above to the hand and fingers.

Examine the back of the head, noting if there are any marks of bruises on the "pillow-bone" from a fall, &c. Examine the hair, the nape of the neck, and behind the two ears. Examine the arm, elbow, and back of the hand as above. Examine between the shoulders to see if there are any marks of cauterization, the back generally, ribs, waist, &c. Note if the body bears marks of corporal punishment, and examine the inside of the knees, and the calves. Any scars on the ankle-bones, if both on the inside and outside, may be set down to torture; if on the outside only to a blow. Examine the heels, the sole of foot, under the toes, and the quick. These are not vital under ordinary circumstances.

Note the age of the man, and take his height and breadth across the shoulders; observe also the position of the wounds, and their nature, whether scrapes or bruises. Observe if they are of a dull livid red or black hue; and noting the size and extent of each, determine on the immediate cause of death. Mark on what places there are scars of tattooing, cauterization, or boils, and note down whether recent or old, and whether or not there is any pus. Also, whether there are traces of the itch, sores, or other marks on the body, both natural and artificial. All such should be carefully written down, or marked as wanting.

In examining a body or skeleton where the wounds are not visible, spread grains and sprinkle some vinegar upon the corpse in the open oil-cloth umbrella and hold it between the sun and the parts you want to observe. The wounds will then appear. If the day is dark or rainy, use live charcoal [instead of the sun]. Suppose there is no result, then spread over the parts pounded white prunes with more grains and vinegar, and examine closely. If the result is still imperfect, then take the flesh only of the white prune, adding red pepper, onions, salt and grains, and mix it up into a cake. Make this very hot over a fire, and then, having first interposed a sheet of paper, lay it on the part. The wound will then appear.

Where death has resulted from blows, and the wounds have not been severe enough to break limbs, the flesh will adhere tightly to the bones, and if it cannot be washed away should be removed with the finger-nail, when the wounds will be visible [on the bone].

The human body is naturally red or brown, but after death it changes to a livid blue. Where no wounds are visible, only suspicious appearances, first sprinkle the corpse with water, then take the heart of an onion, smash it with a blow of the hand and lay it on the part, spreading over it a sheet of paper dipped in vinegar. Let it stay on a little while, then take it off and wash with water. The wound will thus appear. If there are several dark-coloured marks on the body, take some water and let it fall drop by drop on to them. If they are wounds, they will be hard and the water will remain without trickling away; if they are not, they will be soft and the water will run off.

In examining wounds, the finger must be used to press down any livid or red spot. If it is a wound it will be hard, and on raising the finger will be found of the same colour as before. If water is dropped on to it and the drops do not trickle away, it is undoubtedly a real wound. If it is a part which has changed colour it will turn white when the finger is pressed down on it, and water dropped on to it will not remain. Change of colour is caused by the blood in the bowels dispersing after death; not being able to congeal in any one place, it spreads over the whole surface. But where the blows were given before death, the blood congeals into a wound there where vitality ceases—[i.e., at the spot struck]. Now the blood is dependent for its motion on vitality; if vitality stops, the blood stops also—hence the hardness.

There are certain parts such as the eyebrows, the windpipe and gullet, the ribs, &c., which are not marked as vital, but which may prove so if the wound happen to be very severe. This is an important point to remember at the time of examination.

Any slight red marks on the back, thighs, calves, &c., are the result of the corpse lying on its back and the blood sinking to those parts: they are not connected with the cause of death.

Part II.—Examination of a Corpse which has not been Buried.

Whether in a room, on the ground, or on a bed, or whether in the open air, behind or in front of a house, on a hill, in water, or the grass, you must first measure the exact position of a corpse with regard to surrounding objects. If in water, how far from the nearest hill or bank; also ask on whose ground, and the name of the place. If in a room, note in what part of it and if there is anything covering or spread underneath it. Then, and then only, should the body be removed for examination.

Begin by stripping off all the clothes, &c., down to the shoes and stockings, taking a note of each as well as anything else about the person. Then wash the body in warm water before proceeding to examination. Grains and vinegar must not be used till this is done.

Part III.—Examination after Temporary Burial.

First see the grave, asking on whose land it is and the name of the place, and take the size and height of the mound. If the coffin has been placed temporarily in any one's house, take the measurements as before.

Next observe which way the head points. Suppose towards the east and the feet towards the west, then note how far each is from any particular spot, and the sides of the body in the same way. In the presence of all, remove the earth or bricks and observe what is spread underneath, whether the coffin is varnished or ornamented, and whether the mat, or whatever is under the corpse, has a border or not. Then remove the body to a convenient spot and proceed to examination.

Chapter V.—Washing and Preparing the Body.

When the body has been removed to a convenient spot, first inspect it as it is. Then dash water over it. Next wash off every particle of dirt with soap, and throw more water over it, the corpse lying all the time on a door or a mat to keep it clean. This finished, grains and vinegar may be spread on as usual, the clothes of deceased laid over the body and saturated with hot vinegar, the whole being covered over with mats (to keep the steam in). In a little while, when the body has become soft, remove the coverings, wash off the grains and vinegar and proceed to examine. Do not trust too much to your assistants; if they only sprinkle spirit (without the grains) and vinegar the wounds will not appear.

Prepare plenty of grains and vinegar, also paper for putting under the body. The best kinds of the latter are *t'eng lien* and *pai ch'ao*. Bamboo paper is spoilt by vinegar and salt and may injure the body.

At the beginning of spring and during winter the vinegar and grains should be used very hot; in the middle of spring and towards the end of autumn they should be rather less so. In summer and autumn, if the grains and vinegar were at all hot, this, added to the heat of the weather, might cause injury to the skin. Late in autumn use them hot, and at a distance of from three to four feet on either side of the body light fires to stimulate their action.

In very cold weather, when the corpse is frozen hard and no amount of grains and vinegar, however hot, or clothes piled up, however thick, will relax its rigidity, dig a hole the length and breadth of the body and three feet in depth; lay in it a quantity of fuel and make a roaring fire. Then dash over it vinegar, which will create dense volumes of steam, in the middle of which place the body with all its dressings right in the hole, cover it with clothes and pour on more hot vinegar all over it. At a distance of two or three feet from the hole on either side, light fires as before. When you think the heat has thoroughly penetrated, take away the fire and remove the body for examination.

At the end of winter and beginning of spring it will not be necessary to make a hole but merely to light fires on each side. This, however, must be left to the judgment of the Coroner.

Chapter VI.—First Examination of the Body.

At a first examination, if it is a case where death resulted from blows, it will not do to report the corpse decomposed and therefore presenting no reliable features; but the scars must be carefully scrutinized and the causes of death ascertained. If from lapse of time decomposition has really taken place, the body may be reported as unfit to stand the necessary handling.

When the examination as to the presence or absence of wounds is over, leave the corpse on the mats in the exact spot where it was examined, cover it up and seal it all round with the lime seal, taking a note of the number of seals. Then hand over charge of the body to the head man of the place, and add his formal receipt to the records of the case as a precaution against any one tampering with or injuring the corpse.

Part II.—Further Examinations.

If the body is many days old, and the head and face have swollen up, the skin and hair come off, the lips turned back and the mouth open, and maggots have already made their appearance—then it is quite unfit for examination; and if the wounds were inflicted by a sharp or other weapon, or a blow of the hand or foot on a fleshy part where there is no bone, you may report that examination is impossible. But if the bone has been injured in any way, then the body must be washed and carefully inspected, and the cause of death ascertained. The plea used in the other case will not hold good.

The Coroner who thus re-examines should, on the completion of his task and if there is no dissentient voice, hand over the body to the nearest relatives. Where there are no near relatives, it may be entrusted to the head man who should be ordered to bury it and see that it comes to no harm. If there happen to be dissentient voices, then it must not be entrusted to any one, but dig a hole and in it lay the corpse with all its mats, &c.; cover it over with a door and pile up earth on the top. Then seal all round with the seal and take a note of the number of seals in case of there being a still further examination. The person left in charge must sign a formal declaration to that effect to be put with the records of the case.

At the end of the first or further examination the relatives of deceased or the head man should be instructed to take charge of the body but it should on no account be carried off to the Yamên, to their great inconvenience and for no particular object. The accused and the witnesses only should be arrested; the others can be summoned afterwards.

Chapter VII.—Decomposition Different at Different Seasons.

In the spring months, when a body is two or three days old, the flesh of the nose, mouth, abdomen, sides and chest becomes slightly livid; after ten days a foul liquid issues from the mouth, nose and ears. The bodies of fat people swell and the skin separates from the flesh, which does not take place with the bodies of thin people or those who have been long ill, till more than half a month has elapsed

In the summer months, first the face and then the flesh on the belly, ribs, and chest changes colour in one or two days. In two or three days a foul liquid issues from the mouth and nose, and maggots appear. The whole body swells, the lips pout, the skin rots and separates from the flesh, blisters rise, and in four or five days the hair falls off.

In very hot weather, the injured parts of a body which has been prepared will be mostly covered with a white skin, but the uninjured parts will be dark coloured. The exact nature of the wounds it will be very difficult to distinguish, but if from a dread of the smell, &c., you fail to make a most searching examination, the result is sure to be unsatisfactory. Wherever there is the slightest suspicious appearance, the loose skin

should be removed, and if there is a wound, there will be a hole-like appearance beneath it.

In very hot weather, supposing maggots appear at the temples or other places before appearing at either of the nine orifices, it will be because in that particular part there is a wound.

In the autumn months, two or three days produce the same effect as one or two in summer, and so on in proportion.

In the winter months, a corpse turns after four or five days to a yellowish purple. In half a month the flesh on the face, mouth, nose, &c., begins to decompose, but wrapping it in mats and placing it in a moist place will preserve it longer. Be careful to be guided by the seasons, not by the months.

In extremely hot weather, decomposition begins after one day, the body assuming a dark, dull, hue, and emitting a smell. In three or four days the flesh becomes rotten, maggots appear, a dark fluid issues from the mouth and nose, and the hair gradually falls off.

In spring and autumn, when the weather is mild, two or three days are equal to one in summer, and eight or nine to three or four. In very cold weather, five days are equal to one in summer, and half a month to three or four summer days.

Chapter VIII.—To Distinguish Real and Counterfeit Wounds.

In examining a body which is not yet decomposed, pay attention only to such parts as are red, swollen, cut or bruised, discriminating between mortal and non-mortal wounds. Such parts as are of a livid or purple colour need not occupy your time, as this hue is common to all bodies in a state of decomposition. Wounds on bones may be of various colours and shades, which are counterfeited in the following manner: *red*, by taking some genuine safflower, sapanwood, and black plums, and making them into an ointment, adding alum, and painting it on the bone, pouring over it boiling vinegar, when a red colour will be obtained of a darker or lighter shade exactly like that of a real wound; *purple*, by taking sapanwood and "earth's blood," and applying as before; *dark blue* or *black*, by taking green alum or nutgalls and mixing with vinegar into a thick liquid, regulating the proportions of each according as the shade required is light or dark. Though very deceptive, these counterfeits may be detected by the dullness of their colour, by their being apparent to the touch, and by the absence of the usual halo-like shading-off of colour all round. But everything depends on the energy displayed at the time of examination; there must be no carelessness.

Where blows have been given resulting in death, the injured parts will be surrounded by a purple or red halo. If, when death has already taken place, lighted bamboo strips have been used to burn a wound, which it is pretended was inflicted before death, the wound will present a scorched appearance, will be level with the surrounding flesh, and not hard to the touch. If the bark of the willow tree be used to make a wound, the flesh will be rotten and black, livid all round but without any swelling, and not hard to the touch. Counterfeit wounds are also made by lighting paper inside a cup and applying it to the flesh; such wounds resemble a blow from a fist, but all round there is a red scorched mark, the flesh inside is yellow, and although it swells, it does not get hard.

All wounds must be determined by the inflammation, which is the gradual diminishing of the wound, change of colour, from dark to lighter shades, and lessening of intensity. Also, near where the wound ends there should be a halo-like appearance, like rain seen from a distance, or like fleecy clouds, vague and indistinct, fresh-coloured and smooth-looking, a result which should proceed naturally from the infliction of the wound. This is the most important principle of all. If the red is by itself and purple by itself, of a dull colour and collected into one spot, there being moreover, no halo, then the wounds are counterfeit.

At the time of examination take a piece of white cloth or paper and dip it in the wine and vinegar you are about to use. If the latter have been tampered with the cloth or paper will change colour.

Chapter IX.—De corporibus feminarum inspiciendis.

Si quaestio de morte virginis habeatur, primum locum ubi jacebat mortua notare oportebit, quo facto corpus aliorsum auferatur. Tum obstetricem appella, quae ungue medio secto necnon et lana cincto, digitum in vaginam coram omnibus inserat. Si lana sanguine rubido maculata, virgo erat.

Corporibus feminarum inspiciendis, vagina maxima cura scrutari debet, si forte acus vel aliud quoddam acutum hac via in vulvam introductus sit. Vulnus superficiale maculam rubram prope umbilicum efficiet; nulla profundi vestigia.

Feminarum corpora quae, vulnere in pudendis accepto, jamdiu putrida scrutari non possunt, omnia in vertice summo et in osse sacro maculas habebunt.

Scire an sit gravida femina, obstetricem jube ventrem mortuae manu deprimere. Si venter ut lapis aut ferrum durus, gravida est.

Si corpus feminae quae gravida caesa vel parturiens mortua est, in sepulcro positum, deinde post multos dies de novo inspiciatur, parturitionem interea sine ullo auxilio perfectum fuisse videbis.

Persaepe accidit viduas et virgines juventa prima vulvae morbo affici; hic post nuptias, harmonia *yin et yang* confecta, partus forma erumpit, monstrum jam colubri jam alio hujus generis simile, et plerumque a vero partu non sine cura distinguendum. Sunt quae alia horrenda edunt, eadem cura inspicienda.

Ubi parturitio vulneribus accelerata est, foetus aetatem et formam (perfectam vel imperfectam) obstetrix determinet. Nam si foetus formam est imperfectus, massa et praeterea nihil, qua liquefacta nil nisi liquor putridus remanet, parturitio vulneribus accelerata minime potest adjudicari.

Infantem propter matris timorem in vulva mortuum, placenta purpurea et nigra et sanguine maculata et mollis sequetur. Si post partum mortuus est, corpus rubidum, placenta alba. Si post partum ob rem quamdam nefariam vel manu vel pede strangulatus sit, gulam digitis comprimere oportebit: facies vel purpurea et rubra, vel purpurea et nigra erit. Si puer qui ita occisus annorum decem est, ejus manus et pedes luctaminis signa ostendent.

Chapter X.—Dried-up Corpses.

First lay down charcoal ash, about the length and breadth of the body in extent; cover it with a piece of thin cloth and sprikle water all over it. Lay the body on this and cover it entirely with another piece of cloth, on which spread more charcoal ash, covering it once more with cloth, and sprinkling water as before. In a little while the skin and flesh will begin to soften, and then the cloths and ashes can be removed and the body washed with hot vinegar. Spread on the injured parts a mixture of red pepper, onions, salt, and white prunes rubbed down into a poultice with grains and heated over the fire, interposing a sheet of paper between the parts and the poultice. The wounds will then be distinct.

If the wounds have become invisible from the flesh drying, take five catties of grains, powdered ephedra flava, yellow horsetail, and powdered liquorice-root, of each 3 oz., boil it all into a gruel and let it cool. Then smear it all over the body, make a hole in the ground, and use the steaming process as in winter, heating the ground, throwing plenty of wine and vinegar on, laying the body in, and covering it up with mats, &c. Besides this, take a pot of clean water, pour into it 2 lb. of samshoo and boil in it two pieces of cloth. When the corpse is soft remove it into a convenient place and use these to rub it clean: the wounds will then appear.

Chapter XI.—Examination of a Decomposed Body.

The position of a body having been noted, dash away the maggots and dirt with water, and when the corpse is clean begin to examine. Before applying grains and vinegar, keep on dashing fresh spring water all over the body.

The skin and flesh of wounds inflicted by striking or cutting are of a red colour;

if very severe, livid or black; the flesh adheres to the bone and is free from maggots.

Where death has resulted from blows, but decomposition has taken place and the maggots have left nothing but bones, the blood on the wounded part sticks to the bone and dries up black. If there is no wound, but the bone has cracks in it like hairs or like the cracks in china and barely discernible, these are proofs that there was no wound.

Where the body is too much decomposed for examination, you must report clearly that the hair was gone, the skin and flesh on the temples, head, face, and all over the body was livid or black, quite decomposed and eaten away by maggots, so that the bones were exposed.

If the skin and flesh are in a state of decomposition, you must report whether entirely so, round the parts where the bones show, or whether only slightly so on the surface; also, whether there are any other injuries on the body, as also the age of deceased, his facial appearance and the cause of death. Moreover, that the body was really too far gone for examination, and that having felt it all over with your hands you failed to find any broken bones.

Chapter XII.—Examination of Bones.

Man has three hundred and sixty-five bones, corresponding to the number of days it takes the heavens to revolve.

The skull of a male, from the nape of the neck to the top of the head, consists of eight pieces—of a Ts'ai-chow man, nine. There is a horizontal suture across the back of the skull, and a perpendicular one down the middle. Female skulls are of six pieces, and have the horizontal but not the perpendicular suture.

Teeth are twenty-four, twenty-eight, thirty-two or thirty-six in number. There are three long-shaped breast-bones.

There is one bone belonging to the heart of the shape and size of a cash.

There is one "shoulder-well" bone and one "rice-spoon" bone on either side.

Males have twelve ribs on either side, eight long and four short. Females have fourteen on each side.

Near the kidneys of both males and females there is a bone about as big as the hand perforated with eight holes in rows of two.

The bones in the forearm are two in number as also in the leg between the knee and ankle. At the wrists and ankles of males there are *ribs;* women have not these. Both knees have a bone hidden inside as big as the thumb. In the hand and foot there are five spaces, the thumb and big toe being each divisible into two parts. These last have each two joints; the other fingers and toes, three.

The pelvic bone is like a pig's kidneys, with the indented part just under the spine. In males, there is quite a curve where the spine meets this bone, making it appear as if there were horns sticking up on either side, like the water caltrop. It has nine holes. In women, the part where the spine joins is flat, and there are only six holes. Take a thin piece of twine or a strip of bamboo and tie a paper mark to each bone for convenience' sake at future examinations, and to prevent confusion.

Chapter XIII.—Examination of Bones long after death.

For the examination of bones the day should be clear and bright. First take clean water, and wash them, and then with string tie them together in proper order so that a skeleton is formed and lay this on a mat. Then make a hole in the ground, 5 ft. long, 3 ft. broad, and 2 ft. deep. Throw into this plenty of firewood and charcoal and keep it burning till the ground is thoroughly hot. Clear out the fire and pour in two pints of good spirit and five pounds of strong vinegar. Lay the bones quickly in the steaming pit and cover well up with rushes, reeds, &c. Let them remain there for two or three hours until the ground is cold, when the coverings may be removed, the bones taken to a convenient spot and examined under a red oil-cloth umbrella.

If the day is dark or rainy the boiling method must be adopted. Take a large jar and heat in it a quantity of vinegar; then having put in plenty of salt and white prunes boil it all together with the bones, superintending the process yourself. When it is boiling fast, take out the bones, wash them in water and hold them up to the light

The wounds will be perfectly visible, the blood having soaked into the wounded parts, marking them with red or dark-blue or black. Next carefully observe if any of the bones are cracked or split.

The above method is, however, not the only one. Take a new yellow oil-cloth umbrella from Hangchow, hold it over the bones and every particle of wound hidden in the bone will be clearly visible.

In cases where the bones are old and the wounds have been obliterated by long exposure to wind and rain, or dulled by frequent boilings, it only remains to examine them in the sun under a yellow umbrella, which will show the wounds as far as possible.

There must be no zinc boiled with the bones or they will become dull.

Where bones have passed several times through the process of examination they become quite white and exactly like uninjured bones; in which case, take such as should show wounds and fill them with oil by the cracks and holes there always are. Wait till the oil is oozing out all over, then wipe it off and hold the bone up to the light; where there are wounds the oil will stop and not pass, the clean parts have not been injured.

Another Method.—Rub some good ink thick and spread it on the bone. Let it dry and then wash it off—where there are wounds, and there only, it will sink into the bone.

Another Method.—Take some new cotton wool and pass it over the bone. Wherever there is a wound some will be pulled out. At all injured places note whether the splinters of bone point inwards or outwards. In wounds from blows they point inwards; and if they point outwards these are not wounds. Wherever the skull has been hurt the bone is dark-coloured; wherever a bone has been broken, there will be traces of blood on it.

Carefully observe any dark-coloured or purple and black halo-like appearance. If long-shaped, it was inflicted by a weapon of some kind; if round, by the fist; if large, by the head; if small, by the point of the foot.

When the preparing and examination of the bones is finished, the assistants should call them over in order. For instance, that such and such bones are all complete, and so on. Next, let every single bone be marked carefully with a number to facilitate their being put together again (if required). Wrap them up in several folds of paper and three or four folds of oil-paper, tie the packet securely with string, seal and sign it. Then pack it up in a tub, covering the top with a board, make a hole and bury the tub, piling up earth and setting a mark, besides using the lime seal.

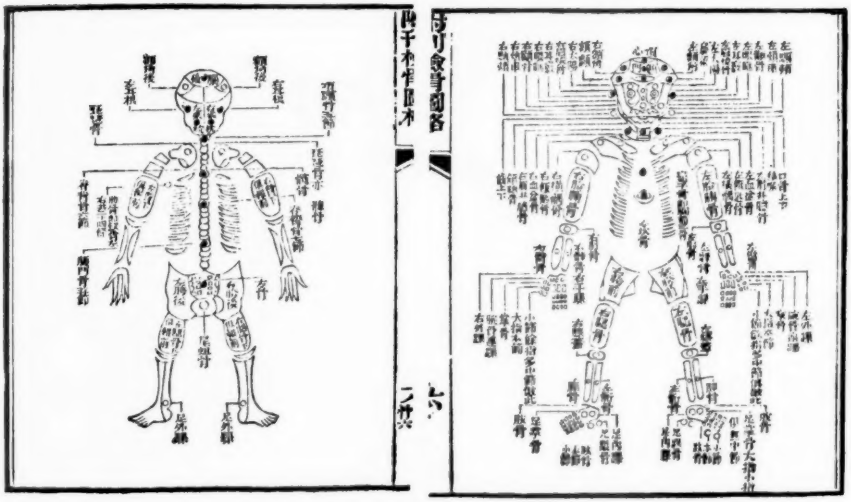


FIG. 2.—For the study of anatomy.

Part II.—To Ascertain whether the Wounds were inflicted Before or After Death.

Wounds inflicted on the bone leave a red mark and a slight appearance of saturation, and where the bone is broken there will be at either end a halo-like trace of blood. Take a bone on which there are marks of a wound and hold it up to the light; if these are of a fresh-looking red, the wound was inflicted before death and penetrated to the bone; but if there is no trace of saturation from blood, although there is a wound, it was inflicted after death.

All men have old scars on their bodies, either from falling down in youth or fighting, being bambooed, boils, &c. Although the place heals in time, the scar never passes away; it takes a darkish hue and remains visible after death. For where the blood has once congealed, it will never resume its former appearance. But old wounds have not the halo-like appearance, are soft to the touch, are on a level with the parts surrounding, and of a dull colour. The flesh and bone are both different from those of a recent wound.

Chapter XIV.—Anatomy of the Human Body.

(A mere list of bones.)

Chapter XV.—Dropping Blood.

The bones of parents may be identified by their children in the following manner. Let the experimenter cut himself or herself with a knife and cause the blood to drip on to the bones; then if the relationship is an actual fact the blood will sink into the bone, otherwise it will not. N.B.—Should the bones have been washed with salt water, even though the relationship exists, yet the blood will not soak in. This is a trick to be guarded against beforehand.

It is also said that if parent and child, or husband and wife, each cut themselves and let the blood drip into a basin of water the two bloods will mix, whereas that of two people not thus related will not mix.

Where two brothers who may have been separated since childhood are desirous of establishing their identity as such, but are unable to do so by ordinary means, bid each one cut himself and let the blood drip into a basin. If they are really brothers the two bloods will coagulate into one; otherwise not. But because fresh blood will always coagulate with the aid of a little salt or vinegar, people often smear the basin over with these to attain their own ends and deceive others: therefore always wash out the basin you are going to use, or buy a new one from a shop. Thus, the trick will be defeated.

The above method of dropping blood on the bones may be used even by a grandchild desirous of identifying the remains of his grandfather; but husband and wife not being of the same flesh and blood, it is absurd to suppose that the blood of one would soak into the bones of the other. For such a principle would apply with still more force to the case of a child who had been suckled by a foster-mother and had grown up indebted to her for half its existence. With regard to the water method, if the basin used is large and full of water the bloods will be unable to mix from being so much diluted; and in the latter case where there is no water, if the interval between dropping the two bloods into the basin is too long, the first will get cold and they will not mix.

Chapter XVI.—Examination of Ground.

There are some atrocious villains who, when they have murdered any one, burn the body and throw the ashes away, so that there are no bones to examine. In such cases, you must carefully find out at what time the murder was committed and where the body was burnt. Then, when you know the place, all witnesses agreeing on this point, you may proceed without further delay to examine the wounds. The mode of procedure is this: Put up your shed near where the body was burnt, and make the accused and witnesses point out themselves the exact spot. Then cut down the grass, &c., growing on this spot, and burn large quantities of fuel till the place is extremely hot,

throwing on several pecks of hemp-seed. By and by brush the place clean, then, if the body was actually burnt on this spot, the oil from the seed will be found to have sunk into the ground in the form of a human figure, and wherever there were wounds on the dead man, there on this figure the oil will be found to have collected together, large or small, square, round, long, short, oblique, or straight, exactly as they were inflicted. The parts where there were no wounds will be free from any such appearances. But supposing you obtain the outline only of the wounds without the necessary detail, then scrape away the masses of oil, light a brisk fire on the form of the body and throw on grains mixed with water. Make the fire burn as fiercely as possible, and throw on vinegar, instantly covering it over with a new well-varnished table. Leave the table on a little while, and then take it off for examination: the form of the body will be transferred to the table, and the scars of the wounds will be distinct in every particular.

If the place is wild, and some time has elapsed since the deed, so that the very murderer does not remember the exact spot, inquire carefully in what direction it was with regard to such and such a village or temple, and how far off. If all agree on this point, proceed in person to the place and bid your assistants go round about searching for any spots where the grass is taller and stronger than usual, marking such with a mark. For where a body has been burnt the grass will be darker in hue, more luxuriant, and taller than that surrounding it, and will not lose these characteristics for a long time, the fat and grease of the body sinking down to the roots of the grass, and causing the above results. If the spot is on a hill or in a wild place where the vegetation is very luxuriant, then you must look for a growth about the height of a man. If the burning took place on stony ground, the crumbly appearance of the stones must be your guide: this makes the process much easier.

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Chapter I.—Death from Blows in a Fight.

Where death has resulted from blows in a fight, the mouth and eyes will be open, the hair and clothes disordered, and the two arms stretched out. [For just previous to death the mouth will be in full play, and the eyes will be glaring fiercely; the hair and clothes will get disordered in the scuffle; and the arms, employed in defence, will be stretched out.] Where there are wounds the skin will separate from the membrane below and will sound if tapped by the finger. If hot vinegar is applied, the cicatrix will appear. Observe its size and measure its length and breadth. Also note how many wounds there are, either of which would have caused death, but fix on some one in the most vulnerable part as the mortal one. If death occurs either within or without the limit, it may be that medical aid has been of no avail, or from exposure to the air, in which case the face would be yellow and flabby.

Chapter II.—Wounds inflicted by the Hand and Foot, or by Weapons.

Where there is blood there is a wound, and all such as are not inflicted by the hand or foot are also called wounds from weapons. "Weapons" are not necessarily swords or knives.

Wounds from the hand are mostly about the upper part of the body, the back and chest, or upper ribs, rarely on the lower ribs. Kicks are generally in the pit of the stomach, the ribs, &c. They are indeed found on the upper part of the body, but not unless the victim was lying on the ground. At the examination all those points should be carefully considered, and not merely the size and shape of the wounds. Blows given by the hand or foot or weapons must be on such parts as the head, face, chest, breasts, &c., to be considered as mortal. An arm or leg broken may cause death; there will be a halo round the parts if the wound was inflicted during life. The colour of a wound inflicted by a weapon, the hand, foot, or anything hard, is, if very severe, a dull purple, accompanied by a slight swelling; if less so, purple and red with a slight swelling, purple and red [without any swelling], dark blue, or even only a little discoloured. Wounds from weapons are long-shaped, either oblique or horizontal; from the fist, round; from a kick, larger than those dealt by the fist.

Wounds which occasion death in a couple of days will be somewhat larger than usual; if inflammation sets in badly, death will result in about that time. If death is instantaneous, the wound will be deeper and more severe, the inflammation purple and black, penetrating at once and thus causing death on the spot.

Wounds received by knocking against things are, even though the skin is unbroken, round or with straight outlines. Though the skin should be broken, the wounds will not be deep. Wounds from weapons, or the hand or foot, where the skin is broken but without bleeding, will have a purple and red halo. [Wounds from knocking against things have no halo; those from blows have.]

Wounds from a stick or bludgeon are oblique and long-shaped: one end will be higher than the other. You must be careful to note *which* end, and also whether the blow was struck from the right or from the left, as the wound must tally with the blow to make the case easy of investigation.

Where a stick or bludgeon has been used, the wounds will generally be on non-fleshy parts, and the victim may die in any time from two hours to ten days. Where harder weapons have been used, causing death, pay still more attention to the severity of the wounds. If there was a bit of a scuffle first, accused catching hold of deceased's hair and then letting out with his hand or foot, the wounds will generally be in some fleshy vital spot. If a mortal wound is given by the foot in some vital spot, see if accused had on shoes or not.

In all cases of kicks, first ask accused what he had on his feet. If common, home-made shoes with soft soles, the wound will be slight and swollen; if shop-made shoes, with sewn soles, the wounds will be more severe and hard. Shoes with sharp-pointed toes will cut into the bone, and nailed shoes will inflict a still heavier wound, discolouring the bone. Wounds which injure the bone most are inflicted by shoes with smooth, round toes, heavily studded with nails. Such require careful discrimination.

Blows given with the head, elbow, knee, &c., must be classed as such according to the evidence, and considered generally as coming under the head of blows from "weapons."

Where wounds are inflicted with weapons and the skin is not broken, but the bone and flesh are injured, or where the wound is in a fleshy place, examination should proceed at once. It will be necessary to elicit clearly whether, if the wound is on the left side, it was inflicted with the right hand, or whether on the right side, and consequently more round towards the back, it was inflicted from behind.

Examine carefully the length, breadth, shape, and size of all wounds inflicted by the hand, foot, or any weapon; also, if the skin is at all broken. Before washing the body, sprinkle them with water, take the heart of an onion, pound it to a pulp and lay it on the injured parts; apply a poultice of grains and vinegar, and in a short time the wounds will be clear and distinct.

A blow from the open hand, though it will not cause death unless on a vital spot, still comes under the heading of "hand." The marks of fingers and a palm will be there, corresponding to the hand. Such are always on the face.

When the instrument of death was a fowling-piece, the aperture of the wound may be examined or even the bones may be examined some time after death. As, however, the belly and bowels soon decompose, and there is no basis of operations in

such a case, take what decomposed flesh there is in the coffin and wash it out with water. If the wound was a gun-wound, there will be shot in it, though you must be very careful that none are inserted purposely by interested parties.

Chapter III.—Wounds from Wooden and Metal Weapons, Brickbats, &c.

Long-shaped, oblique wounds on the bone may be set down to wooden weapons; round wounds with jagged edges, and stab-like three-cornered wounds, to brickbats and tiles. If, however, the wound is oblong, round, oval, &c., the bone smashed and marked with blood, soaking into the middle or even right through to the other side, the colour deep red, approaching to purple and sometimes dark blue or black, then the weapon used was of metal. Such weapons are of various kinds, as life-preservers, knuckle-dusters, iron hands [holding a sharp iron rod pointed like a pencil], shooting stars [i.e., a piece of iron at the end of a string], but the wounds inflicted are similar in character, piercing right into the bone and very severe, not like wounds from wooden weapons, or the hand, foot, &c., which barely reach the bone.

Further, if the bone is broken into irregular splinters and of a light red or red hue, the wound was inflicted by a wooden weapon; but if the splinters are of equal length, broken into an angle one side with the other, and of a dark red or purple hue, the wound was given with some iron weapon.

Chapter IV.—Death from Kicks.

Kicks in the pudenda which cause death may be examined if the corpse is not decomposed, though from the very position of such wounds the examination is less likely to be searching and minute. There is, however, a "bone method" which may be adopted, although there are actually no bones in these parts, and such as there are [in proximity] do not show the wounds. To depend, indeed, on the evidence of the bone immediately below the wound would be to let many criminals slip through the meshes of the law. Where wounds have been thus inflicted, no matter whether on man or woman, the wounds will be visible on the upper half of the body and not on the lower. For instance, they will appear in a male at the roots of either the top or bottom teeth, inside; on the right hand if the wound was on the left, and vice versa; in the middle if the wound was central. In women, the wounds will appear on the gums, right or left as above.

Regarding mortal wounds in the upper or lower abdomen—if in the former, and the flesh is decomposed, you must examine the bone with the square holes, which should be red and purple; if the lower abdomen, examine as for the pudenda.

Chapter V.—Wounds from Knives.

In cases of death from knives, &c., before you arrive at the place of examination, ask your informant (the plaintiff) whether the criminal is caught and what class of man he is, what weapon he used and whether it has been secured. If it has, cause it to be produced that you may note its size, and draw a facsimile of it on paper. If it has not been secured, ask plaintiff where it is and make him draw it and sign his name below the drawing. Also, be very careful to ask if accused is a relative of deceased, and if there was any ill-feeling between them.

Where death has resulted from wounds with knives, &c., the mouth and eyes will be open, the hair disordered, and the hands slightly clenched. The mortal wound will be the largest and longest, the skin shrunk and the flesh exposed; if the belly is cut open the bowels will protrude.

When the murdered man saw his slayer about to strike him with a knife, he naturally stretched forth his hand to ward off the blow; on his hand, therefore, there will be a wound. If, however, the murderer struck him in some fleshy vital part and killed him with one blow, he will have no wound on his hand, but the death-wound will be severe. A cut on the head will sever the hair as if with a knife or scissors. If the skull is fractured at the crown, it was done by some sharp-pointed weapon; this [the

fracture] you must ascertain by pressing down the bone with your finger. A wound from a pointed knife or chopper will be wide at the mouth and narrow inside: a shallow wound from a sword will be narrow, a deep one broad. A wound from the edge of a sword or knife will be narrow at both ends, there being no extraordinary pressure exerted at either. A shallow spear-wound will be narrow, a deep one round, from the spear-*handle* having penetrated. Suppose the weapon was a bamboo spear or a sharp-pointed coolie's pole used against a vital part, the mouth of the wound will be jagged and irregular. Such scars may be of almost any shape. Where death has resulted from a wound with a sharp weapon, the clothes of deceased must be examined to see if there is a cut, and if the blood-stain corresponds with the position of the wound. Where from a knife wound the bowels protrude there will be several cuts upon them. But, it may be asked, how can one blow produce several cuts? Evidently from the peculiar arrangement of the bowels coiled up as they are in the abdomen.

Where death has resulted from blows with weapons, if the weapon was sharp-pointed the wound will have been a stab, but if blunt, not a stab. If the wound was given on the belly, it will be necessary to give the length, breadth, and depth of the wound. If the membrane is pierced and the bowels protrude, there being congealed blood, such a wound was the direct cause of death. So also for the pit of the stomach and the ribs. So too for the throat when the wound has reached and injured the bone, and the parts round about are irregularly lacerated, and the gullet and wind-pipe severed. A wound on the top of the head, or on the temples, or on the back of the head—it would take a heavy, sharp weapon to break the bone—with blood accompanying the scattered brains, must be regarded as a mortal wound, and the actual cause of death.

Wounds are generally inflicted face to face, the weapon being held in the right hand, and are generally inflicted on the left side. For unless held horizontally, the point of the sword could not first come in contact with the right side; but supposing it did, this would be evident from the character of the wound. If the striker is left-handed the wound will be on the right. If it is a case of a sleeping man being wounded, note first the entrance to his sleeping apartment and how the bed was placed. Inquire how deceased was in the habit of lying, and which way his head and feet pointed, and then proceed to examine according to the answers.

Where a man strikes with the hand he is not daily accustomed to use, the blow will fall too high or too low, and will not be even and straight. For instance, a man who is in the habit of using his right hand strikes with his left at the neck of a man lying the wrong way for him, the point of the sword will fall a little too low and slightly wound the right shoulder.[1] [A case is given in the notes of a coroner who fixed on the actual murderer of a man killed in a junk-fight, by making all engaged in the brawl eat before him. When they had finished their meal he dismissed them with the exception of one, to whom he cried in a voice of thunder, "You are the man! Deceased was killed by a wound on his right side, and you alone ate your rice with your left hand." The murderer confessed.]

Where from lapse of time the knife used shows no stains, heat it red hot in a charcoal fire and pour on it some first-rate vinegar, the marks of blood will then appear.

[An instance is given in the notes of a murder being cleared up in the following manner: The coroner who had identified the wounds as inflicted by a sickle, and had found out that a certain man had quarrelled with deceased about a loan of money, went to the village where the suspected man lived and caused every man to produce his sickle laying them on the ground before him. In a little while he turned to the suspected man and accused him of the murder. He denied his guilt with many protestations, but the coroner pointed to the *flies* which had singled out his sickle among seventy others, *attracted by the smell of blood.* The murderer confessed.]

[1] With reference to several obscure passages in the *Hsi Yüan Lu* from which it has been found almost impossible to extract any definite meaning, I would observe that no two native scholars having yet been met with who agree as to the exact interpretation of any one of them, I have elected to shroud such in the obscurity of the original rather than impart shades of meaning not to be identified in the Chinese text.—Reader. "—si quid novisti rectius ipsis Candidus imperti; si non, his utere mecum.

Part II.—To distinguish Knife-wounds given before and after Death.

In examining knife-wounds, the first point is to ascertain clearly if they were inflicted by the edge [i.e., not the point] and also, whether before or after death. A wound inflicted with the edge of the knife before death will be gaping and irregular in its formation; clean, regular wounds may be regarded as inflicted after death. Wounds inflicted before death will be characterized by the presence of clotted blood, and by the fresh-looking appearance of the blood and flesh at the mouth of the wound, death being caused by the rupture of the membrane. The flesh of wounds inflicted after death will be dry and white; there will not be the same appearance of the blood.

Where knife-wounds have been received while life was present, the skin and flesh will shrink; there will be a subcutaneous stain all round. Wherever a limb has been cut off, the muscles, bone, skin, and flesh will be in a sticky mass; the skin shrunk from contact with the blade and the bone protruding. Where a corpse has been cut to pieces the skin and flesh will not change in appearance, there will be no subcutaneous stain, the skin will not have shrunk, there will be no blood at the end of the wound, and it will be of a white colour. If the wound is washed and the two sides pressed together with the fingers and no clear blood flows from it, the wound was not inflicted during life. Where decapitation has been performed during life, the muscles will have shrunk inwards, the skin curled up, and the bone will protrude; the shoulders also will be higher. Performed after death, the neck will become elongated, the skin will not shrink, the bone protrude, or the shoulders become higher.

When it is a case of a body the head and trunk of which are in different places, first make the relatives identify the corpse, and when you have taken its exact position measure how far from the head or feet.

In case of mutilation of limbs, when you have measured how far each is from the body and taken notes accordingly, put all the parts together complete and lay the corpse in a coffin, comparing a few of the limbs with the stumps to see that they agree. If the flesh is not red and if, although there may be the appearances of wounds, there is no blood and marrow, your verdict must be that the mutilation was inflicted after death when the blood had ceased to circulate.

Chapter VI.—Suicide with Knives, &c.

In cases of inquests on suicides, begin by asking your original informant what class of man deceased was, at what time he committed suicide, and what kind of weapon he used. If anyone comes to identify the corpse ask deceased's age and whether when alive he was right or left-handed. If he was a slave, cause the bill of sale, &c., to be produced. Ask too if he has any relatives and whether he was right or left-handed. The wounds also must be carefully examined as to whether they were inflicted before or after death; in the former case there will have been a flow of blood, in the latter case not.

If the throat was cut by the suicide himself before death, the mouth and eyes will be closed and the two hands clenched; the flesh will be yellow and the hair in order; there will be a wound on the neck of a certain length and breadth; the windpipe and gullet will be severed.

The mouth and eyes of all who cut through the lower part of their own throats are closed; their hands are clenched and their arms drawn up closely. The colour of the flesh is yellow and the hair undisturbed.

A small knife will inflict a wound from 1 in. to 2 in. long, a cooking-knife from 3 in. to 4 in. or thereabouts. If crockery is used the wound will be small, though a wound from any sharp-edged weapon will cause death if the windpipe is cut, no matter how slightly. Cuts from sharp weapons on the throat, pit of the stomach, abdomen, ribs, temples, crown of the head and such vital spots, even though not of large size, will cause death if the membrane is pierced. If on the other hand such wounds fare not deep, even though there may be several, death will not ensue.

If the left hand is used the wound will begin from behind the right ear, extending from one to two inches on the other side of the throat, and vice versa. The beginning

of the wound will be of a severer character than the end. [Pain will cause the suicide to relax his grip.]

Where the suicide has cut his throat with one cut and died on the spot the wound will be nearly 2 in. deep, both gullet and windpipe severed; if he lingers a day, about 1½ in., the gullet severed and the windpipe slightly injured; if death results after three days only the gullet will have been severed, the wound being about $1\frac{3}{16}$ in. deep, the hair will be in disorder, and there will not be more than one wound, the suicide not being able to inflict a second. If the hair is in disorder, the wound irregular, with no distinction between its depth at one end and the other, the case is one of murder.

Where the throat has been cut with a knife, the characteristics vary, noticeably in the mouth and eyes. Where suicide was committed in an outbreak of passion, the teeth will be firmly set, the eyes slightly open and looking upwards, from the angry feelings excited. If committed through excess of pent-up rage, the eyes will be closed, but not tightly, the mouth slightly open, and the teeth in the majority of cases not shut, arising from the disturbed state of the mind up to the last moment. If driven to commit suicide from a fear of punishment, the eyes of the dead man will be closed, as also his mouth, for he looks on death merely as a return home and a happy release from the responsibilities of life. If such be the case, inquire carefully whether his disposition while alive was rough or gentle, distinguishing between the three periods of youth, manhood, and old age.

The right hand of a man who cut his throat with that hand will be soft, and one or two days after death will curl up, but the left hand will not curl up, and vice versa. Where death was caused by another person neither hand will curl up.

Where a man has cut off his own hand or a finger the flesh and skin will be evenly cut, and if properly bandaged will not cause death immediately, but such a result will be from want of taking due precautions. The flesh and skin about such a wound will curl inwards, but not if the wound was inflicted after death.

A finger bitten off by oneself will generally cause death because of the poison in the teeth. All round the wound where the bone is broken there will be a quantity of matter; the skin and flesh will be rotten, and death will result from the impossibility of a cure. There will be marks of teeth, and a generally uneven appearance.

Chapter VII.—Suicide by Strangulation.

At an inquest on a suicide by strangulation, begin by asking where, in what street, and in whose house it occurred, what persons saw it, what was used to consummate the act, where the body was suspended, and whether a running or tight noose was tied. Then examine whether deceased's clothes are old or new, measure the position of the corpse, noting which way the face and back pointed, and what deceased stood upon. Measure the distance between the head and whatever the body may be suspended from, as well as that between the feet and the ground. In case of the place of suicide being low, measure the distance between whatever the rope was attached to and the ground. Cut the body down before the assembled people, and carry it to a place where there is plenty of light; then only may you cut the rope from the body, measuring its entire length and the length of the part round the neck; also the circumference of deceased's neck, noting the exact position of the scar. This done proceed to examination.

Begin by asking the original informant what class of man the deceased was, whether he committed suicide early or late in the day, and whether he was cut down in the hope of saving his life; also whether his death was reported early or late. If any one identifies the body, ask him deceased's age, and what was his occupation, what family he had, and what was his reason for committing this act. If deceased was a slave cause his master to produce the documents to that effect, and see if any relatives are mentioned therein, as also his age. Observe carefully the place where deceased committed suicide, and, if already cut down, ask whether when cut down life was extinct or not; also, how much time has elapsed since. Be careful, too, to ascertain accurately the height of the beam. If the feet dangle in the air, the tongue protrude, and the scar round the neck is not a circle, return a verdict of suicide from strangulation, the characteristics being distinct from those of murder.

In examining a case of strangulation where the body is still hanging, notice first the

distance from the ground and from what it may be suspended, also whether the latter would be sufficiently strong for such a purpose. If the body is not actually suspended, note what there is under its feet, what kind of rope was used, and the circumference and diameter of the noose; also observe the breadth of the scar on the neck before cutting down the body and taking it away for examination. If the body has been already taken down, ask if the rope or whatever was used is still on the neck of the deceased or near the body or in the place where the act was committed, for the rope should be carefully compared with the scar. If the weather was wet and muddy, note what deceased had on his feet, and if whatever he stood upon is marked accordingly.

In cases of suicide by strangulation the two eyes will be closed, the lips and mouth black, and the teeth slightly showing. If the rope was above the *Adami pomum* the mouth will be tightly closed, the teeth firmly set, and the tongue pressed against the teeth but not protruding; but if below, the mouth will be open, and about one-third of tongue protruding, the face will be of a purple-red, at the corners of the mouth and on the chest there will be frothy saliva; the hands will be clenched, the thumbs and toes pointing downwards, and there will be marks on the legs as from cauterization; the abdomen will be pendulous and of a livid or black colour, &c., the scar on the neck will be purple and red, or black, as if from a bruise, extending from the back of one ear to the back of the other, and measuring from 9 in. to 1 ft. and upwards.

If deceased's feet were off the ground, the scar under the Adam's apple will be deep; otherwise not so deep. It will be deep if deceased was fat and the rope thin; less so if he was thin or the rope thick. Where a rope of calico or cloth is used the scar will be spread over a larger surface. If the body was hanging at an acute angle with the ground [feet touching, of course], or lying on the ground, the scar will be oblique, not reaching to the hair at the back.

Whatever kind of knot is used it will be necessary to observe what deceased stood upon, and whether when the noose was made there would be enough rope over.

With a running or tight knot death may result if the feet touch the ground, even if the knees touch; with the single-twist cross knot the feet must be entirely off the ground. The single-twist cross knot is used when deceased first tied the rope round his neck and then attaches it to something high up; it will be necessary to observe the dust on whatever that was, and also to note what deceased stood upon, and whether he could have reached up to attach the rope himself. Observe carefully if the rope be stretched or not, and that there is at least a foot between deceased's head and the beam. For if the head be close up against whatever it may be, the feet dangling in the air, and there is nothing deceased could have stood upon, then the hanging of the body was the act of some other person.

Where, in a case of self-strangulation, there is no mark of the knot, it will be because deceased first wound the rope several times round his own neck and then, attaching it to something high, let himself swing till he died. Or else when he had suspended the rope, he hanged himself in a double noose, standing on something high, and thrusting his head into the noose, with an extra turn or so round his neck. The scar resulting, therefore, will be double, the top scar passing upwards from behind the ear towards the hair without crossing, the lower one encircling the neck. Such distinctions should be carefully drawn in reporting the case.

Where there is no mark of a knot on the throat of a suicide by strangulation, there will be in the middle of the scar, on either side of the chin, a faint mark extending towards the ears on either side, with a tendency to diminish gradually the higher it gets. Where a single cord has been used, there will be on either side of the knot a subcutaneous appearance of blood rising obliquely upwards and continuous, not extending in a straight line backwards.

After suspension, circulation ceases, and the body becomes purple and black like collected clouds, or as if in a state of decomposition, but differing in appearance from a red and livid body swollen from beating, or one discoloured in large patches from the effects of poison. In an old or emaciated man this would be less apparent.

Where there has been long illness without hope of recovery and a desire to die as soon as possible, and the sick man lying on his back strangles himself with a rope or girdle, the eyes will be closed, the teeth showing, a small portion of the tongue will protrude, bitten by the teeth, the colour of the flesh will be yellow, the body thin, the hands clenched, &c. The rope or whatever was used will probably be found grasped by the hand of the suicide, the distance of which one from the other must be accurately measured. The scar round the neck will be purple and red, a foot or more in length; the knot will be on the lower part of the throat, and the scar will be deepest in front. If the rope has been already cut away, the belly of the corpse will have swelled, and the tongue will not be bitten.

Beneath the spot where suicide has been committed by hanging, dig a hole three or more feet in depth; if charcoal is present, that was the place. [The note says this results from the natural influence of the dead body over the ground, and must not be regarded as anything extraordinary.]

Where suicide was committed in a room, observe carefully the dust on the beam or whatever the rope was attached to. If it is much scattered, this may be considered in most cases as bona-fide evidence of suicide; but if there is only a single rope-mark, a contrary conclusion follows.

Where suicide was committed in a low place, the body will generally be in a recumbent position, either on its side or face. If the former, the scar will be oblique but horizontal beneath the neck; if the latter, the scar will be straight up from beneath the throat, reaching from behind one ear to the back of the other, but not extending to the hair.

Tap lightly with a stick on the suspending rope; if it is tight, the case is one of suicide; if loose, the body has been hung up there by others. Generally, when a body has been thus brought from elsewhere and hung up, there are two scars, the old one being purple and red with a subcutaneous appearance of blood, the other white without that appearance. The purple and red scar will also be deep, though instances occur of depth unaccompanied by the purple and red discoloration. A white scar is, however, unmistakeable evidence of the body being removed from elsewhere.

In cases where from lapse of time the body has become decomposed and the head alone remains attached to the rope, the body having fallen to the ground and the bones become exposed from the rotting of the flesh, it only remains to see whether the rope is in the channel beneath the jaws and whether the two wrists and the bones of the forehead are red. If so, it is a case of suicide.

Chapter VIII.—Strangulation with Violence passed off as Suicide.

Wherever a man has been strangled or otherwise killed, and it is pretended that he commited suicide by hanging, the eyes and mouth will be open, the hands apart, the hair in disorder, circulation will have stopped in the lower part of the throat, the scar will be shallow and faint-coloured, the tongue will not protrude or even be pressed against the teeth, the flesh on the neck will show marks of finger-nails and elsewhere on the body there will be mortal wounds.

Where the victim was half-strangled and then hung up, there will be two scars, one deep and the other shallow, very like in appearance to those which a suicide himself may cause by giving the rope an extra turn round his neck, though the latter are both deep; but in this case the scars are half red and half white and the subcutaneous appearance of blood is not similar.

Wherever a man has been strangled from behind something, as a window or a tree, the same being passed off as suicide, the rope will not have crossed, the scar on the throat will be regular but deep and of a dull black colour, and not beginning behind the ears, &c. Where a man has been strangled on the lower part of his throat, the knot will be at the back of his neck, his two hands will not hang down, or at any rate not straight down. Strangling from behind is generally against a pillar or something of the kind, and often with a piece or part of some garment, the mark being left on the lower part of the throat which, being a vital spot, death ensues from the stoppage of respiration.

Where murder has been committed by strangulation, the rope having been passed

several times round deceased's neck, the knot will generally be at the back in the middle or slightly inclined to either side; there will be some rope over, hanging down, and the corpse will be lying on its face. From the struggle which he made for his life, deceased's hair will be in disorder, and over his body will be found the marks of bruises, scuffling, &c.

Where the hands, feet or neck of corpse have been tied round with rope, because the man is already dead and circulation has stopped, there will be no subcutaneous appearance of blood, and although the cords have penetrated deeply, the scar will not be livid or red, but white.

Where a scar has been burnt in with a hot iron, the mark will be red or scorched and moist.

Where a man has been beaten and finally killed by strangulation, there will be a dark mark on the lower part of the throat, six or seven inches long, not reaching to the back of the neck.

Where a man has been strangled in the ordinary way, there will be a black scar all round the throat over a foot in length.

Sometimes a man is strangled on another's back [the rope being over the murderer's shoulder], in which case there will be traces of the rope having been crossed, the scar will be straight towards the back of the neck, the extremities falling a little and gradually shading off, and generally on the lower part of the throat, not on the chin or under jaws. For in strangling a man over the shoulder on the back, unless the feet are raised off the ground, death would not immediately ensue.

Chapter IX.—Death from Drowning.

In cases of death from drowning, begin by asking the original informant if it was early or late when he saw the body in the water, and whether it was in the very spot it now is [or whence it has just been taken], or whether it floated down from elsewhere and if so from what direction and how it came to stop in this place. If witness says he saw deceased fall into the water, ask whether he pulled him out or not and, if he did so whether he reported the case instantly or after some delay.

If, either in a river or a lake, it is difficult to take the proper measurements as to the position of the body, observe the spot where it was floating; if it was not floating but raised and taken out, inquire for the spot whence it was thus raised. Pools or pits with water in them deep enough to drown people, should be fathomed in order to get the depth, and their position should be taken; moreover, wherever a body is found floating or is thrown up in river or stream, lake or pond, the name of the place should be ascertained as well as that of the tenant or occupier.

Where a body has been many days in the water it will have swelled up and the direct cause of death will not be distinct. In such cases report that the hair has fallen off, the skin peeled off, that the head is swollen, the lips are turned back and opened, the skin and flesh over the whole body is livid or black; and let your verdict be that these appearances arise from being so long in the water, well or river. as the case may be; that you have found traces of froth in the mouth and nose of deceased, that the abdomen has swelled up, and that whether or no there are any other injuries on the body there is no evidence to show.

In the case of the body of a drowned man, where it is no question of blows, having on the head or face wounds from sharp weapons, it will be necessary to empty out the water in order to see if there is any sharp thing, either metal or crockery, against which he might have struck. For striking against any such thing before life was extinct would cause a flow of blood, and the wound would closely resemble one inflicted before death, which mistake however, must be carefully guarded against. So also for suicides in wells.

If it is a case of a slave or a wife who before throwing herself into the water received wounds from beating, in pronouncing a verdict of accidental death or suicide, &c., it will be necessary to enter the wounds in the prescribed form, noting that they were inflicted before deceased jumped into the water. Bodies take a long time to float in the cold weather at the beginning of spring, a shorter time in spring, summer, and towards the end of autumn.

If examination be delayed and the corpse is exposed to wind and sun, the skin all over it will rise and form into white blisters.

Where the water is at all deep and broad, the suicide or murdered man as the case may be, will show no bruises from striking against anything; but if the place be shallow and narrow, the result will be similar to jumping or being pushed into a well. Generally speaking, water three or four feet in depth is enough to drown people, and if there is no other apparent cause, your verdict may be "Death by drowning." But if there is a rope attached to the body or any other suspicious appearances, then it is a case of murder rather than suicide.

If deceased slipped and fell into the water, the mouth and eyes will be both open, the hands will not be clenched, and if the place was narrow, there will be wounds on the head and face.

Where suicide by drowning is committed in consequence of illness, the depth of the water is an unimportant matter as almost any depth would cause death. There will be no other marks about the body, except a slight yellowness in colour.

Where from illness deceased fell into a ditch and was drowned, the mouth and eyes will be open, and the hands slightly clenched. Wash the parts which have been in the water and mud first with water, and then spurt wine over them from the mouth. The flesh will be rather white, the skin of the abdomen slightly swelled, and mud in the finger-nails which will not come out with washing.

An old man may be held under water and so suffocated, but in this case there will be no marks on the body and the belly will not swell.

Part II.—To distinguish between Bodies of drowned Persons, and those thrown in after Death.

The body of a drowned man floats on its face[1] though not if there is any silver money about it; the body of a woman floats on its back with the face looking upwards, the hands and feet pointing forwards, the mouth closed, the eyes either open or shut, the belly swelled, and sounding if struck with the hand. [Where deceased *fell* into the water, the hands will be open, the eyes slightly open, and the skin of the abdomen slightly swelled up; but if he jumped into the water, the hands will be clenched, the eyes closed, and the bowels much swelled.] The skin beneath the feet will be white and wrinkled, the hair not in any way loose. Both the head and hair as well as the finger-nails and toe-nails, or if shoes were worn, these as well, will be full of sand and mud. [The note says sand and mud are sucked into the nose and mouth of a drowning man by his efforts to draw breath, which of course is not the case with a body thrown into the water after death.] If in the nose and mouth there is froth, and faint traces of blood specks, or if there are any marks of injuries from striking against anything, these are evidences of death from drowning.

If death was the result of illness and the body was subsequently thrown into the water, there will be no traces of froth in the nose or mouth, and there being no water in the belly it will not be swelled up. The face will be slightly yellow, and the body thin.

Where a man has been killed by blows and then thrust into the water, the colour of his flesh will be rather yellow than white, the mouth and eyes open, his arms stretched out, his hair in disorder, the skin of the abdomen will not have swelled up, no water will flow either from the mouth, eyes, ears, or nose, there will be no sand or mud in the finger-nails, the hands will not be clenched, the soles of the feet will not be wrinkled or white but puffed out, and the mortal wounds on the body will be of a black colour. Some corpses are fat and others lean; observe this at the time of examination, and take note of any wounds there may be, for the presence of such, even though it be a case of suicide, renders a careful investigation imperative.

If there are no traces of wounds on the body, but the face is purple or red and the mouth and eyes open, death resulted from deceased being held in the water by his feet with his head downwards.

[1] It is curious to note that the contrar belief prevailed among the ancients.

Chapter X.—Death by Drowning in a Well.

In cases of drowning in wells begin by asking the original informant why, when he first saw there was somebody in the well, he did not attempt a rescue; and how, before the body floated, he knew there was one there at all. If the well is one attached to the house ask how, deceased being missing the whole day, he came to know he was in the well. Wherever there is a man in a well, the fact will be first apparent by there being frothy bubbles on the surface, and this may serve as a guide in investigations. Take the exact position of the well, the name of the man on whose ground it is and the name of the place. If the body was found at the bottom of the well it will not be necessary to take the position, but only to get an approximate measurement of the depth before taking out the body.

A body in a well which has swelled up will show about a foot out of water, but this will not be the case if the water is shallow. If there is any part above water, observe whether the head is uppermost or the feet, and measure accordingly; but if not, then with the measuring rod take the distance down to the body, noting also whether the head is uppermost or the feet.

Wherever any one has jumped, been pushed, or fallen into a well, there will be wounds on the head from striking against or grazing the bricks, his finger-nails will be full of sand and mud, his belly swelled, and if laid on his side or face, water will come out of his mouth. Where a man has been pushed or fallen into a well, his hands will be stretched out, his eyes partly open, and perhaps he may have money about him or valuables, but if he jumped in to commit suicide, his eyes will be closed, his hands clenched, and there will be no money about his person.

Suicides generally jump in feet first; the contrary is the result of falling in under pursuit or being pushed in. Where a man has slipped and fallen into a well, his mouth and eyes will be open. Carefully observe the marks on the ground where he slipped.

In the 5th and 6th moons there is, both in wells and in grave mounds, foul air destructive to human life, and when in summer or autumn, the water being gone, men descend to clean out these wells, death is often the result of inhaling this poisonous air. This fact must be kept in mind at inquests on suicides.

Chapter XI.—Death by Burning.

In cases of death by burning, begin by asking the original informant where the fire began, where deceased was at the time and why; also whether any assistance was rendered to him, and whether he had had any fight or quarrel with another person. When all this is clear, you may proceed to examination. It may happen that the hair is burnt up, the head and entire trunk are scorched black, and that it is impossible to say whether or not there may be on the body any other causes of death, or to specify deceased's death and facial appearance. State clearly whether in the mouth or nose there is any trace of ashes, for this is the crucial test of death by burning.

In examining a body where death has resulted from burning, first observe if there are tiles, straw, and ashes beneath it or not; for where a man has been burned in a house roofed with tiles or straw, the body will be found underneath these, whereas if he has been forced in by another man out of revenge, &c., his body will be found uppermost. Also note the direction of the head and feet.

Where a body has been burnt to cinders and there is nothing left to examine, take the evidence of your assistants and the neighbours to that effect; then having closely investigated the circumstances of the burning, report that nothing of the body remains for examination, and determine the case according to the evidence.

Part II.—To Distinguish between Bodies Burnt Before and After Death.

Where the burning took place before death, there will be in the mouth and nose of the deceased a sooty-like ash; the hands and feet will be drawn up. If burning

after death, although the hands and feet will be drawn up, there will be no ashes in the mouth; and if the elbows and knees are not burnt, the arms and legs will not be drawn up.

It is a common saying that muscles are the connecting links in the human body, and this is more than ever true of their relation to the bones. Corpses of persons who have been burnt are indeed generally found upon their faces, but if burnt upon the back the corpse rises into a sitting posture from the contraction of the muscles, often frightening people thus. Therefore the fact of the hands and feet being drawn up is not sufficient guarantee that the body was burnt either before or after death; but as a general principle it may be held that if the colour of the body after burning is scorched and black, death had already taken place, and that if the bones are yellow and greasy death resulted from the burning.

The bones of a man who has been burnt to death will sound if let fall; those of a corpse burnt after death will not.

If fire be applied to wounds the skin will not blister; the flesh beneath will be purple and red.

Where an invalid of long standing has been accidentally burnt to death, the colour of the flesh will be scorched and black, and the flesh itself perhaps shrivelled up; the hands and arms will be drawn up on the breast, the knees also drawn up, the eyes and mouth open, or perhaps the teeth clenched or biting the lips; there will be a yellowish fat bursting through the skin and flesh.

Where a man has been strangled and subsequently thrown into the fire, his hair will be scorched and yellow, his head and whole body scorched black, his skin and flesh shrivelled; there will be no watery blisters or broken places on his body. On the lower part of the throat there will be the marks of strangling.

If deceased was killed with some sharp-edged weapon, and it is pretended that he was burnt to death, bid your assistants pick up the bones and sweep up the ashes and dust. Then upon the clean spot where the body lay sprinkle the whey of grains and vinegar, and if deceased was murdered with a knife or similar weapon there will be a fresh coloured blood-stain where the blood soaked into the ground. But first ask where deceased slept the last night of his life, for fear the body might have been moved after death, in which case there would be some difficulty in basing a decision on such evidence.

In cases of death from burning, wherever the victim fell there will be below the surface the outline of his body. It will be necessary to sweep away the ashes, &c., from the spot, sprinkle vinegar and cover it over for a little while with mats, when it will appear to view. Where a man has been strangled, and thrown into a fire, the inside of his throat will not be burnt, the scar will preserve it. Where a man has been killed with a knife or similar instrument, there will be blood on the ground. The place must be swept and vinegar sprinkled, when a fresh-coloured red will be the result.

Chapter XII.—Death from Scalding.

In cases of wounds from scalds the skin and flesh of deceased will be broken, the skin peeled off and white, showing white flesh below. The flesh generally will be rotten and red.

In most cases of scalding and burning the wounds are on the victim's hands, feet, or breast. If any blows were struck, or he was knocked into the hot water or fire with the head, the foot, or the hand, the wounds will be on the back of either forearm, or on the thighs, &c. Blisters will not rise on injured parts, which are quite different from scalds, &c.

Scalds, unless over the heart before or behind, are not mortal. They are principally on the head and ribs, the hands and feet, more severe in some parts and less so in others. Self-inflicted scalds are chiefly on the hands and feet, the chest and upper part of the back.

A corpse which has been scalded turns white, but is not otherwise affected; neither do blisters rise on it.

BOOK III.

Chapter I.—Miscellaneous Remarks on Suspicious Appearances.

At an inquest regard nothing unimportant. The adage says—

A difference of a hair will be
The difference of a thousand *li*.

For often it is difficult to tell whether a man has been strangled or whether he committed suicide by hanging himself; whether he was accidentally drowned or threw himself into the water; whether he died from the effects of blows received or fell a victim to disease. Besides which, slaves frequently commit suicide in one way or another in consequence of being beaten, &c., necessitating a prompt and searching investigation into the ever-varying circumstances which thus create suspicion.

In examining a corpse bearing suspicious marks upon it, if the weapon was sharp-edged and penetrated right through, note both where it went and that where it came out. If the body is decomposed, compare the position of the wound on the body with the mark on the clothes deceased was wearing. Suppose deceased to be lying on his face and with some short weapon or a pointed piece of bamboo in his hand, in certain cases it is quite possible he may have fallen down in a fit of drunkenness and inflicted the wounds on himself. If he is lying near some high place or in mud, examine carefully if there are any other marks or any money about the body or signs of injuries whatsoever.

Where a man has been killed, but from lapse of time the body has decomposed and there remains nothing certain to go by, examine the bone between the top of the head and the forehead, commonly called "the cover of the mind." This will have started up, leaving a slight fissure, and will be of a faint red or livid colour, caused by the rush of blood thither in consequence of the stoppage of respiration. Examination of this bone will make the case clear.

The "door of life" bone is very tender, and death may result from its being struck by the hand. For on either side of it there are two holes through which runs a red muscle as thin as silk connecting the two kidneys; death would ensue if this were broken, but there would be no trace of the injury on the surface of the body. If death is stated to have been caused by striking the "door of life," examine the bone, which in such case should be purple and red. The "door of life" bone extends from the os sacrum upwards to the seventh joint in the spine; there is a small hole on either side by which it may be known.

In all cases where there are no marks of wounds, but the face is of a dull, livid colour, and perhaps looking as if was swelled on one side, you may be almost sure that death was the result of something being pressed over the mouth and nose, or from strangulation with a handkerchief, girdle, or something of that kind, there being, consequently, no wounds visible. Observe if the skin on the neck is hard or not, as this a very important place; also if the hands and feet show any signs of being tied, the tongue of being bitten, the pudenda, &c., of being stamped upon. If you find none of these marks, then see if there is any saliva in the mouth, and if the neck is swollen or not. If there is saliva accompanied by swelling, death was probably the result of diphtheria, or perhaps occurred in a fit of drunkenness.

It often happens that a man after fighting and separating goes either to wash his face or get a drink in whatever river, pond or pool may be near, and then from sheer exhaustion—sometimes from drunkenness which in the first instance caused him to fight—gets giddy, slips and falls, into the water and is drowned. As he was alive when he fell into the water, the abdomen will swell, there will be sand and mud in his finger-nails, and his two arms will be stretched forward; these are evidences of death by drowning, and, although wounds may be found on the body which must be entered on the form, your verdict should be Death by Drowning after a fight. For although the wounds may be on vital spots, there still remains the death-limit, and death from other causes outside the death-limit, both of which come under the head of death from wounds; but in the present case death was the result of falling into the water, and, although there are wounds, must be considered as proceeding from other causes

Further, it very frequently happens that subsequent to a fight a man falls down in getting up on to some high place and is killed. Examine the height of the place, and whether the injuries which resulted are or are not on vital spots. Also closely question those who saw the fight and its termination.

Where a man dies in consequence of blows received in a fight but has no wounds visible, it may be that he was suffering from a disease of long standing, or had taken too much to drink before he began to fight, and then from striking against something during the fight, injured himself so that he died.

Si res ita se habeat, testes ambo in uterum contracti non poterunt cerni. Linum sume calido aceto mersum, quod super infimam abdominis partem spatium caenae imponas. Hoc facto et abdomine manu oppresso, testes statim descendent. Si senex et aegrotus qui ita pugna occisus est, vulneribus non apparentibus, scrotum maxima cura inspicere debet; nam saepe unus tantum testis in uterum contrahitur, qui abdomine manu oppresso statim descendet.

In cases where there are no marks of injuries and no signs of illness, making it very difficult to arrive at any verdict, the chances are that some sharp weapon will have been passed through the crown of the head and perhaps into the brain, thus causing death. When the relatives and witnesses have drawn up their statements of the case, proceed to cut off deceased's hair and examine.

Examine the teeth, tongue, ears and nose of murdered men, as well as the nails on their hands and feet, to see if anything sharp has been thrust into them.

In all cases where your verdict is death by poison, self-strangulation, or jumping into water *after* being beaten, it is necessary to make a searching personal examination. If it is a case of poison being put in the mouth of a man who has been beaten to death as if he actually took it, hanging up a body and pretending it was suicide by hanging, or throwing a body into the water and pretending it was suicide by drowning, the slightest mistake may entail most serious consequences. Carefully note the wounds on deceased's body; if these are not on vital places, but the self-strangulation, drowning, or poisoning are apparently bona-fide, then your report may be worded to that effect.

If it is a case of murder by night, and there are no witnesses or the corpse is not forthcoming, it will be necessary to institute secret inquiries after the plaint has been made; it will not do to act upon guesses or to cook up evidence to suit the case and make it complete.

Bribery.

In all cases where prosecutor and accused are men of the slightest influence or position, choose experienced and trusty assistants, taking them along with you and never letting them out of your sight even to eat or drink, unless watched by some one, but adjourn the case for a while until they may have finished. Unless you do this, these men will be tampered with, and there will be an end to all justice or redress. This cannot be too cautiously guarded against.

Chapter II.—Miscellaneous Remarks on Wounds.

Disease.

Whenever an inquest is held over the body of a man who has died from disease, ask first where deceased came from, and then, whether anybody knows him or not, what disease he had, how old he was, and how long ill. If deceased was a slave, inquire for the bill of sale, &c., ask whether he has any relatives, from whom he received medical advice, and what medicine he took. Then proceed to give your verdict in the presence of all parties. If there are no other causes of death, report that the body is of a yellow colour, and nothing but a bag of bones, which could only come about before death; mention also what disease was the cause of death, taking a certificate from the doctor to that effect. If deceased, according to the evidence of all, died of illness and came to his end by no unfair means, no one can apply for a further inquest.

Wherever it is a case of death from illness, the body is emaciated and the colour of the flesh a faded yellow, the mouth and eyes in most cases are shut, the belly sunk in, the two eyes quite yellow, the two hands slightly clenched, the hair in disorder, and

perhaps there may be recent or old marks of cauterization upon the body. If no further marks are found upon the corpse, death resulted from disease.

Hunger.

If death has happened at the road-side from hunger, the body will be emaciated and of a dull yellow colour, the mouth and eyes shut, the hands slightly clenched, the mouth and teeth of a smoky yellow colour, and the lips will not cover the teeth.

Sudden Death.

In cases of sudden death in spring, summer, or the beginning of autumn, there appears after two or three days a slightly livid hue on the lower part of the belly and at the fissures between the ribs. This arises from decomposition after death, the foul matter distributing itself and occupying parts of the skin and flesh; it has nothing to do with the causes of death.

Fright.

The corpses of people frightened to death by devils, goblins, &c., will generally be fat and of a slightly yellow hue, the mouth and eyes will be shut, and in the former there will be frothy saliva, but no marks of violence upon the body.

In cases of sudden death the flesh will not have sunk away, in the mouth and nose there will be frothy saliva and the face will be purple and red; this will be the result of the saliva having collected and choked up the windpipe.

Where death has been sudden from fright, &c., the eyes will be open and the pupils white, the mouth will be closed and the teeth firmly set, sometimes the eyes and mouth will be drawn down on one side or the other, from the corners of the mouth and the nostrils there will be a flow of frothy saliva, and the feet and hands will be drawn up.

In cases of death from fright the corpse will be fat and generally of a glossy white, the mouth and eyes closed, with a copious flow of saliva. Sudden death and death by supernatural agency will not be characterized by fatness or the reverse, but the two hands will be clenched, and the nails of the hands and feet will generally be dark blue. Where death has been the result of fright the mouth and eyes will usually be deflected, the hands and feet drawn up, the arms, legs, hands and feet shrunk, and there will be a flow of saliva.

Catching Cold.

Where death resulted from catching cold, the whole body will be red and purple, the mouth and eyes open, with a purple liquid oozing out, the lips will be slightly open and the hands not clenched or drawn up.

Where death has resulted from atmospheric causes, the eyes and mouth will be open, the whole body of a yellow colour, with a slight separation of the skin, and both hands and feet stretched out.

Where death has resulted from red eruption there will be small red spots scattered here and there on the body without much swelling.

Heat Apoplexy.

Heat apoplexy is generally fatal in the 5th, 6th, and 7th moons; the tongue does not protrude, nor the bowels; the face is white and yellow, and sometimes from the nose, &c., there is a discharge of blood.

Cold.

Where a man has been frozen to death, the face will be of a dull yellow, there will be freely saliva in the mouth, the teeth will be hard [closed], the body stiff, and the two arms drawn up on the chest. In washing the body with wine, at the first contact with the hot steam the jaws will become red and the face will become the colour of the *Hibiscus mutabilis*. Saliva will flow from the mouth, but it will not be sticky.

Hunger.

In cases of death from hunger the whole body will be dark-coloured, thin, hard, and rigid. The eyes will be closed, the mouth open, the teeth tightly set, the belly sunk in, and the hands and feet stretched out.

Fright.

In cases of death from fright, the eyes will be staring, the mouth open, the two arms stretched out as if in terror.

Repletion.

Where death has resulted from over-eating or drinking, begin by making the assistants wash the body with vinegar; then, if there are no wounds, pat the belly with your hand, and if it is swelled up and sounds like a drum, death was caused by repletion, and by the wine penetrating into the heart and lungs. Make the relatives depose as to the amount of wine usually necessary to make deceased drunk, as also to the quantity he took on the present occasion.

Where death has resulted from drinking samshoo, the teeth rattle about and easily drop out, the body is soft, and a watery fluid comes from the mouth and nose. Samshoo should not be heated over the fire in a pewter vessel, or left to stand all night; if thus left for a few days, it may cause death to whoever drinks it, the face turning a dull livid colour.

[Where death has resulted from getting drunk off samshoo, life may be restored by pouring down the throat water collected from the lids of saucepans and cooking-pots.]

Trampled on.

Where deceased has been stamped upon and injured internally so as to cause death after over-indulgence in eating and drinking, the appearance of the body will not be a reliable guide. If there were no other causes of death, ejection of food at the mouth, nose, &c., will at any rate be some indication. Make careful inquiries whether deceased had fought with anybody and thus came to be trampled upon before you give your verdict.

Cauterization.

In cases of cauterization with a needle, it will be necessary to obtain the certificate of another doctor as to whether death may not have been caused by the operation being performed in a wrong spot. If the five viscera are short of the *yin* (female principle), and acupuncture is performed (from a faulty diagnosis) with a view to add more *yang* (male principle), this is called *chung chieh*. Death is sure to result, the patient dying quietly. The opposite of this is called *ni chüeh*, and is equally fatal, but death is not peaceful.

Homo qui sibi ultra modum indulgeat, semine exhausto in ipso feminae corpore moritur. Falso verum diagnoscere perfacile est, hoc erecto, illud dejecto membro.

When men and women die of irregularities of the *yin* and *yang*, their lips and nails will be of a dull livid colour; in severe cases the whole body will be purple, arising from exhaustion of the latter (vital fluid) or coagulation of the former (blood).

Where death has resulted from an irregularity of the *yin*, the corpse will be of a dull livid or black colour, resembling a case of poisoning, but of a fainter hue. The mouth and eyes will be shut, the two hands closed, &c., &c.

Bambooing.

Where death has resulted from corporal punishment with the bamboo, the marks should be carefully examined as to their breadth, and it should be noted if the scrotum, ribs, waist, or lower abdomen show any subcutaneous appearance of blood.

The smaller bamboo will leave scars 3 in. long by 2½ in. on the left, and 3½ in. long by 3 in. on the right side. Depth, $\frac{3}{10}$ of an inch on either side.

The large bamboo will leave scars from 3 in. to 3½ in. either way on both sides, and $\frac{1}{10}$ of an inch deep. There will be matter in and around the wounds, and the flesh will be rotten.

It sometimes happens that people die after a bambooing from quite other causes. If the front of the thighs and the lower abdomen is slightly red, do not look on this as a subcutaneous appearance resulting from infliction of the bamboo; for very possibly it came from lying on something hard while being punished, and appeared as might be expected after death. This requires care at the time of examination.

If there are slanting, broken wounds on both sides, several inches long by something less than an inch broad, and reaching down to the bone, with a scab, death resulted from deceased catching cold in wounds inflicted with the bamboo.

Falling from a Height.

Where death has resulted from falling down off a tree or house, notice the branches, leaves, &c., the height of the house, the place where deceased's foot slipped, the depth of the mark in the ground, and the particular nature of the wound which caused death. If the injury was internal, blood will flow from the mouth, eyes, ears and nose. If the wound is very severe, be even more careful in your examination, and measure the exact height of the place from which deceased fell.

In cases of falling down either from slipping or tripping, the force being low down, the wounds are consequently on the leg, foot, or arm. Such, whether on the right or left, only injure the limb half way round. If it is a case of being pushed down, then the force, being high up, the wounds will be chiefly on the head and the two wrists. For pushing implies the exertion of force, and the head is the heaviest part of the human body. Thus, if a person was pushed down while he was expecting it, his hands would probably touch the ground first, but, if taken unawares, his head. Although it does not necessarily follow that the wounds are more extensive, they are distinguishable from wounds received in falling down.

Crushed to Death.

Where a man has been crushed to death the eyes and tongue will protrude, the hands will be slightly clenched, all over the body there will be traces of dead blood of a dull purple colour; from the nose blood will flow, or a watery fluid; the injured parts will be red and swollen with a subcutaneous appearance of blood; the parts where the skin has been broken will be red and swollen, and perhaps the bones and muscles broken. The above refers to crushing on a vital spot, for if the spot be not vital, death will not ensue; neither will a body which has been crushed after death present these appearances.

Where a man has been crushed to death by a house or wall falling on him, or by a stone, the length and breadth of wounds on the fleshy parts of the body must be reported, and that they were inflicted by something hard; note also if the bones are broken or not. If death was caused by a falling tree, note the size of the wound thus inflicted, which should be oblique and corresponding to the dimensions of the tree. Crushing after death has not the same characteristics.

Where two men are carrying anything on a pole, one strong and and the other weak, and the weak one gets crushed, the shoulder as well as the hand and foot on the same side will all be more or less injured. Where the crushing resulted from accidentally rushing up against anything, observe carefully whether the wounds are at the front or on the back, on the right or left side. If on the back, then on the front part of the body there will be traces of injury from knocking against something, and vice versa. So also with regard to the right and left sides.

Choking or Gagging.

Wherever death resulted from the forcible stoppage of respiration, the eyes will be open and the eyeballs starting out; from the mouth and nose a watery blood will flow; all over the face there will be a subcutaneous appearance of blood of a dark brown-red colour; the bowels will protrude, &c.

Where the mouth and nose have been gagged with clothes or wet paper and death has resulted therefrom, the abdomen will be swelled up and dry.

Wherever a man has been choked by other means, his two hands, the back of his arms either above or below, his heel-bones and chest will all show slight wounds. For to choke a man so that his eyeballs start out requires considerable pressure, and though the body may be kept immovable, the hands and feet must be to a certain extent free. But the hands and feet may have been tied, and it will be necessary to see if there are any such marks.

Held up head downwards.

Supposing a man is made drunk and laid down on a carpet or rug, then, when he is sound asleep, rolled up in it and held up head downwards, death will ensue very quickly, but there will be no traces of blood from the mouth and eyes, or, if there are, a little water will soon remove them. There will, however, be a strong exhalation of wine fumes.

Held up head downwards in a tub of water and lime.

Sometimes two tubs, such as are frequently covered one over the other and about the height of a man, are used, one being first filled with water in which a quantity of lime is mixed up, and then the victim is thrust in head downwards, the other tub being used to cover him up. Death soon ensues. This is called "going out for a sail." When the body is subsequently washed no wound of any kind will be visible, the face will be yellow and white, as if death had occurred from disease. The flow of blood which might naturally be expected will be arrested by the lime, as also the blood which would have collected and congealed in the face will be similarly dispersed. This can only be substantiated by "bone examination" of the shell of the brain, for the grits of the lime passing through the mouth and nose, which can be washed clean, penetrate right into the brain and may be found sunk in it. From such evidence as this there is no appeal.

Prodding.

Where a man has died through being prodded with something hard, there will be a subcutaneous appearance of blood at the back of the ribs, purply red and swollen, three or four inches square or round, the skin not broken. Feel with the hand if any injury has been done to the muscles or bone. This is a non-resisting vital spot.

Trampling of Horses, &c.

Where a man has been trampled to death by a horse, the colour of the body will be slightly yellow, the two arms stretched out, the hair in disorder, blood will flow from the mouth and nose, and the injuries will be of a black colour. Where a vital spot has been trampled upon and death caused thereby, the bones will be broken and the bowels will protrude. If a man is merely knocked down or trampled upon in some non-vital spot, the skin will be broken and the scar will be red and black, but death will not result. The mark of a donkey's hoof is small, and wounds from cow's horns are generally in the pit of the stomach or in the abdomen or ribs. If the skin is not broken, the wound will be red and swollen.

The wounds inflicted by the trampling of men, horses, donkeys and mules are distinguishable by their number and severity. A horse moves with great force, inflicting few wounds but breaking the bones and perhaps forcing out the bowels. Where a prostrate man is trampled upon by a number of horses, the wounds will be many, but not so severe as where a man is knocked down by a horse galloping. If trampled on by men the injuries will be in long-shaped patches, severe at one end and slight at the other. Where many people trample over a man, preventing him from rising, the severity and size of the wounds will vary. The wounds inflicted thus by donkeys and mules are not only smaller than those inflicted by horses, but the halo round the wound is distinctly visible. If a man is gored unawares by an ox, the wound is generally

in front on the ribs, lower abdomen, or pit of the stomach. If an ox suddenly rushes at a man so that he has not time to get out of the way, the wound will generally be on the back or ribs.

Crushed.

Where a man has been crushed by a cart-wheel, the colour of the corpse will be slightly yellow, the mouth and eyes open, the hands slightly clenched, the hair tightly plaited. [This applies only to instant death.]

Run over.

Death generally results from the wheel passing over some vital spot such as the chest or ribs; if the part is not vital, death will not ensue.

A cart may pass over a man either crosswise or lengthwise; crosswise when a man, passing before a cart going too fast to be pulled up, falls down and the cart goes over and kills him. The injury will be either on the neck, head, or chest, back or ribs, and possibly on the arms or legs. If a man is run over by a cart meeting him face to face, the wounds will be on the arms, legs, or ribs, but only on *one* side, and passing along the body in a straight line. The wounds will be long and generally on the front part of the body. If run over from behind, the wounds will be much the same but on the back.

Lightning.

Where a man has been struck by lightning the body will be of a scorched yellow colour, soft and black. The two hands will be partially clenched, the mouth open and the eyes starting out, the hair behind scorched and yellow, and generally in disorder. At the burnt places the skin and flesh will be hard and shrivelled, the clothes on the body burnt to cinders, or perhaps not burnt. The injuries are generally on the head at the back, the sutures open, and the hair looks as if it had been singed off from top to bottom. Occasionally there are patches of loose skin, as large as the hand, of a purply red colour, the flesh is not injured, and on the chest, neck, back, and arms there are streaks like the strokes of the seal character. There are two kinds of death by lightning: one from fright, where no wounds are to be found, the other from being struck where wounds are to be found.

Tigers.

Where a man has been bitten to death by a tiger, the colour of the body will be yellow, the mouth and eyes will generally be open, the hands clenched, the hair in confusion, and the wounds torn and uneven. There will be marks also of the animal's tongue and teeth.

Tigers generally bite the head and neck, leaving marks of the claws and feet on the body. The wounds are like holes, in which the bone is sometimes visible. They will be on the chest and legs. There will also be marks of the tiger's claws on the ground, which should be copied by an artist, the head man of the place and the neighbours being present as witnesses. [At the beginning of a month tigers bite the head and neck, in the middle of a month the back and belly, at the end of a month the feet. Cats bite rats in the same way.]

Mad Dogs.

Where a man has been bitten by a mad dog and died in consequence, there will be a mark of a wound, and the belly will have swollen up hard. The appearance of the victim in the first stages will be that of a man with a violent cold, terribly afraid of wind, barking every now and then like a dog, wanting to bite people and tear their clothes, the belly pendulous, and micturition difficult.

Snakes, &c.

Where death has been caused by the bite of a snake or reptile, the wound will be black and show marks of the teeth; the parts round it will be livid and swollen,

a watery fluid of a yellow colour will flow from the wound, the poison will have penetrated to the extremities, and the body will have swollen up black and shiny.

Rats, &c.

Where a dead body has been gnawed by rats, &c., the skin will be broken, but there will be no appearance of blood, and round the injured parts there will be marks of teeth. Supposing the parts to be ragged and uneven, if inflicted by some other animal, the wound will be on a larger scale.

Directly a case of death on the high-road is reported, proceed at once to examine the body, and observe if there are any wounds, taking a note of the apparent age of deceased, his facial appearance and clothes. If there is any property found with him, make a public delivery of the same to your treasurer, to be kept until identification by his relatives. Buy a coffin and bury the body in a temporary manner, marking the spot with a mark. If it is a case of death by violence your investigations and report must be made with double care.

CHAPTER III.—ACCIDENTAL POISONING.

Death is not caused only by such drugs as arsenic or snake-eater but even by gin-seng and the *Cyperus esculentus*, if constantly taken; the mouth and nose discharging blood, and the skin breaking all over into cracks about an inch apart. In the medical work by the Emperor Huang and his minister Chi, we have mentioned the tendon-swelling, commonly called "the black-rash swelling." A wrong diagnosis will lead to fatal results. All such diseases as also the vulvae morbus are characterized as follows: The nails of the hands and feet are of a dull livid colour, or perhaps quite livid and purple, and in severe cases the head and whole body are both purple, the reason being that the blood has been paralysed and acquires this colour from congelation. In Canton and Fokien they have also miasmatic contagion. If a man is attacked by this let him be pricked at once with the sharp point of a piece of broken crockery-ware, either on the forehead, or between the eyebrows, or on the arms; about a pint of blood taken from him will be enough to effect a cure. If the attack is slight the blood will be red and copious; if severe, purple and scarce. Blood of a blackish purple and very little in quantity indicates that the attack is extremely severe, and recovery next to impossible. The people of those provinces regard a copious flow of blood as a subject of congratulation, and know that when it comes with difficulty recovery is out of the question. The head and face of the corpse are for the most part of a dull livid or entirely purple and black, as also the nails of the hands and feet. On the other hand, there is abscess of the liver and other diseases of the viscera where a foul mass it thrown up, black blood excreted, the rectum swells up, and the bowels protrude: in such cases death must not be hastily attributed to poison. Besides, death is often caused by mixing things which disagree; for instance, you cannot eat honey with onions, nor should there be any dust or cobwebs with a dish of torpedo. If from the rapidity with which death occurs you are led to infer that it was caused by poison, you will fall into grievous mistakes.

Miasmatic contagion is extremely fatal in Yunnan and Canton. The body is not entirely livid, purple, or of a dull black, but the face is of a bright red colour, the lips and mouth purple and black, the chest, abdomen, and throat swelled up, the nails of the hands and feet of a dull livid colour, and from the mouth, eyes, ears, nose, &c., there will be a discharge of blood. If the bones are examined after death, they will be of a cloudy dark-blue colour.

Reptiles.

Poison can be taken into the system otherwise than through the stomach, as from the bites of insects, snakes, and such animals, but in such cases there will be the mark of the bite. Where death has ensued from the bite of a mad dog after the wound had healed, there will still be the marks of teeth: note the size of these. All such wounds will be livid and black and swollen.

In cases of death from poisoning the bones will be rotten, and will be found on examination to be of a dull black colour, the chest, pit of the stomach, roots of the teeth, and tips of the fingers, being all of a dark blue colour.

Chapter IV.—Suicide by Poison.

Where death has resulted from intentionally taking poison, the mouth and eyes will generally be open, the face of a dull purple or livid colour, the lips purple and black, the nails of the hands and feet of a dull livid, there will be blood from the mouth, eyes, ears, and nose. In severe cases, the whole body will be black and swollen, the face livid and black, the lips curled and blistered, the tongue shrunk up or cracked, rotten, swollen, and slightly protruding. The lips will also be rotten and swollen, sometimes also cracked. The tips of the finger-nails will be black, the throat and belly swollen up, of a black colour and blistered. The body will be marked with livid streaks, the eyes starting out, a purple black blood coming from the ears, mouth, and nose, with the hair on the head and face in disorder.

Before death takes place a foul mass will be thrown up and black blood may be passed. The rectum will be swelled up and the bowels will protrude.

Where poison has been taken death will ensue either at once or at some time in the same day; if the poison is slow, at the expiration of one or two days. The vomiting may be intermittent or continuous. Search carefully deceased's clothes for any remnant of the poison; also the spot where he committed suicide for any pot or vessel that might have contained the drug.

Silver Needle Method to detect Poison.

To detect suicide by poison, take a silver needle, wash it in lye made from the soap bean and insert it into the dead man's mouth, sealing up the mouth tightly with paper. After a little while take it out, and you will find it of a dark blue or black colour, which will not come off if washed again in soap-bean lye. If it is not a case of poison the needle will remain fresh and white.

Another Method.

Another method is to take three pints of common rice and boil it soft; then take one pint of good glutinous rice, clean it well, and steam it in a pudding-cloth over the already boiled rice; then take a fowl's egg—a duck's egg will do—make a tiny hole in it and take out the white, throwing it over the glutinous rice and mixing it well up together. Then tie up as before and lay it on top of the boiled common rice, and with three fingers squeeze it up into balls about as big as a hen's egg. Take these quickly, while still hot, and put into the deceased's mouth in front of his teeth, covering up the mouth, ears, eyes, &c., with three or four sheets of paper. Then take several strips of new cotton wool and plunge them into three or four pints of good vinegar, which has been made to boil fast. When they have been some little time immersed, spread grains all over the body and cover over with the cotton wool. If deceased died from the effects of poison, the body will swell up, there will be a foul and stinking discharge from the mouth, blown out upon the cotton wool. [You had better not stand too near.] Afterwards take away the cotton wool and the glutinous rice, when, if it has been turned black and foul smelling by the discharge, you may return a verdict of death by poison. The absence of these signs must be regarded as proving that death was not caused in this way.

Fowl Method.

Another method is to thrust a lump of boiled rice into deceased's throat and allow it to remain there a little while, the mouth being meanwhile covered up with a sheet of paper. Then take it out and give it to a fowl to eat. If the fowl dies, poison is present.

Full or Empty Stomach.

If poison is taken on a full stomach, the abdomen will swell and be of a livid colour, but the lips and finger-nails will not be livid; if taken on an empty stomach, the results are exactly reversed. Where, however, poison is taken by a man with a weak stomach or by an invalid of long standing, and death occurs directly, neither the abdomen, mouth, lips, or finger nails will necessarily be livid. The disease under which deceased may have been suffering will require to be taken into consideration.

Where Time has Elapsed.

Where some time has elapsed since the poison was swallowed, and it is no longer possible to detect its presence, first take a silver needle and thrust it into deceased's throat; then, beginning from the feet upwards, plaster over the body with hot grains and vinegar, allowing a free passage [through the mouth] for the air inside the body. If a poisonous vapour is emitted the needle will turn to a black colour. If the poultice of grains and vinegar is applied from the head downwards, the poison vapour will be forced the other way and can only be detected by applying the silver needle to the rectum.

Where other food has been taken on the top of poison, and it becomes impossible to detect the presence of the latter, the ordinary operation by the mouth may be performed by the latter method.

Whether Administered Before or After Death.

Where poison was accidentally or intentionally taken by any one, the whole body will be of a livid colour, retaining the skin and flesh for many days; or it may be of a black colour. After some time the skin and flesh will rot and fall off, leaving the bones exposed and of a dull black colour. The chest, pit of the stomach, roots of the teeth, and tips of the fingers will be of a dark blue or livid hue.

Where poison has been administered to a dead man in order to pass off his death as the result thereof, the skin, flesh and bones will be of a yellow colour.

Care must be taken not to confuse the accidental with the intentional taking of poison.

Swallowing Snakes.

[A case is recorded of a man who tied his victim's hands and feet, and forced into his mouth the head of a snake, applying fire at the same time to its tail. The snake jumped down the man's throat and passed into his stomach, but at an inquest held over the body no traces of wounds were found to which death might be attributed. This, however, may be detected by examination of the bones, which, from the head downwards, will be found entirely of a bright red colour, caused by the dispersion of the blood; and, moreover, the more the bones are scraped away the more bright coloured do they become.]

Chapter V.—All Kinds of Poisons.

Ku.

Where *Ku* poison has been taken, the whole body, head, face, and breast, will be of a dark blue or black colour. The body will swell, and there will be a discharge of blood from the mouth and anus.

To Make Ku.

Ku is made as follows: Take a quantity of insects of all kinds and throw them into a vessel of some kind, and let a year pass away before you look at them again. The insects will have killed and eaten each other until there is only one survivor, and this one is *Ku*. The word is derived from *ch'ung*, insects, and *min*, a vessel, &c.

Where death has resulted from gold worm *ku*, the body will be thin and emaciated, and of a yellow and white colour, the eyes will have sunk in, the teeth will be visible, the lips will be shrunk, the abdomen will have sunk in, and a needle will show a shaded yellow stain, which will not come off when washed in soap-bean lye.

Sometimes the body swells and the skin and flesh look as if scalded into blisters by hot water, with here and there traces of matter, and the tongue, lips and nose cracked and broken. These are unmistakable signs of gold worm *ku*. The first paragraph applies to thin people, this latter to fat ones.

Both in Kiangnan and Kiangsi there is a kind of poison called *Illicium religiosum*, very much resembling *ku* in its action, only the lips are more cracked, the gums livid and black; it operates after twenty-four hours, blood flowing from the nine apertures.

Where croton seeds have been taken the mouth will be dry, the two cheeks red, the top of the head, palms of the hands and soles of the feet will be hot; there will be incessant purging.

Arsenic.

When sublimed arsenic has been taken, in a little while the whole body will be covered with small blisters, and turn to a livid or black colour, the eyes will start out, and on the tongue there will be little broken blisters, besides which the mouth and lips will be cracked, the two eyes will swell out, the belly will swell, the rectum will swell and gape, and the finger-nails will be livid and black.

Swallowing sublimed arsenic will be attended with vomiting and painful griping, delirium, and discharges of blood from the seven apertures of the head; the mouth and lips will also be livid and black. If taken on a full stomach, the upper half of the body will be livid; on an empty stomach, the lower half. Testes magnopere crescent.

Where *Corydalis heterocarpa* poison has been taken, blood will flow from every aperture on the body; its characteristics will be like those of sublimed arsenic.

Ice-flakes (Borneo Camphor).

A little more than a mace of Borneo camphor taken in hot wine will be enough to stop respiration, make the blood boil and flow from the seven apertures.

Quicksilver.

Death from swallowing quicksilver may be detected by gold which will be turned white.

Where a man has been poisoned by vegetable or mineral substances, the body will be marked here and there with swellings like blows from the fist, or large patches of a livid or dark colour. The nails will be black, the flesh on the body cracked and slightly bloody; the belly may be swollen, accompanied by hæmorrhage from the rectum.

Wine.

Where a man has been poisoned with wine, the belly will swell, accompanied by hæmorrhage from the rectum.

Where death has resulted from eating poisonous mushrooms, the nails of the hands and feet as well as the whole body will be of a livid or black colour, much blood will come from the mouth and nose, the skin and flesh will be cracked all over, the tongue and bowels will protrude.

Dross of Silver.

No mention is made in any book of the means of detecting the presence of the dross of silver used as a poison.

Salts.

Where death has been caused by taking salts, the hair will be in disorder, the nails off, and on the chest there will be marks of nails in the flesh, for the pain will be so acute that the victim will roll about on the ground and tear himself to pieces.

Where salts have been taken, blisters will not rise on the body, the mouth will not be cracked, the belly will not swell, the nails will not be livid, the needle will not turn black, but will be of a slightly dull colour and will come white with washing, the whole body will be yellow, the two eyes closed, and in the mouth there may be some frothy saliva. Although the body decomposes, the heart and lungs will not. If some of the liquid [in the stomach] be heated over a fire it will become salt.

Soda.

Death is sometimes caused by a dose of washing-soda, given to a sick man in his medicine. In such cases the hair is in disorder, the finger-nails torn off, the body bent up, and blood flows from the nose and mouth.

Where *Ficus japonica* has been taken, the victim gets confused as if he had had a paralytic stroke, or as if delirious.

Bitter Almonds.

Bitter almonds are poison; they are found in all the north-western provinces. Taken either raw or cooked they are not hurtful, but a good many eaten half-cooked will cause death. The eyes of the body will be closed, the tongue, lips, ears, fingers and toes will be livid, and the belly will be swollen with patches of a livid colour on it. [To cure a patient, make him sick.]

Aconite.

Aconite is poison. It is found on the left of the Yangtze. Its juice, when boiled, is called *shê wang*, and is much stronger: if spread on wounds it will cause death instantly.

Chapter VI.—Extraordinary Poisons.

Hsien and Tortoise Flesh.

Spinach eaten with tortoise is poison. [This is a plant with a long, reddish stalk; the tortoise alluded to has three feet and hears with its eyes.]

Monstrosities and Unnatural Food.

Monstrosities in birds, beasts, reptiles and fishes are the result of the parents having experienced some disturbing influence. Such, for instance, are animals with two tails, crabs with one claw, sheep with one horn, and fowls with four legs. Similarly, there are irregular creations, such as white birds with black heads, black fowls with white heads, white horses with dark feet and the reverse. Some animals are not extraordinary in their appearance, but their flesh has unusual characteristics; for instance, if let fall to the ground dust will not adhere to it; hung up all night it will still be warm in the morning; no sun or fire-heat will dry it, and put into water it will move. Sometimes it is the viscera only, and not the skin and flesh which exhibit these peculiarities. For instance, the liver is of a dull dark-blue colour, the kidneys purple and black. Fishes without entrails and gall-bladder, oxen with only one "leaf" of the liver belong to the same class.

Occasionally something which is generally found to agree with the stomach very well, taken with another particular kind of food, becomes extremely hurtful; as for instance, shell-fish eaten with venison is poison, mutton with minced-meat cream is unwholesome, sheep's liver with red pepper causes purging, pork eaten with carum destroys the navel. There are also some kinds of food which do not ordinarily affect each other but when taken into the stomach become living things; as for instance, raw minced carp, eaten with butter produces maggots, tortoise-flesh eaten with spinach produces tortoises [see *ante*], beef eaten with pork produces small white worms about an inch long, as also does pork and mutton eaten with boiled or broiled sticks of the mulberry or *Broussonetia papyrifera*.

[Cases are given of death resulting from drinking pond-water which had been poisoned by snakes, water in which flowers had stood, eating the flesh of a fowl which had swallowed a centipede, drinking tea or water which had been standing uncovered all night, wearing clothes which had been wetted with perspiration and dried in the sun, going into rooms, &c., which had long been shut up, &c., &c.]

Asphyxiation.

Death from asphyxiation is caused by the leaking of the stove-bed. The corpse is soft and without marks; death is painless as if the result of nightmare.

BOOK IV.

CHAPTER I.—METHODS OF RESTORING LIFE.

Hanging.

Where a man has been hanging from morning to night, even though already cold, life may be restored; if from night to morning, the operation will be more difficult. If there is warmth beneath the heart, life may be restored after the body has hung more than a day. On no account cut the rope, but gently taking the body in your arms, have the knot untied and lay the body out at full length. Then let someone place his feet on the shoulders of the patient and firmly hold up the head by its hair, not allowing it to hang down for a moment. Let another manipulate the throat, another rub the chest, and another chafe the feet and hands, pulling them backwards and forwards. If the body is already stiff, let them be gradually bent at the joint. If this is done, in about the space of a meal respiration will begin, and with the recommencement of breathing the eyes will open. When consciousness is restored, give the patient some cinnamon tea and some gruel to drink in order to keep his throat moist, and let two men blow into his ears through small tubes. Recovery under these conditions is certain.

Another method.—Stop up the patient's mouth tightly with your hand, and in a little over four hours respiration will be restored.

Another method.—Take equal parts of soap bean and *Anemone hepatica* finely powdered, and blow a quantity of this, about as much as a bean, into the patient's nostrils.

Another method.—Take two or three tenths of an ounce of genuine goat's blood, rub it down very smooth and mix it with good wine. A dose of this will cause instant recovery.

Another method.—In all cases where men or women have been hanged, a recovery may be effected even if the body has become stiff. You must not cut the body down, but, supporting it, untie the rope and lay it down in some smooth place on its back, with the head propped up straight. Bend the arms and legs gently, wrap up the pudenda, &c., in cotton wool that no air may escape, and let someone sitting behind the head with his feet on the shoulders pull the patient's hair tightly. Pull the arms out straight, let there be a free passage through the wind-pipe, and let two people blow incessantly into either ear through a bamboo tube or a reed, rubbing the chest all the time with the hand. Take the blood from a live fowl's comb and drop it into the throat and nostrils (the left nostril of a woman, the right of a man; also using a cock's comb for a man, a hen's for a woman). Re-animation will be immediately effected. If animation has been suspended for a long time, there must be plenty of blowing and rubbing; do not think that because the body is cold all is necessarily over.

Drowning.

Where a man has been in the water a whole night, recovery may still be effected. Pound some soap bean, wrap it up in cotton, and insert it into the rectum; in a little while the water will be discharged and life restored. Or, having bent the patient's legs, let him be carried by the feet over another man's shoulders, back to back, when he will vomit forth the water and be revived.

Another method.—Break up part of a mud wall and pound it to dust; lay the patient thereon on his back, and cover him up with the same excepting only his mouth and eyes. Thus, the water will be absorbed by the mud and a recovery will be effected. This method is a very sure one even though the body has become stiff.

Another method.—Sprinkle the face all over, excepting only the mouth and nose, with hot sand, replacing it as it gets cold or damp. After several changes of sand, animation will be restored.

Another method.—Pour half a cupful of vinegar into the nostrils.

Wrap up some lime in cotton, and insert it into the anus. Water will be discharged and re-animation will follow.

Hold the patient up by his feet and pour good wine into the nostrils and anus.

Another method.—Hold the patient up by his feet, strip off his clothes, clean out the navel, and let two men blow into his ears through bamboo tubes.

Another method.—Strip off the patient's clothes without loss of time, and cauterize him on the navel.

Another method.—Immediately on taking a man out of the water, prise open his mouth and insert something to keep the teeth apart that the water may come out; blow through tubes into both his ears; take some finely powdered *Finellia tuberifera* and blow it into his nose; and fill a tube with powdered soap bean and blow it into his anus. If the accident happens during summer, take the drowned man and lay him crosswise on his belly over a cow's back, two people supporting his head and feet; then let the cow be walked about quietly, when the water in the stomach will be thrown up and pass away also through the penis and anus. Give the patient green-ginger tea and decoction of rose maloes or the juice of raw ginger. If a cow is not to be had, let someone go down on his hands and knees and lay the drowned man across his back as before; then let the man on his hands and knees wriggle himself about when the water will come out. If it is impossible to get a cow and no one is willing to undertake the job, a large pot or jar will answer the purpose. If it is in winter strip off the wet clothes and change them; heat some salt, wrap it up in a cloth and apply it to the navel; spread out a mattress and bedding and cover it over with plenty of ashes from reeds, grass, &c.; then lay the drowned man on it face downwards, placing a small pillow under the navel, and sprinkle ashes thickly all over the body, covering it with bedding, &c., and taking care that none of the ash gets into the patient's eyes; pour into the mouth, kept open as mentioned above, rose maloes and green-ginger tea, blowing into the ears, nose, and anus as for the *summer* method. In winter, when consciousness is restored, give the patient a little hot wine to drink; in summer, a little gruel. The power of ashes to absorb water in this way may be tested on drowned flies, which, if covered up with ashes, will revive.

Another method.—Take a small wine-jar and throw into it a strip of lighted paper, turn it upside down and place the mouth of the jar in umbilico; when the jar is cold repeat the process until the water comes out, which will be a sign of recovery.

Another method.—If when the body is first recovered there is the slightest sign of breathing or the least warmth on the chest, make some one quickly take off his underclothing, and, having wrapped the drowned man in them, lay the body across his own and roll it gently backwards and forwards so that the water may come out of the stomach. If the water comes out there is a good chance of recovery. Also burn some thick paper under his nose for a little while and then blow some finely-powdered soap bean into his nostrils. If there is the slightest sneeze a recovery can be effected.

Wounds, &c.—In cases of knife-wounds, &c., where the membrane has not been pierced, take about as much as a soap bean of olibanum and myrrh, mash up smooth, mix with half a cup of urine and half a cup of good wine, and heat altogether; when warm, administer a draught. Also take dolomite powder, or cuttle-fish bones, or dragon's bones, and bind over the mouth of the wound; the bleeding will thus be stopped. Two cases of examination by an assistant Prefect Chuan Ting are on record where the wounded men not being quite dead, he made the head man take the heart of an onion, heat it very hot and apply it to the wound; the result was a sudden inhalation of air. A second application of onion causes no pain.

Dolomite powder (so-called) contains olibanum, myrrh, archangelica, lophanthus, aconite, *Magnolia hypoleuca*, iris, *Anemone hepatica*, laka wood, *Aralia edulis*, calomel, sapan-wood, sandal-wood, dragon's bones, musk, and dolomite.

Another Method.—Where a knife-wound keeps on bleeding, take some laka wood, scrape it with a piece of broken crockery-ware, rub it fine with a stone and spread it on the wound. The bleeding will stop and there will be no scar.

Pain Killer.

Where the pain of a knife-wound, &c., continues, take some good "chicken-bone" charcoal, which rings when thrown on the ground, and knead it up into a lump with an equal quantity of clear resin; then take plenty of the expressed juice of old leeks, mix and let it dry of itself (not in the sun) repeating several times, when nothing but a fine

dust will remain. (The mixture should have been previously prepared on the 3rd of the 3rd moon, 5th of the 5th, or 7th of the 7th.) If spread on the wounded part the pain will cease at once, and when well the flesh will appear as usual.

Knife Wounds and Bowels Protruding.

Where a wound has been inflicted with metal and the bowels protrude, take five pints of rye and nine pints of water, boil away to four pints, strain it clear, wait till it is quite cold, make the wounded man lie down on a mat, and let someone squirt the liquid from his mouth over the patient's back; the bowels will gradually recede. The liquid must be squirted unknown to the patient, and there must not be a lot of people talking in the room at the time. If the bowels do not recede, take the mat by the four corners, raise it up and gently shake it; the bowels will then go in. When in, sew up the wound tightly with thread dipped in hempseed oil, binding the body round with silk moistened in the same way. Be careful not to shake the patient about and thus re-open the wound.

Arrow-heads.

Where a wound has been inflicted by the head of an arrow, take some meat which has been a long time salted, peel off the skin—a good red piece should be chosen—and chop up the fat very fine until it is pulpy, mix with it some ground ivory and human finger-nails until the ingredients are well distributed, and apply a thick lump of this to the place where the arrow-head is; in about the time it takes to eat a meal the arrow head will come out.

Scalding.

In cases of injury from scalding, get a large oyster and put it in a basin with its mouth upwards somewhere quite away from people; wait till its shell opens and then shake in from a spoon an equal portion of genuine musk. The oyster will then close its shell and its flesh will be melted into a liquid. Add a little more of the above ingredients, and with a fowl's feather brush it over the parts and round and round the wound, getting nearer and nearer every time, until at last you brush it into the wound: the pain will gradually cease. A small oyster will do if a big one is not to be had. This is a first-rate prescription. When the burning has stopped, take the shell of the oyster just used, burn it to a cinder, but so that it keeps its properties, pound it to dust, and mix with it a little Borneo camphor and musk, and apply it round the wound. If it is a place where there are no oysters, Borneo camphor alone rubbed round and round the wound and finally into it will heal the place gradually.

Cold Water, &c.

In cases of scalding, on no account apply cold water, cold things, mud from the bottom of wells, &c., to the injured parts, for the heat will thus be driven farther in, and the least result will be that the parts will contract, though if the poison of the heat extends as far as the heart, death will speedily ensue.

To Cure Scalds.

Take good Hangchow meal ground very fine, and mix it with good hair-oil and spread it on the parts. If hair-oil is not to be had, pine-oil will do as well.

Another Method.—Spread over the wound a quantity of very old soy. When cured there will be a black scar.

Another Method.—Take some Lui Chi Nu (a plant named after its discoverer) pounded fine, and having first brushed over the wound with a fowl's feather dipped in glutinous rice water, sprinkle it upon the wound. The pain will cease and there will be no scar. Generally speaking, it is advisable in the case of a scald to begin by sprinkling the wound with powdered salt in order to preserve the flesh, and then to apply remedies.

Another Method.—Rhubarb mixed with rice-vinegar will effect a cure in two days.

Sunstroke, Heat Apoplexy, &c.

Where a man has received a *coup de soleil*, quickly dig a hole in the ground and pour in some water; mix the water up with the mud and give it to him to drink; he will then recover.

If cold water is given in such cases, death will be the result. Cover over the patient with warm ashes taken out of a stove, and apply a cloth which has been dipped in hot water to the abdomen, ribs, &c.; after some time consciousness will be restored. On no account give anything cold to eat.

Where a man falls down overpowered by the heat, carry him into a shady and cool spot; give no cold water to drink, but apply a cloth or something dipped in hot water to the lower part of the abdomen, letting a constant supply of hot water drip upon the cloth, so that the heat penetrates to the bowels. If warmth is induced the patient will gradually come round. Should there be no hot water ready to hand, scoop up some of the hot earth from the roadside and pile it up on the abdomen, the more the better, changing it as it gets cold, and afterwards giving cooling medicines. Or, if hot water cannot be had, pile up hot earth as before, and having made a hole in the middle of it, *jube circumstantes in hoc mingere*, and thus effect a cure.

Where a man is quite stupid from the effects of heat, let him chew a good-sized piece of garlic and wash it down with some cold water. If he is unable to chew it, mash it up with a little water and give it him to drink; consciousness will be restored at once. Suppose a case of a thirsty man not being able to procure water to drink; let him chew up a piece of raw garlic about two inches in length, and salivating it well, swallow it. This will be equivalent to a quart of water.

In cases of heat apoplexy, roast a pint of hemp-seed black, spread it out till cold, pound it up, and administer mixed with fresh-drawn water.

Freezing to Death.

In cases of death from cold, the arms and legs are stiff, the jaws close set. If there is the slightest sign of respiration, heat some ashes in a large pot, wrap them up and apply to the heart, changing when cold. When the eyes open, give warm wine and thin gruel in small quantities at a time. If the patient is brought near the fire before warmth has been communicated to the heart, the conflicting sensations of heat and cold will cause death.

Another method.—Take a piece of carpet or mat, wrap the body up and secure it by a string, lay it down on a level spot; and let two people, standing on either side, roll it backwards and forwards from one to the other with their feet. When the arms and legs are warm consciousness will be restored.

Laughter.

Where a man has fallen into the water in winter and has quite lost all consciousness from cold, if there is the slightest warmth about the chest, life may still be restored. Should the patient show the least inclination to laugh, stop up his nose and mouth at once, or he will soon be unable to stop laughing and it will be impossible to save him. On no account bring the patient hastily to the fire, for the sight of the fire will excite him to immoderate laughter and a recovery will be impossible.

Where a frozen person has been restored to consciousness, administer green ginger pounded up with the skin and old orange-peel also pounded up, the two being mixed with three pints of water and boiled down to one.

Nightmare.

In cases of nightmare, do not at once bring a light, or going near call out hastily to the person, but bite his heel or big toe and gently utter his name. Also spit on his face and give him ginger tea to drink; he will then recover.

In cases of nightmare where the patient cannot be waked, change his position a little and quietly utter his name; he will then awake. If there was a light burning at the time keep it alight; otherwise do not kindle one.

Another method.—Blow into the patient's ears through small tubes, pull out

fourteen hairs from his head, make them into a twist and thrust into his nose. Also give salt and water to drink.

Another method.—Express half a cup of the juice of leeks and pour into the nostrils. In winter use the root, which will also cause sneezing.

Another method.—Cauterize the two great toes on the spot where the hair grows.

Another method.—Powdered soap bean, about as much as one bean, may be blown into the nostrils. A sneeze may give a free passage for air, and will even effect a recovery after three or four days.

In cases of nightmare, where the body is not quite cold, administer rose maloes mixed with wine; a recovery will thus be effected.

Goblins, &c.

Where death resulted from seeing goblins, &c., or an invalid has gone off suddenly in his sleep, the categorical denomination is the same. Take the heart of a leek and thrust it up the nostrils (the left of a man, the right of a woman), about six or seven inches; cause the eyes to open and the blood to flow, and life will be restored.

Look along the inner edge of the upper lips for blisters like grains of Indian corn, and prick them with a needle.

Take powdered soap bean or raw *Pinellia tuberifera* and blow about as much as a bean into the nostrils.

Fumigate the nostrils with the smoke of burnt sheep-dung.

Steep some cotton-wool in half a cup of good vinegar and squeeze it into the patient's nose. Grasp his two hands; do not let him be frightened, and in a little he will recover.

Cauterize him on the navel, blow powdered soap bean into his nose, or pour the juice of leeks into his ears.

Express the juice of raw *Acorus terrestris* and give the patient a cupful to drink.

Death from Fright.

Where a man has been frightened into a fit, administer one or two cups of warm wine when he will recover.

Falls and Blows.

In cases of the five deaths (child-birth, fright, strangulation, nightmare and drowning) or death from falls or blows, if the heart is only a little warm a recovery may be effected even though a day has elapsed. Place the dead man on the ground in squatting posture, like a Buddhist priest: let somebody pull his hair down towards the ground (i.e., in order to get the nostrils into a horizontal position), and blow through a bamboo or paper tube a quantity of raw *Pinellia tuberifera* in powder into the nose. If consciousness returns, administer pure juice of raw ginger to counteract the poison of the *Pinellia tuberifera*.

In cases of sudden death from falling from a height, tumbling down, nightmare, &c., if the body is not cold, mix some rose maloes with wine and give it to the patient to drink. If he swallows it, he will live.

Death from Falling and being Crushed.

Where a man has fallen and been severely crushed, blood will flow from the mouth and ears, and unconsciousness will ensue. If there are any signs of vitality or the body is at all soft, a recovery may be effected. But the patient should not be surrounded by a crowd of excited people, as this will prevent him from recovering himself; a relative should speak to him, support him, and place him in a sitting position on the ground. His arms and legs should be then bent up close to his body, and after a little he should be placed in the lap of the person who spoke to him, ano ejus genu obstructo ne flatus effugiat. If there is the slightest return to consciousness, let him be removed to the place where he generally sleeps, and let the doors and windows be closed up tightly to darken the room. Then, having again bent his arms and legs, he should be placed again in the same position and not allowed to lie down. Warm urine of a child should be then administered; horse's urine is the best, but if not procurable a man's

will do, rejecting the beginning and end of the discharge, and taking care that it is clear and flows quickly, and not that of anyone who has eaten onions or garlic. Give one or two cupfuls of it; if it gets down his throat a recovery will be effected. Also, give "four ingredient" broth, three times the original prescription, putting in peach kernels without the skin and with the point taken off, and safflower, each one ounce, *Aralia edulis* and southern *Cratægus cuneata*, pounded up, each two ounces, raw rhubarb two ounces, a bowl of child's urine, and if in summer add about $\frac{1}{10}$ of an ounce of gentian.

Aralia edulis	1 mace.
Hsiung from Ssŭch'uan (blood purifier)... ...	7 cenid.
Rehmannia glutinosa	3 mace.
Cooked paeonia albiflora	3 mace.

Take plenty of quick running water, and warm up the above prescription over a quick fire, pour it into a bowl and hold under the wounded man's nose, so that the steam may penetrate into the bowels, and the sickness be avoided which would follow if taken at once in the mouth. Give a small cupful as a dose, administering another at a short interval if not retained. Keep on giving doses until all is gone, not permitting the patient to lie down. When the medicine has been taken, the anus must be kept still more tightly closed so as to prevent the passage of wind. If the medicine has already begun to operate and the anus be not tightly closed, a recovery will be impossible, for the air (necessary to life) will all escape from behind. Wait until you can hear that the medicine has begun to operate and there have been several internal heavings, then delay no longer, but support the patient that the discharge may take place. This discharge will be entirely of a purple colour. When half the poison has been got rid of, the patient may sleep. If the discharge is all fæcal matter, stop giving the medicine; if not, one or two more doses will do no harm. Subsequently, the patient will be gradually restored to strength; medicines for this purpose must not be incautiously administered.

Snake and Reptile Bites.

Poisonous snakes can destroy life, but the danger may be averted by cutting out the wound with a sharp knife.

In cases of snake or reptile bites where no medicine is procurable at the moment, take a cupful of the juice of the indigo-plant, $\frac{1}{2}$ of an ounce of powdered orpiment, mix and spread on the wound, administering the liquid part as a draught. If no indigo-plant is to be had, use prepared indigo or indigo dye.

If a man is bitten by the *hui fu* or large-headed snake and the poison penetrates, he will die. Quickly take some cord and bind tightly round the wounded part, so as to prevent the poison from reaching the heart and bowels. Let someone take a mouthful of rice and vinegar or wine, apply his mouth to the wound and suck out the poison, spitting it out and changing the wine or vinegar. Let him continue until the wound is a light-coloured red and the swelling has gone down, taking care not to swallow the poison. Another method is to drink some hemp-seed oil to counteract the effect of the poison on the heart and to spread powdered ginger on the wound.

Where death has resulted from the bite of a snake, take some scented iris and mix it with a decoction of *Ophiopogon japonicus* and administer a dose. If in a hurry use water instead; life will be thus restored.

Another method.—Take 1 oz. of magpie's dung, half an ounce of orpiment, mix with wine and administer as a draught, $\frac{1}{2}$ of an ounce at a time. Smear the liquid over the wound.

Mad Dog Bites.

Before the poison has had time to disseminate, take seven cantharides (*Mylabus cichorii*), pull off their heads, feet, and wings, and boil them with two hen's eggs; take out the insects and eat the eggs without salt, &c.; clots of blood will then appear in the urine. If the penis swells up and is painful, it is because the clots of blood have not entirely passed away. Another dose will clear them out and the pain will cease.

Another method.—Fry some of these insects with rice, wait till the rice is shrivelled and yellow, take out the insects, pound the rice to powder, boil an egg, and eat as in

the former prescription. When the blood clots have all passed away, recovery will ensue.

Another method.—On being bitten, go at once to a pond or river and wash the wound thoroughly, squeezing out the blood and drinking plenty of the juice of raw ginger. The poison may be thus counteracted. Close up the wound tightly with bandages so that the air may not get to it.

The Seven Scruple Powder.

Good red oxide	1 mace	2 cand. (weight)
Genuine musk		1 cand. 2 li.
Borneo camphor		1 ,, 2 ,,
Olibanum	1 mace	5 ,,
Safflower	1 ,,	5 ,,
Myrrh	1 ,,	5 ,,
Dragon's blood	1 ounce	
Catechu	2 mace	4 cand.

Procure the above drugs, choosing such as are brought from places most noted for producing them, and at noon on the 5th day of the 5th moon, mix them all up into a fine powder, stow them away in an earthenware jar, and seal the top with wax; the longer kept the better. No more than seven scruples should be taken as a dose, and must not be taken during pregnancy.

The above prescription is especially adapted for curing wounds inflicted by metal weapons: wounds from falls, blows, &c., where the bones or muscles are broken. Where the flow of blood cannot be stopped, take the seven scruple powder, mix with wine and administer as a draught; then take a similar preparation and spread it on the wound. If it is a severe wound from some sharp weapon, or the gullet severed, do not bind it up with a fowl's skin, but sprinkle the above powder all over it; the pain and the blood will stop, and favourable symptoms will appear. The same prescription may also be used for a number of poisons which have no specific name, mixed and administered as above. Wounds from blows yield infallibly to this treatment.

In cases where coroners, in examining wounds, happen to have no medicines with them, or in out of the way places where there is no good doctor, and it is to be feared that slight wounds may become severe ones, that severe ones may result in death, this prescription is invaluable on account of its infallibility. If reverently preserved and produced as occasions require, the aggravation of slight wounds may be avoided, and the fatal effects of severe ones turned aside; not one life, but two may be saved (including that of accused)—a benevolent act indeed. Moreover, it is cheap and easy for anybody to make, therefore the prescription is given that benevolent people may avail themselves of it,

Another prescription much used in Szechuan:—

Old cash	½ oz. boiled in vinegar
Peroxide of iron	as the preceding
Dragon's blood (from the tail) ...	
Olibanum	
Myrrh	
Catechu	

The above ingredients, which should be all finely powdered, may be used for healing wounds from blows or falling. The prescription has often been tested with the most astonishing results. The powder should be mixed with wine and enough taken to cause intoxication. When the wounds are healed it will be necessary *a coitu centum dies abstinere*.

Chapter II.—Antidotes against Various Poisons.

Where arsenic has been taken but a little time, beat up from ten to twenty eggs in a bowl, throw in three mace of powdered alum and administer as a draught. After the vomiting give another dose, and when all has been thrown up a recovery will be effected. Where the arsenic has been some time swallowed and has already passed into the bowels so that it cannot be thrown up, take a lump of lead 4 oz. in weight and rub it on

a stone with well water, administering the black liquid which results as fast as it is made; when it is all rubbed away favourable symptoms will appear. After the vomiting mentioned in the first method administer a dose of this lead and water so as entirely to counteract the poison and prevent any further pain.

Another method.—Where arsenic has been swallowed quickly administer a dose of hot duck's blood; the poison will thus be neutralized. Or give a dose of clear manure, which will have the same effect.

Another method.—Boil some bean-curd into a thick liquid and drink it, or—

Mix the juice of liquorice with an infusion of indigo and drink it, or—

Drink the whey of boiled bean-curd.

Croton Oil.

Where croton oil has been taken and the purging cannot be stopped, boil a pint of large beans and drink the juice.

Rhubarb, gentian, the shoots of phragmites, fungi, and *Veratrum nigrum* will stop purging caused by croton seeds.

The juice of plaintain leaves pounded up will also effect the same result.

The juice of black beans is an antidote to *Illicium religiosum*. Or, take an old calyx of a water-lily with the stem attached and let it dry, bite it off and boil in two or three cupfuls of water; administer this as a draught. If not procurable, use the heart of the nelumbium or a joint of the arrowroot plant, boil in water and administer the liquid nearly cold. The poison will thus be dispersed.

For *Ficus japonica* drink some juice of blue cerulean or liquorice-root. Medicines are dangerous in such cases.

Bitter Almonds.

Drink a decoction of the bark of the apricot tree, and life may be restored even though at the last gasp.

For cantharides, take some pig's lard and bean-juice, crystallized salt and infusion of indigo, and administer as a draught. Or take some salt water, boil in it pig's lard and croton oil, or the juice of black beans, and give as a dose. Or administer a draught of soapy water and tickle the throat afterwards with a goose quill. Vomiting will ensure recovery.

Another method is to give half a dozen raw fowl's or duck's eggs; if sickness can be caused a recovery will be effected. In case the teeth are tightly set they may be forced open with a chop-stick.

Mushrooms.

Fungi are very common at Ningpo and in the neighbourhood, differing from one another in species. Some are fatally poisonous, being impregnated with the venom of snakes and reptiles. A Buddhist priest recommends the following method: Make a hole in the ground, fill it with water, and stir it up till quite thick with mud; wait a little while, and then administer a draught of this, which will effect a recovery. This prescription is mentioned in the *Chinese Herbal*. The mushrooms which grow on the sycamore tree, if eaten, cause uncontrollable laughter, and are commonly called the "laughing mushrooms." The above remedy is important to people who live among the hills.

Where *Corydalis heterocarpa* has been taken, a dose of the juice of excrement will effect a cure; or supposing the poisonous water has been swallowed in which some may have been steeped, and that there is a discharge of blood from every possible outlet, then quickly take an egg which has been sat upon for some time, mash it up fine, and administer it mixed up with hempseed oil. If the poison is vomited a recovery may be effected.

When enough fungus has been taken to cause vomiting, chewing fresh leaves of the honeysuckle will be found to be an efficient remedy.

Aconite.

A remedy for aconite is rice-sugar and black beans mixed with cold water.

For decoction of aconite, the juice of sweet-grass, or the leaves of the small bean, duck-weed, and *Platycodon grandiflorum* mixed with cold water.

Calomel.

For calomel, take 5 lb. of pewter and make a pot. Put into it 15 lb. of spirit, ½ lb. of smilax and 3 mace of resin; seal up the pot hermetically and boil it for twenty-four hours in water; bury it in the ground that the fire-poison may be absorbed, and drink every morning and evening several cups. The urine should be received in an earthen vessel, and will show a sediment. The medicine should be continued until the muscles and bones are no longer sore. A draught of fresh water will counteract the effects of Borneo camphor.

Where salts have been taken administer a draught of the water in which a duster has been boiled (i.e., the dirty rag used by Chinese to clean their tables, &c.). If sickness can be produced the patient may recover.

Swallowing Gold.

In cases of poisoning from swallowing gold, the flesh of the partridge should be eaten; for silver, gentian and liquorice-root. Salt used to wash gold, and the fat of camels, donkeys and horses, as also *Spondias amara*, will all be found to soften gold; sheep's fat will act similarly upon silver. If gold or silver has been swallowed, administer the above remedies according to circumstances; the metal will thus be softened and be easily passed.

General.

As a general remedy for all poisons, the *Ku* poison (see ante) and all kinds of mineral poisons, rub down stone crabs with hot water and administer as a draught.

Quicksilver in the Ear.

Where quicksilver has got into the ear, lay a piece of gold under the ear as a pillow; the quicksilver will then come out of itself. If quicksilver has got into the flesh and has contracted the muscles, iron the parts over with a piece of gold; the quicksilver will then come out attracted by (lit., to eat) the gold and a recovery will be effected.

Asphyxiation.

In cases of asphyxiation a draught of cold water will bring about a recovery, or the juice of turnips poured into the nose and mouth. Move the patient into some place where the wind can blow upon him; he may thus come round.

Food.

Where poison of some unknown kind has been taken with the food or drink, administer some sweet-grass or *Platycodon grandiflorum* broth; a cure may thus be effected.

CHAPTER III.—TO CURE CASES OF POISON BY KU AND CHIN TS'AN.

A Buddhist priest in Chüan-chow could cure cases of *Ku* and *Chin ts'an*. Let the patient begin by tasting alum; if it does not seem astringent, but on the other hand rather sweet, and if black bean taken afterwards has lost its ordinary flavour, then poison has been taken. Administer as a draught a decoction of pomegranate skin and root. If the insect is excreted or thrown up, recovery is certain. Li Hui-chih says: Where poison has been taken, pound up alum and young tea-sprouts, and drink with cold water.

Prescription :—

Potato	½ an ounce.
Orpiment	
Cinnabar	1 ounce (pounded fine)
Yellow oxide of lead (Massicot) ...	
Musk	
Cantharides	2½ mace, with the heads and feet taken off.
Glutinous rice	half raw, half cooked
Red centipedes	one live, one roasted
Croton seeds from Szech'uan ...	

Put the above ingredients into a mortar and mix them up together, either on the 5th of the 5th moon, the 9th of the 9th moon, or the 8th of the 12th moon, in some place quite away from women, fowls and dogs. Make up into pills with glutinous rice-water, about the size of lung-an stones, and let them dry in a dark place in an earthenware pot. Swallow a little tea with everyone you take, being careful not to chew them. In a little while the poison will be excreted, the pills passing away with the clotted blood. If the pills are then washed and preserved their power will be increased threefold.

For all kinds of *ku* poison take dried eel, powder it and swallow on an empty stomach ; or take it when cooked and strong-smelling. Those marked with coloured stripes are the best.

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Section of Laryngology.

President—Mr. H. J. Banks-Davis, M.B., F.R.C.P.

Notes on Fourteen Cases of Intrinsic Cancer of the Larynx, with Lantern Demonstration of the Pathological Anatomy.[1]

By J. S. Fraser, M.B., F.R.C.S.Ed., and Donald Watson, M.B., F.R.C.S.Eng.

(ABSTRACT.)

The fourteen cases reported on were seen between the years 1916 and 1922. Six of the patients were private and eight hospital cases. (The rule in Edinburgh in regard to other diseases, is that at least 80 per cent. of patients are hospital cases.)

Sex.—Of the fourteen cases, only two were females.

Age.—The age varied from 36 to 76 years. The oldest patient made a good recovery and is alive and well four years after operation. The youngest patient, a man of 36, died of recurrence of the growth. There is some reason for supposing that cancer appearing in a comparatively young person is likely to be of a distinctly rapid and malignant type.

Symptoms.—The main symptom was hoarseness, the duration of which varied from one month to two years. Slight pain was complained of by several of the patients and dyspnœa in one case, in which both cords were affected.

General Condition.—Six of the male patients showed marked pyorrhœa alveolaris; at least two had marked arteriosclerosis, the blood-pressure in one being as high as 260; one patient had tertiary syphilis (Wassermann reaction positive); one had bronchitis and emphysema and one had a mediastinal growth. Although all the patients had been investigated by a physician before operation, the presence of the last two conditions had not been noted and the patients had been certified as fit for operation.

Local Appearances.—Two of the thirteen patients upon whom thyrotomy was performed showed a papillomatous variety of growth, the cancer forming a marked fringe which projected into the lumen of the glottis. In one case the affected cord showed a condition of hyperkeratosis, with the formation of white desquamating masses. In eight the appearances were those of a greyish-pink, sessile, warty growth which, in three instances, reached the anterior commissure, and in one involved the extreme anterior end of the opposite cord. In the remaining two cases the growth had invaded deeply, with marked ulceration: (thyrotomy was performed in the first place and laryngectomy later, after recurrence of the tumour: both patients died shortly

[1] These cases formed the subject of a communication read before the American Laryngological Society at Atlantic City, May, 1923.

after the latter procedure. Undoubtedly in these two cases the larynx should have been excised in the first place). In the remaining case, the cancer had already spread in a ring round the glottis when the patient was first seen. In this patient, therefore, laryngectomy was performed at once and the patient lived for seven years after the operation and only died recently from carcinoma of the cervix uteri.

With regard to the mobility of the affected cord, of the thirteen cases treated by thyrotomy we found that in eight the cord moved more or less freely. In three the movement was very limited: (one of these three recovered after thyrotomy: the other two patients died of recurrence of the growth). In the two remaining cases the cord was fixed: both died of recurrence in spite of laryngectomy.

The *diagnosis* seldom presented any difficulty on indirect laryngoscopy, but in every case, after the patient's consent to operation had been obtained, a piece was removed by the direct method and submitted to the pathologist. In all but one the report was "squamous epithelioma." As already mentioned, in this one case the condition appeared to be a hyperkeratosis. After consultation with Dr. Logan Turner, it was decided to proceed with laryngo-fissure, and subsequent microscopic examination revealed some invasion of the deeper tissues.

The importance of a thorough medical examination before operation must be emphasized. Two deaths from pneumonia a few days after thyrotomy would almost certainly have been avoided had the medical examination been searching enough. In two other fatal cases, no blame could be attached to the physician or to the operator (J. S. F.) in regard to medical examination. It was known that one patient had marked arteriosclerosis. Death in this case occurred from apoplexy one month after the patient had returned home. In the other case the presence of tertiary syphilis had been ascertained by the Wassermann reaction; the venereal disease expert, however, held that efficient treatment would take too long and advised immediate operation. In the remaining three fatal cases death was due to recurrence of the growth; in these cases the wrong operation was performed; the larynx should have been excised in the first place.

With regard to the preparation for operation and the surgical procedure itself, the following questions arise: (1) As to whether it is right to remove a piece of the growth for rapid microscopic examination, twenty-four or forty-eight hours before laryngo-fissure. Some American surgeons are very averse to this procedure. (2) Spraying of the larynx shortly before operation with cocaine and adrenalin. (3) The method of anæsthesia. (4) The necessity for excision of a piece of the false cord if the growth appeared to be very limited. (5) Removal of part of the thyroid cartilage. (6) Packing the wound above the tracheotomy tube at the end of the operation. (7) The danger of gauze being inhaled into the lower part of the trachea, and (8) the time of removal of the packing: (it has recently been stated that in Vienna the wound above the tracheotomy tube is left open and packing continued for a considerable period, in order to avoid inhalation pneumonia).

Progress.—In six cases there was slight dysphagia after the operation, but only in one case was there any return of fluid through the tracheotomy opening. There was no trouble with post-operative hæmorrhage in any of the cases, but in five a granuloma formed at the site of removal of the vocal cord.

Results.—Of the fourteen cases seven recovered and seven died. Of the

recoveries, one patient was well seven years after operation, one, six years; two, four years; two, two years, and one one year after operation. Of the seven fatal cases, one died of apoplexy, three of pulmonary complication following operation and three of recurrence. This recovery rate of 50 per cent. compares somewhat badly with the 80 per cent. of Sir StClair Thomson and the 78 per cent. of Dr. Chevalier Jackson. It has already been pointed out, however, that efficient medical examination and the elimination of cases in which the vocal cord is fixed would have greatly lowered the mortality.

Conclusion.

Thyrotomy is a suitable operation for the cure of intrinsic cancer of the larynx in a well-defined group of cases, as has been pointed out by Sir StClair Thomson. In view of the brilliant results obtained from excision of the larynx, by Tapia, of Madrid, and Mackenty, of New York, it is advisable to remove the larynx in all cases in which the affected vocal cord is not freely movable.

DISCUSSION.

Mr. H. Tilley (referring to Mr. Fraser's remarks as to the opinion expressed in America as regards the wisdom of removing a piece of growth for microscopical examination) said he (the speaker) agreed that the less one did to the growth before the actual operation, the better. In fact, he thought it would be better to make a mistake in the diagnosis and find the case was not epithelioma, than to interfere with the latter before the operation. This opinion turned on what was considered to be the true pathology of cancer. He thought these growths should be regarded as an unnatural form of growth which, during its period of activity, produced a certain amount of immunity. When the growth was removed, that immunity lasted a certain time, and when it was exhausted there was a recurrence somewhere, if the patient lived long enough. From that point of view the saying: "Once cancer, always cancer," had something in it.

Last May he saw a gentleman, aged 78, whose left vocal cord he (Mr. Tilley) had removed seventeen years previously for malignant disease. During that time he had been using his voice in addressing public meetings and carrying on his profession as a solicitor, and his health had been very good. When the recurrence took place, it was in the scar which had been left from the operation seventeen years before. He thought the conclusion was irresistible that at the operation some small element of cancerous growth had been left behind.

In another case, fourteen years previously, he had removed an epithelioma from the right vocal cord, and at the end of that period a little nodule recurred in the scar of the incision in the neck. After operation there was again freedom for two years, and then infiltration occurred widely in the scar and its immediate neighbourhood, and the patient died. At the operation he felt that he must have left some small cancer cells in the neck wound, but that they were inert while the immunity lasted. Therefore he felt that the less interference before the operation the better, in order not to disseminate any particles of the growth.

Professor Burger (Amsterdam) said that last week he saw a man, now over 80 years of age, who, twenty-three years ago, had carcinoma. Last week he came back with recurrence on both sides. In that case Mr. Tilley's idea of immunity did not help, as twenty-three years was too long for such to be true.

Sir StClair Thomson said he had a case for which he did laryngo-fissure, and the growth removed proved to be epithelioma. But within six or eight months the patient's larynx became infiltrated again, and, after consultation with Sir Henry Butlin, he removed the entire larynx. He had sections cut and, to his horror, there were no evidences of carcinoma! Yet that man lived seven years, and then died of recurrence of cancer in the glands of the neck. He did not see how infection from the original

neoplasm had been brought about. The man was a syphilitic, which had much to do with the condition. In Mr. Fraser's series, those who had syphilis and those who had tubercle did badly.

The operation was very successful when the disease was limited to the vocal cord or to its edge. It was interesting to see in how many the disease went right down to the thyroid cartilage, although one could not imagine this beforehand. That was the reason he had, for ten years, been removing the thyroid cartilage. The sections also showed that the most dangerous cases of intrinsic cancer were those in which the growth was subglottic, as they tended to spread through between the thyroid and the cricoid cartilages. The patients in subglottic cases did not come for examination until the disease was well advanced. He (Sir StClair Thomson) approved of removing a piece for examination beforehand if a satisfactory piece was removable, as in a few cases with papillomatous projections. But he agreed with Mr. Tilley that wherever possible a positive diagnosis should be made before doing the operation. In the majority of cases, however, one had to depend upon clinical knowledge, and upon being able to exclude every other possible cause of chronic infiltration, i.e., chiefly syphilis and tubercle. But in subglottic cases—where preliminary examination was wanted most of all—it was impossible to remove a piece for this purpose. He had seen a number of cases in which removal of a piece had been unsatisfactory.

Sir JAMES DUNDAS-GRANT endorsed what Sir StClair Thomson said about the advisability of removing a piece for examination, when removable; he said he had seen cases in which a diagnosis could not be made without this being done. Tuberculosis often simulated carcinoma, and, more often, carcinoma simulated tuberculosis. The removal made an early diagnosis possible, and he did not think it rendered the patient any the worse. He did his best to sear the cut surface with the galvano-cautery, to close the lymphatics and check the escape of cancer cells.

Dr. JOBSON HORNE said that in order to be able to base a positive diagnosis of malignant disease of the larynx upon the microscopist's report on a piece of the growth removed for the purpose, the piece must be removed in a particular manner and the section must also be cut in a particular manner. He was opposed to the methods commonly followed. It were better that at the operation there be present an expert histologist and section-cutter, who could cut a section and examine and report there and then. That was done in connexion with the removal of growths in other parts of the body. The surgeon could then proceed to operate accordingly. The more he saw of these cases, the more the removal of a piece from a larynx, before an operation had been planned, seemed to him meddlesome interference. The healing of the raw surface after the removal of a piece for diagnosis before an operation—as practised by Sir James Dundas-Grant—Dr. Jobson Horne was opposed to, for if there were a tendency to the development of epithelioma, such a procedure would put the seal upon it.

Dr. SYME agreed with Mr. Fraser as to the desirability of removing a piece of growth for examination. If he could, he would do it in every case before thyrotomy. In many cases in which it was impossible to get a piece away by the indirect method, it could be done with the aid of the suspension apparatus. Several times he had opened the larynx, to find that the condition was not epithelioma, and he did not think the opening of the larynx was so free from danger as some of his colleagues considered it to be. He could not see any difference between taking a piece of growth two or three days before operating, and doing so just before dealing with the condition operatively. A definite opinion on such an important matter was very desirable.

Mr. J. S. FRASER (in reply) said he found in New York that Mackenty was not doing thyrotomy at all, but was removing the whole larynx in every case of malignant disease, whether intrinsic or extrinsic. There might be some reason for refraining from removing a piece for microscopic examination if one was only going to perform thyrotomy, but if it was proposed to take away the whole larynx, it was necessary to be very certain beforehand. With regard to recurrence, one of his patients had a "recurrence" (?) of cancer seven years after the larynx was extirpated, but in this case the "recurrence" was in the cervix uteri. Mr. Fraser did not regard this second appearance of cancer as having anything to do with the larynx. It only showed that the patient was predisposed to cancer.

Case of Extensive Intrinsic Carcinoma of the Larynx in a Young Female Patient, treated by Complete Laryngectomy.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

THE patient, aged 29, had for several years suffered from increasing hoarseness and more recently from extreme pain when speaking and to some extent also on swallowing. Ultimately difficulty in breathing and laryngeal stridor developed. The case had been looked on as one of tuberculosis, but when referred to me at the Throat Department of Brompton Hospital showed such a large rounded, though irregular, swelling almost completely concealing the interior of the larynx, that I thought it more likely to be neoplastic than tuberculous. I removed a portion for microscopic examination and received the report that it presented the characters of typical carcinoma.

With the assistance of Mr. Ormerod, the anæsthetic being administered by Dr. J. D. Mortimer, I performed laryngo-fissure, but finding at once that the growth was too extensive for removal by that means, I carried out complete extirpation of the larynx, having beforehand fortunately obtained permission to do so if necessary.

There was extremely little hæmorrhage, and apart from my having severed the trachea a little lower than I might have wished, thereby throwing a considerable strain on the sutures which held the trachea up out of the thorax, the operation was quite straightforward. I may say that I detached the larynx from the œsophagus and pharynx from below upwards.

I introduced a gum-elastic feeding tube through the nose and owing to the presence of a fistula at the upper point of attachment of the trachea, this had to be retained for between five and six weeks, the fistula having taken five weeks to close. To prevent the dripping of liquids from the wound area and pharynx into the trachea the tracheotomy tube was "jacketed" with ribbon gauze, but afterwards a folded strip, moistened at first with iodoform emulsion but later with red lotion, was introduced above the tube and renewed several times a day. I am indebted to my colleague, Mr. Tudor Edwards, for taking charge of the case during my absence on holidays.

The patient can now swallow "anything," is putting on flesh and feeling quite comfortable. She can make herself understood by means of intensified articulation and there is every prospect of her acquiring a reasonably audible pharyngeal voice.

She has always been of a very talkative disposition, and in her occupation in a draper's shop has had to use her voice a great deal in the midst of much noise in an atmosphere loaded with dust and fluff from the materials she handled.

P.S.—The patient can now make herself so well understood, without any artificial apparatus, that she can go about and do her shopping alone.

DISCUSSION.

Mr. L. COLLEDGE said that owing to Sir StClair Thomson's kindness he had had some experience of these operations. The condition was unusual in the female sex, but two women figured in Mr. Fraser's series. Sir StClair and he, however, operated together upon three women with intrinsic cancer of the larynx on three succeeding days. The agenda notes did not state whether preliminary tracheotomy was done; if that were done, it was easier to remove the larynx from above downwards than it was to turn it up. In two of the last four cases operated upon from above he had found that tracheotomy gave no trouble at all. There were several reasons for doing that operation: the trachea could be amputated at the exact level desired; it could be cut away to meet the skin flaps accurately. If Sir James had done that, he might have been spared some anxiety in the after-treatment, as it was stated in the notes that there was a good deal of tension on the stitches. If the trachea had torn away from the flaps, the case would most probably have been lost. A further reason for what he had said was, that the operation from below was, to some extent, a blind operation, as one could not see at what level one was dividing the pharynx, nor see the exact position of the laryngeal opening, nor the distribution of the growth. A reference was made in the notes to feeding the patient, the exhibitor stating that he retained the gum-elastic feeding tube for five or six weeks. He (Mr. Colledge) had not found it necessary to retain the feeding-tube more than twenty-four hours, and it was passed through the mouth each time the patient was fed. Usually there had been some leakage, but it did not affect the wound in any way. In one case in which Keen's operation was done, healing was by first intention, and there was no leakage. He did not think the patient need be put to the inconvenience of having the feeding-tube retained in the nose. Sir James seemed to have had some difficulty with the tracheotomy tube. He (the speaker) showed the tube he used himself, a Lombard's tube, with extension devised by Professor Moure. This kept the inner tube well away, so that it could be changed by the nurse without trouble. He also showed a tube, designed by Mr. Mayer, which could be worn by the patient indefinitely. It had a pilot, which enabled it to go in and out easily, an important point when neither surgeon nor nurse were available. Sir James had asked him about an artificial larynx. He (Mr. Colledge) showed a model which Dr. Tapia, of Madrid, had invented, and it was simple and light. Inside was a piston, which was held down by a light rubber band. These tubes were now made by Mayer and Phelps.

Sir WILLIAM MILLIGAN said that the operation had been especially successful in regard to the voice; the patient could produce a very fair pharyngeal voice, and with the instrument a fair laryngeal voice. He thought there must be much uncertainty as to the frequency with which malignant disease affected the larynx in females. The patients in the last two cases upon which he had operated were females, and a fair proportion of all his cases had belonged to that sex. With regard to the question of recurrence, he was not certain what Mr. Tilley's theory was, nor was he certain that what was termed a recurrence after seventeen years could be considered a recurrence. He thought that in such cases there was another new growth. In a case in which he did total laryngectomy, the patient lived for ten years afterwards quite comfortably. He came again at the end of that time for what was diagnosed as hydronephrosis, but the condition was, post mortem, found to be epithelioma of the bladder. In these operations the danger largely centred round the anæsthetic. What anæsthetic was used in this case, and had Sir James ever done total extirpation of the larynx under local anæsthesia? He (Sir William Milligan) was sure a proportion of these cases died of pulmonary complications, and it was an important question as to whether this could be avoided by using a different method of inducing anæsthesia. At the Manchester Royal Infirmary, in several such cases, rectal anæsthesia had been tried. In the first the patient did remarkably well from every point of view; but the second and third were not so successful. He had not himself tried to extirpate the larynx under local anæsthesia, but he knew this had been done. One of the patients in his cases died, within twenty-four hours, of acute œdema of the lungs, and two others from pneumonia. In these cases chloroform had been used.

Dr. JOBSON HORNE thought the term "recurrence" had been used loosely that evening. He did not think the appearance of malignant disease in the cervix uteri after removal of epithelioma of larynx could be called a recurrence of cancer of the larynx. And the terms "intrinsic" and "extrinsic" were inexactly used. Extrinsic cancer of the larynx was not uncommon in women, but intrinsic cancer was very rare among them. The present case was not one of intrinsic cancer; the specimen submitted was extrinsic in origin, and had subsequently broken down within the larynx.

Dr. IRWIN MOORE asked for a more exact history of this case: for instance the number of years during which the hoarseness had been present; the question of tuberculosis of the lungs. Why was the case mistaken for tuberculosis in the first instance?

Sir STCLAIR THOMSON asked members to refer to the *Proceedings* (May 3) 1912, v (Section of Laryngology), pp. 151-3, and compare a case there described and discussed with the case now shown. It was that of a man, aged 28, who was admitted to a sanatorium with the diagnosis of tubercle. When shown at the Section, Members could not make a diagnosis, and Sir Felix Semon said a diagnosis was impossible; that there was perichondritis, and that it might have been tuberculous, traumatic, or syphilitic. It proved to be subglottic malignant disease. He (Sir StClair Thomson) made an exploratory laryngo-fissure, but the disease had passed between the cricoid and the thyroid, and nothing could be done.

Sir JAMES DUNDAS-GRANT (in reply) said he made it clear in the printed agenda notice that he thought he might have avoided this prolonged fistula if he had not cut off the trachea so short. This was to get below the tracheotomy cut. On a former occasion he had bevelled the trachea, but he thought that took away some of the spring of the tracheal rings, and therefore he cut it straight across and bent it, rather than cut it obliquely. The gum-elastic feeding-tube did not cause any great discomfort to the patient; it was taken out from time to time, and re-inserted, care being taken that it did not come down through the fistula. The purpose of the jacketing of the tube was to make it press on the little fistula. The substitution afterwards of a strip of gauze answered very well, and the patient showed no signs of pneumonia. She had complained of hoarseness for several years, but the exhibitor saw her for the first time shortly before the operation, when for purposes of diagnosis he thought it right to remove a piece for microscopical examination. If this had been done at an earlier stage and the diagnosis established, the disease might, as he had hoped, have been removable by simple thyrotomy. Fortunately he had obtained permission to do the major operation if found necessary, as it was. The anæsthetic used was chloroform, and it was administered through the mouth, then through the tracheotomy tube, by Junker's apparatus. He had never employed it by the rectal method. A fair proportion of his cases of intrinsic epithelioma had been in females, and "post-cricoid carcinoma" was essentially a woman's disease. He did not agree with Dr. Jobson Horne that the growth in this case was originally extrinsic; it clearly started in the right vocal cord. The members must have observed the talkativeness of the patient. She had Tapia's artificial larynx but could make herself understood without it. In a case which Sir Charters Symonds and he had had, the patient's trouble seemed to have been due to having to lecture to members of the Air Force in the noise and bustle of a depot. He regretted that the disease had not been previously diagnosed in time for it to be removed by thyrotomy. It was a painful moment for him when he had to decide to carry out the major operation on this young woman.

Case of Double Nose.

By F. F. MUECKE, C.B.E., F.R.C.S., and H. S. SOUTTAR, C.B.E., F.R.C.S.

(I) Mr. SOUTTAR (introduced by Mr. MUECKE).

J. H., GIRL, aged 3, was sent to me from Manchester in July with a condition which so far as I am aware is quite unique. She had two completely formed noses side by side, the right one being more central and larger than the left (fig. 1). Each nose had its own septum, two nostrils, and well-formed alæ nasi. At the root of each nose and on its mesial aspect was a minute, round depression, the morphological meaning of which it is



FIG. 1.

difficult to determine. They may have been abortive eyes, but I am more inclined to think that they represent the minimal form of fissura lacrimalis described by Mr. Bishop Harman.[1] I do not think that the whole of the deformity can be explained on developmental grounds and in my opinion the smaller nose is that of an included twin. The child presented no other abnormality, the lips, jaws and palate being perfectly formed and perfectly symmetrical.

The surgical problem presented was that of forming one mesial nose and as it was evident that the internal structure was of quite as much importance

[1] *Trans. Ophthal. Soc.*, 1903, xxiii, p. 256.

as its external appearance I invited Mr. Muecke to co-operate with me in its construction. The method we adopted was to excise the mesial halves of both noses, including the nostrils and their mucous lining, and to bring the two septa together. On a full exposure we discovered that whilst the smaller nose was provided with a well developed bony septum, that of the larger nose was cartilaginous. We therefore divided the base of the bony septum, brought it over to the middle line and fixed the two septa firmly together. The nose itself and the columella were formed with ease. The child now has a somewhat broad and flattened mesial nose with two nostrils which function perfectly (fig. 2). It is intended at a later date when she has reached puberty to make what further adjustments may be necessary from the cosmetic standpoint. I think the case is a good example of the advantages of combination between the general surgeon and the specialist.

(II) Mr. Muecke.

The four nostrils in the double nose went right through. The two external choanæ were very large, the two central very small, and the left septum was the best. This was fractured and stitched centrally. Both nostrils are now excellent; there is a complete membrane on both sides. She now has a normal nose, a little broader than usual.



Fig. 2.

Case of Functional Aphonia, Voice restored by application of Negus's Hand-pressure and Bárány's Noise Machine.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

THE patient, a woman of about 30 years of age, was instructed to press her hands together against traction exercised by a nurse holding them apart. When she did this actively the vocal cords and then the ventricular bands became approximated and the voice was restored to some extent. When the sounding noise-machines were placed in her ears her voice became quite loud.

DISCUSSION.

Dr. JOBSON HORNE asked whether this was really a case of functional aphonia. The larynx did not present the clinical appearances usual in such cases, and the voice had not been restored. Care should be taken as to how these cases were described. A wrong description might do the patient much harm. In the present case the loss of voice appeared to be due entirely to the state of the general health. Pains should be taken to exclude pulmonary tuberculosis. If that disease were present, no effort should be made by the patient to produce the voice. Loss of voice—at times mistaken for functional aphonia—was one of the earliest signs of the incidence of phthisis.

Mr. H. J. BANKS-DAVIS (President) said that sometimes "functional aphonia" was one of the earliest signs of tuberculosis of the larynx, and that this was an accepted fact.

Mr. H. M. WHARRY said he had tried the method in five cases, and in three, in which the voice was very weak, there was marked temporary improvement, but only as long as the pressure was kept up. In two cases in which there was no phonation from the cords he found no improvement from the pressure.

Dr. H. SMURTHWAITE agreed that one should make sure there were no signs of tuberculosis before calling a case functional aphonia. In functional aphonia, much depended on the observer's personality and power of moral suasion. There were two forms of functional aphonia—one spastic, in which the false cords came together, the other in which they did not. The instrument would have no effect in the spastic cases.

Sir JAMES DUNDAS-GRANT (in reply) said that apart from the question of tuberculosis in this case, he brought it forward solely to show the application of Negus's hand-pressure and the noise machine as adjuvants in the treatment of the functional aphonia which had existed for a number of months.

Fibro-lipoma of Larynx.

(Growths and Microscopic Slides shown.)

By JOHN F. O'MALLEY, F.R.C.S.

PATIENT, a male, aged 60. History of "asthma" for many years. "Catarrh" for five years; unable to sleep lying down. "Rasping" and "drawing" noise in the throat in the morning. Has a feeling of wanting to swallow.

July 21, 1919: Removed growth from the front aspect of the left ary-epiglottidean fold, at its junction with the arytenoid, by a cautery snare. The

tumour was free enough to hang down between the vocal cords, and it measured 1 in. in length, and ½ in. in thickness.

The growth recurred, and was again removed on February 13, 1923.

The stump has since been freely punctured with the cautery on two occasions and when last seen on July 23, 1923, it was about the size of a pea.

DISCUSSION.

Dr. IRWIN MOORE said these cases were very rare; Mr. O'Malley, in discussion on a similar growth exhibited by Mr. A. J. M. Wright,[1] in November, 1922, remarked that he had looked up the literature, and could find only fifteen cases recorded.

Mr. J. F. O'MALLEY (in reply) said he had dealt with this case over two years ago, and at that time the literature contained records of only thirteen cases. In front of the attachment of the growth in the larynx there was now a slight prominence, as if it were spreading down on the front of the arytenoid, and he thought this was an extension of the lipomatosis. He put the point of the cautery into it as it was trying to grow again.

Fibro-sarcoma of Soft Palate, treated by Radium.

By F. BRAYSHAW GILHESPY.

M. S., AGED 47, gardener. In June, 1922, he had difficulty in swallowing. When seen in the first week of July there was much swelling of the soft palate on the right side extending over the middle line to the left side.

Operation: Incision over growth through soft palate. The growth appeared to be encapsulated and could be shelled down to its base. On an attempt being made to remove it through the base the growth was found to be adherent to the deep structures of the neck, and a great amount of bleeding was encountered. The external carotid was tied, but the growth could not be removed owing to its firm attachment and excessive hæmorrhage.

Insertion of 110 mg. radium emanation on August 28, 1923. (The first insertion was made after the operation.)

The growth has diminished in size, but there is a good deal of superficial inflammation since the second insertion of radium. Wassermann negative. Growth: Fibro-sarcoma, with large blood spaces.

DISCUSSION.

Mr. E. M. WOODMAN (speaking for Mr. Gilhespy) said it would be helpful to hear what was the best method of dealing with such cases. Mr. Gilhespy started to dissect out the growth, and it shelled out satisfactorily from the top, but it was adherent to the bottom, where it was in contact with the carotid sheath, and there was severe bleeding. The exhibitor tied the external carotid externally, and hoped to remove it from the outside. He afterwards put in 110 mgm. of radium.

Sir WILLIAM MILLIGAN suggested that this case should be treated with a barrage of radium tubes, as the growth was fairly extensive; twelve tubes of 20 to 30 mgm. each, and that they should be left in for from twenty-four to thirty-six hours, the patient being afterwards subjected to deep therapy treatment.

Proc. Roy. Soc. Med., 1923, xvi (Sect. Laryngol.) p. 11.

Mr. G. W. DAWSON said that the glands on the right side of the neck were extensively involved. He had shown before the Section a case of sarcoma of the tonsil five years ago; and when operating on it, he removed all the glands, although at the time they did not seem to be involved. On section, however, they were found to be sarcomatous. The patient was still alive; for a year after the operation he was treated with repeated doses of X-rays.

Dr. WILLIAM HILL said he agreed that this was a case for the multiple application of radium, by the Manchester technique. Fibrosarcoma did not need so much screening from other kinds of rays as was necessary in the case of some other growths. This desperate condition justified desperate measures.

Section of Laryngology.

President—Mr. H. J. Banks-Davis, M.B., F.R.C.P.

Dr. Haslinger's Directoscope with Demonstration.

Shown by C. G. Russ Wood, F.R.C.S.

Mr. Russ Wood said this was a new principle of examining the larynx. As a rule the instrument could be quite easily passed, and gave a good view. At the same time it was self-retaining, leaving both hands free. Last week, under local anæsthesia, he removed an angeioma from the anterior commissure, of which he had got a very good view.

DISCUSSION.

Mr. H. J. Banks-Davis (President) exhibited an instrument, Struycken's Directoscope, probably the first of its kind, which enabled one to see the larynx easily in certain cases. He also showed a recent pattern, made like Hill's œsophagoscope, with a slit down the side. This was Seiffert's supposed improvement on the instrument Dr. Russ Wood had just demonstrated.

Sir William Milligan asked whether Dr. Russ Wood obtained better results with the "directoscope" than with the old Kirstein instrument.

Dr. W. Hill said he had used some of the similar instruments, and the same result was obtained if the same shaped spatula was used. The present apparatus was self-retaining, and was not so complicated as Killian's nor so expensive.

Mr. J. F. O'Malley said the two cases in which he used the instrument were edentulous patients, hence it was very easy to use. He thought it would be a very handy instrument.

Dr. W. H. Kelson said it would be interesting to know what patients thought of these devices. Twenty years ago the Kirstein instrument was shown at the Laryngological Society, and was tried by many, but patients did not like it here, and he thought that was the reason its use was largely dropped in favour of other methods.

Dr. Dan McKenzie said he had heard there were excellent possibilities for the use of this instrument, and patients did not seem to mind it at all. He asked a friend if the extension caused damage to the wall, and he said it did not.

Mr. Russ Wood (in reply) said that the use of the instrument was contra-indicated in post-cricoid growths, as there was much hæmorrhage afterwards. Patients could be examined with it either sitting up or lying down.

Tuberculosis of Nasal Bones.

By C. A. Scott Ridout, M.S.

E. T., aged 42, tramway motor man.

February, 1923: Semi-fluctuating area over the nasal bones: incised; very little pus: bare carious bone felt. Later: Mass of granulations noticed in

right nasal cavity, apparently attached to anterior and upper aspect of septum nasi.

Pathological examination of these granulations on three separate occasions was made and report gave opinion that the lesion was tuberculous.

Wassermann: Negative.

Treatment: Iodide of potash without any apparent result: and curetting on one or two occasions. Right otorrhœa of moderate amount has come on since onset of nasal condition and aural granulations have been cauterized.

Patient is allowed to do duty as pointsman and seems to be slowly improving.

The opinion of the Section is desired as to further treatment.

DISCUSSION.

Dr. CAVENAGH said that at present he had under his care a boy, aged 14, with a tuberculous past and a two months' history of nasal obstruction and a tender fluctuating swelling over the bridge of the nose. There was marked photophobia in the right eye, with irido-cyclitis. The upper region of the nose was full of the typical granulations found in these cases. He hesitated to adopt surgical procedures, as he thought plastic surgery would be required afterwards. He therefore tried a short exposure to violet rays twice a week. At the end of a fortnight there was definite improvement. Now, five weeks later, there was much less swelling and nasal obstruction and no tenderness or photophobia. He asked why iodide of potassium was used in this case, as the Wassermann was negative, and the condition was evidently tuberculous.

Dr. O. STRANDBERG (Copenhagen) said that cases of lupus vulgaris on the side of the nose could be treated with the Finsen light, as there was an affection of the bony nose as well. If there were no bone disease, he would prefer electric cauterization, placing a small electrode in the nose, and a larger one elsewhere on the body. The patient should then be well in a few weeks.

Mr. H. J. BANKS-DAVIS (President) said that the use of iodide of potassium in tuberculous cases was often disastrous, especially in the larynx: there was breaking down of the granuloma, and an aggravation of the disease.

Mr. RIDOUT replied that at first he thought the case was syphilitic, and even when the Wassermann was reported to be negative he thought so, as the microscopical sections were not characteristic. He did not want to do anything drastic, for fear the man might lose his work, and it was very difficult to secure Finsen treatment. He was slowly improving, and he allowed the man to go back to light work, in which he was in the open air.

Injury to Lingual Nerves in Guillotine Removal of the Tonsil.

By DAN MCKENZIE, M.D.

IN 1918 the patient, then a girl of 15, had her tonsils removed by the guillotine method. On recovery from the anæsthetic she noticed that the left side of the tongue was numb, and on examination I found it to be insensitive over the distribution of the left lingual nerve.

Since then some improvement has taken place. The tongue is less anæsthetic, but the sense of temperature has not returned as she still occasionally burns the tongue with hot food. On examination I find that she can now feel light touches over the affected surface, but they are said to be less distinct than on the right side.

DISCUSSION.

Dr. P. WATSON-WILLIAMS asked what was the precise condition of the tonsils in this case. Sometimes the tonsillar hypertrophy extended down to join the lymphoid aggregations at the base of the tongue, making it difficult to engage with the guillotine in the ordinary way, and this might lead to injury of the pillars.

Mr. G. W. DAWSON said that less had been removed on the affected side than on the other. After removal of tonsils patients sometimes complained of neuralgia or of a painful sensation in the tongue.

Dr. DAN MCKENZIE (in reply) said that he had not heard of or seen this accident before. He agreed that on the left side there was now some tonsil tissue showing, but that appeared frequently after total removal of the tonsil. Immediately after the operation the patient complained of anæsthesia. He did not know what the injury to the nerve could have been. He did not think it could have been divided.

Defect in the Vomerine Part of the Nasal Septum Associated with Bifid Uvula.

By DAN MCKENZIE, M.D.

THE patient, a man, aged 37, shows an absence of about one-third of the posterior part of the nasal septum. The defect is widest below near the floor of the nose, the posterior edge of the septum curving up to the usual position in the roof. The defect exposes to endoscopic view the posterior ends of the middle and inferior turbinals from the opposite side of the nose. The uvula is slightly bifid, and both abnormalities are obviously connected developmentally.

DISCUSSION.

Mr. J. F. O'MALLEY said he thought this was an exceptionally rare condition. Apparently the septum had undergone arrest, and the palate had proceeded to completion, except for the bifid uvula. He thought the posterior border of the septum seen now was not the same as seen in a person the posterior border of whose septum was formed by the vomer. It looked as if the greater part of the vomer was absent in this case, and that arrest of development had taken place at about the third month of fœtal life.

Dr. DAN MCKENZIE (in reply) said that Dr. Brown Kelly had written a long article on the subject published in the *Journal of Laryngology* for 1910. In this case there was no submucous cleft of the palate. Sir Arthur Keith had shown him a similar condition in the gorilla.

Nasal Septum Clamps.

Shown by DAN MCKENZIE, M.D.

THE clamps are used instead of packing after submucous resection. They cause very little discomfort, and seem sufficient to prevent hæmatoma.

DISCUSSION.

Dr. P. WATSON-WILLIAMS asked whether the patient could breathe through the nose with this instrument in position. He had used fine wire clamps (Brünings'), but he had found them of little advantage owing to the swelling of the nasal mucosa after operation on the septum causing difficulty in breathing through the nose. He there-

fore preferred the old plan of packing with a piece of rubber glove loosely filled with gauze; it was so easy to remove them without discomfort: in fact they sometimes removed themselves unless one took the precaution of tying them together in front.

Mr. J. F. O'MALLEY said that in some cases, where there was excessive swelling as a result of trauma, there was a danger that the resultant pressure might jeopardize the life of the flap. On that account he had given up using rubber splints.

Sir WILLIAM MILLIGAN asked whether it was considered really necessary to pack the nose after submucous operations. At the Manchester Royal Infirmary he had tried a series of twenty cases in which packing was done with the finger of a rubber glove with gauze in it, well glycerined; and twenty other cases where no packing whatever was used. He found the last twenty gave as good results, and the patients were very much more comfortable than the others were. One of the points in packing was to prevent the formation of a hæmatoma; and he had concluded that if, when replacing the flap, one did not sew it up at all, a hæmatoma was very unlikely to form, but that a couple of stitches and close approximation of the edges facilitated such formation. He, therefore, seldom packed the nose now, and he obtained quite as good results as when he packed.

Mr. A. R. TWEEDIE said that he had for some two years given up using any dressing or application after these operations. He had never seen any untoward complications, and had found patients much more comfortable as a result of this omission.

Dr. W. H. KELSON said he put one stitch in at the anterior inferior angle of the flap. By that means one secured a little space for leakage of blood, and no hæmatoma formed. Patients preferred not to have packing.

Mr. RIDOUT said he considered that hæmatoma was very rare after these operations, and he could see no reason for clamping. A little vaseline, or gauze, he thought ample.

Mr. F. F. MUECKE said he thought clamps were likely to cause sloughing. He said he had tested fifty cases, one series with packing, another with nothing in at all, another with a lightly-packed finger stall, and still another with so-called clamps. In consequence he now employed a little oiled silk rolled over gauze, made at the time the exact size necessary. The object was not to keep the septum straight in the mid-line—the operation should do that—but to prevent hæmatoma. The patients in two cases out of ten, if nothing were left in, would get hæmatoma, and the thickening would last a long time. He never sewed up. It was well to put in a hook and make a hole in one membrane three-fourths of the way back as a drain, and then the result was excellent. With the clamps it was impossible to affect the upper part. The oiled silk drained the secretion away and in a short time the nose looked normal.

Dr. W. HILL said that there might be hæmorrhage after operating on the septum, and that he always felt safer about a case if he packed, especially if there had been bleeding at the time. He put in a rubber or paper compress and he could then go home more contented, as he was less likely to be summoned to the case urgently.

Mr. CYRIL HORSFORD said that his invariable practice was to use Lake's splints, not only to keep the flap in excellent position, but especially if there had been a tearing of the flap, which could not be helped sometimes when there was a sharp-edged spur. The splint should not be too large. Most of the pain was due to blocking of the middle meatus from pressure of the splint or plug, which was too high up. He anchored the splint by a suture in front, because one splint got away and passed into the stomach and out in the fæces. Their removal was painless and bloodless.

Mr. A. J. M. WRIGHT said that one of his patients, before he (the speaker) had begun to tie splints or plugs in front of the nostril, inhaled them, and he had to extract them out from the bifurcation of the trachea through the tracheotomy wound.

Dr. DAN MCKENZIE (in reply) said that the clamps would not cause sloughing of the flaps unless the screwing were done tightly. The clamps were never closed absolutely, and he had found them work very well. Sometimes bleeding from the septum was a very serious matter, and when the packing was removed a day or two afterwards the bleeding was occasionally so severe that re-packing had been required. That had not happened when he had used the clamps.

Tumour of the Larynx.

By F. J. CLEMINSON, M.Ch.

THE patient is a female, aged 20, who came to the Middlesex Hospital, on November 6, complaining of hoarseness of five months' duration. She said that she had had a cough every winter for some years. There are some signs in the upper lobe of the right lung pointing to active or quiescent disease. Her sputum is being examined.

The larynx shows two small fleshy tumours, pinkish in colour. The larger and more posterior is apparently growing from the right ventricular band, and at its anterior end it partially overlaps a smaller mass, which seems to be growing backwards from the region of the commissure.

DISCUSSION.

Mr. T. B. LAYTON said he had the feeling that this was tubercle, but not an ordinary case. He thought the lower anterior lip, which he first considered was a soft fibroma of the anterior commissure, was a little piece coming from the right ventricle beneath the false cord and that there was some swelling and ulceration of the false cord.

Dr. JOBSON HORNE said it was not typical tubercle; he thought it was simple papilloma and nothing to do with the lungs.

Mr. CLEMINSON replied that he proposed to remove the tumour and would report to the Section what it actually was.

Postscript.—Later report: Tubercle bacilli were found in the sputum.

A Case of Enlarged Tonsils complicated by a Venous Nævus of the Palate.

By ELEANOR LOWRY, M.B., B.S.

THIS patient is shown in order to obtain advice regarding treatment.

H. M., a boy, aged 6, was brought to hospital on account of enlargement of the tonsils and glands of the neck, and cough.

The tonsils are much enlarged, and the glands of both cervical and right inguinal regions slightly enlarged. There is a venous nævus of the palate, extending to the uvula, right anterior pillar, and both alveolar margins.

The patient is anæmic, his blood count being as follows: Red blood corpuscles, 5,140,000; white cells, 21,400; hæmoglobin, 80 per cent.; colour index, 0·8; polymorphonuclears, 84 per cent.; large lymphocytes, 3 per cent.; small lymphocytes, 4 per cent.; transitional, 2 per cent.; eosinophils, 1·5 per cent.; basophils, 0·5 per cent.[1]

Nothing abnormal was found in heart or lungs.

[1] The report upon the blood-count was supplied subsequently to the case being shown.

DISCUSSION.

Mr. A. J. M. WRIGHT suggested that the nævus should be left alone; he considered the glandular condition was due to enlarged tonsils.

Sir WILLIAM MILLIGAN said he considered the nævus in this case was a very insignificant matter. If treatment were required for it—he did not think it was—electrolysis should be used. With regard to an operation on the tonsils, he would first like a detailed statement of the blood examination. There was a paleness in the throat and tonsil of this patient which made him think it might be only an accompaniment of a general blood change. If the white cell count was apparently normal, he saw no objection to removing the tonsils in the ordinary way.

Dr. JOBSON HORNE asked whether this was not a suitable case for London paste. He would not urge enucleation of the tonsil in this case, for the stretching and tearing of an enucleation might aggravate the condition of the nævus. But sufficient could be punched out to give the patient comfort. After that, with attention to health and the general condition, the tonsils might subside without any other treatment. It was important to investigate the blood, and to inquire as to hæmophilia

Dr. W. HILL agreed that London paste might be tried in this instance. There was an instrument, that of La Force, by which the tonsil was enucleated by a blunt blade, and there was an arrangement for tying a surgical pedicle, with no blood-vessels exposed. He had a case in which the coagulation time was fifteen minutes.

Dr. GILHESPY said he thought this was a case for dissection. The hæmorrhage could be controlled much better than with the guillotine.

Dr. LOWRY replied that she proposed to send the patient into the country for six months; she would then show him again. He had a very large leucocytosis, more than double the normal. There was no lymphocytosis. The red cells were over 5,000,000, the white cells over 21,000.

A Case of Frontal Sinus Empyema with an unusual position for a Fistula.

By ELEANOR LOWRY, M.B., B.S.

MRS. C., aged 64. The patient was first seen on September 1, 1920. She had a fistula high up on the forehead slightly to the right of the mid-line. Six months before, she had severe pain in the frontal region and a swelling which opened, leaving this fistula, from which pus had discharged ever since.

On examination, a probe only entered for ½ in., going slightly towards the left. No pus could be found in the nose. Skiagrams showed only thickening of the bone, with indefinite outline of the sinuses. The Wassermann reaction was negative.

On operation, the fistula was found to communicate with the left frontal sinus, which was large and extended almost to a line perpendicular to the external angle of the eye. This sinus was filled with pus.

The wound was treated with B.I.P.P. and drained from the external angle. The wound was healed, and the patient left the hospital within a fortnight. She has had no further trouble.

DISCUSSION.

Mr. T. B. LAYTON congratulated Dr. Lowry on the result, and especially on the extreme neatness of the scar. He had had a case with a swelling in the same position, and he had so feared an ugly scar resulting that he made a flap, as suggested by Mr. Tilley, and with rather disastrous results, for it retracted, and when some tension was put on it, a large piece sloughed. This present case showed that when one had to go into that region, the ordinary incision was not as bad as might be supposed. When a large swelling occurred there, how did the pus get under the skin? He understood that in this case it had come through the anterior wall of the vertical portion of the frontal sinus. In his own case it had not done so, having come through in the usual place, beneath the lacrymal duct, then tracking round and forming a swelling. In the case he had spoken of it would not have been necessary to do anything except make the ordinary incision.

Sir WILLIAM MILLIGAN said that the result was very good in respect to the supra-orbital incision. He had only once seen a fistula in this particular situation, and in that case there was a history of syphilis. He thought it resulted from a form of localized septic diploitis, a localized and mild infection, probably staphylococcic, and the fistula resulted from a localized abscess. In a paper on the frontal sinus operation by Sir Alexander Ogston, in 1884, the vertical incision was the one recommended, and he agreed with Mr. Layton that this incision gave surprisingly little deformity if it healed well. In his (the speaker's) early days he invariably operated in the middle line, and when there was bilateral frontal sinusitis it gave a very good approach. Was there a history of syphilis in this case?

Dr. GILHESPY mentioned a recent case in which there was a large swelling over the forehead, and when pressure was made on the head, pus came out of the nose. He (Dr. Gilhespy) made a vertical incision over the swelling and another under the eyebrow, and found the anterior wall over the frontal sinus eroded over an area equal to that of a shilling. This would have been a similar case if it had not been operated upon.

Dr. LOWRY replied that the Wassermann reaction was negative; in spite of that, however, she gave iodide of potassium for two or three weeks, but the patient became worse.

Section of Laryngology.

President—Mr. H. J. Banks-Davis, M.B.

DISCUSSION ON "THE COMPARATIVE VALUE OF COCAINE SUBSTITUTES."

Dr. Patrick Watson-Williams.

Cocaine, discovered in 1860, and first used clinically by Keller, in 1884, in ophthalmic practice, was thence onwards very widely employed as a surface anæsthetic, and for a long period for injection anæsthesia. Valuable as cocaine has proved in many aspects of medical practice, its toxicity and social abuse by the narcomaniac created widespread inquiry for "substitutes." The setting up of the American "Committee for the Study of Toxic Effects of Local Anæsthetics," under the chairmanship of our distinguished colleague, Dr. Emil Mayer, and in this country of the special committee under the British Ministry of Health, and this meeting of our Section, with as many as five other Sections of the Royal Society of Medicine, evidence determination to reach the most satisfactory solution of the problem.

The quest for a substitute for cocaine, safer for use in practice, yet with commensurate anæsthetizing properties, has provided us with quite a large number of substitutes for cocaine, some of very great value, which in a measure and for certain purposes (e.g., for infiltration anæsthesia and nerve blocking) have largely displaced cocaine and restricted its domain in practice; each in turn has been vaunted as efficient, low in toxicity and clinically safe. Some we have learnt to value highly, but how ephemeral the repute of the majority!

The Ideal Local Anæsthetic.

Turning to the comparative value of cocaine and its substitutes the two essentials for consideration are:—

(*a*) Anæsthetic efficiency.
(*b*) Safety in clinical dosage.

But the ideal local anæsthetic must not only be efficient and safe, but also

(*c*) Capable of sterilization.
(*d*) Readily soluble.
(*e*) Non-irritating.
(*f*) Devoid of temptation to the narcomaniac.

Local Anæsthetics and Narcomania.

May I first touch on the important question of drug-addiction, because the cocaine habit has unfortunately so far tainted the reputation of this medical godsend that it shares with morphine and other drugs the need for restrictive legislation to control distribution. Heroin was advocated as devoid of temptation to the narcomaniac till experience proved this assumption fallacious. Sir William Willcox[1] reminds us that "the claim that a new drug is not likely to lead to addiction habit requires the most careful and extended trial both experimentally and therapeutically, before it can be admitted." Have we sufficient evidence as regards any cocaine substitute that it affords freedom from "addict" risk, when it becomes as widely known and used as cocaine. At any rate most of the substitutes are highly selective for the cerebral centres, and some we know are capable of causing cerebral excitation. We all admit and recognize the danger of patients having open prescriptions for cocaine, but its use *for surgical purposes alone* is in a different category. Surely the "addict risk" from cocaine for such purposes has been exaggerated, whilst as regards the newer substitutes, in this respect, we are as yet in unknown territory.

Selection of Local Anæsthetics.

The comparative clinical value of cocaine and of its substitutes largely turns on the purpose for which anæsthesia is required; one which may be most efficient and also safe for a small operation field may be unsafe when the operative field is greatly extended. Hence, comparative values must be adjudged:—

(1) *According to the method* of inducing anæsthesia which is to be employed.

(*a*) By surface absorption.

(*b*) By infiltration or nerve blocking.

(*c*) By intraspinal block.

(2) *According to the region* to be anæsthetized.

For example, the mucosal epithelium of the urethra and of the conjunctiva seems thinner and more permeable by local anæsthetics than is the nasal mucosa, hence for the latter it is probable that a stronger anæsthetic is essential for efficiency than for the urethra or conjunctiva; while in surface anæsthesia or infiltration of the conjunctiva and lid, it may well be that the need of conserving the cornea from injury by the anæsthetic introduces factors which do not weigh with the rhinologist or general surgeon. Again, in the nose and throat surgery, it may be there are greater risks in the fauces and tonsils than the application of the same dosage of the same anæsthetic in the nasal passages.

After referring to the general questions involved, I propose to confine my remarks to *surface anæsthetics in relation to laryngological practice*, leaving all other territories in the abler hands of those who share with me the duty of introducing the subject.

Eucaine and Butyn as Surface Anæsthetics.

My personal experience of eucaine and butyn is restricted to their use by surface absorption, and my remarks have no reference to their employment by any other method.

[1] *Brit. Med. Journ.*, December 1, 1923.

Eucaine.—I have used since 1902 and somewhat later the more soluble eucaine lactate in 5 to 12 per cent. aqueous solution for rhinological purposes. It proved fairly efficient for anæsthesia, e.g., for the galvano-cautery. But though non-irritating it does not afford mucosal ischæmia, and hence its use usually involved the combination of adrenalin.

Butyn.—Butyn I have used for nasal anæsthesia in 2 to 5 per cent. aqueous solution. In its action it appears to be exactly similar to eucaine lactate but about twice or three times as strong in anæsthetic efficiency; also at least three times as toxic, weight for weight.

All known cocaine substitutes share with butyn and eucaine the serious defect that adrenalin must be added if vascular constriction is required, while in even 4 per cent. solution butyn is liable to cause irritation, and vascular dilatation unless this be counteracted by adrenalin. The application of adrenalin, however, is often followed by somewhat prolonged secondary turgescence and sometimes by irritation and coryza lasting for one or two days, a disadvantage which Dr. William Hill[1] has so well emphasized. For these reasons I avoid adrenalin whenever possible, almost restricting my use of eucaine or butyn to such cases as repeated Eustachian catheterization where, as is often the case, ischæmia of the mucosa is not required.

I have used butyn successfully in 5 per cent. solution for nasal sinus exploration, and in several patients, for the purpose of *comparison with cocaine*, I have used butyn and adrenalin on one side and cocaine and adrenalin on the other. I found that butyn, while equally if not more toxic, was not equal to cocaine in efficiency.

Realizing in my own case the fuller need for guidance in the choice of cocaine substitutes by further experimental research, I suggested to my son, Mr. E. Watson-Williams, a research on the toxicity of cocaine and its substitutes approached from the clinical standpoint. As he has already published his article in the current issue of the *British Medical Journal*,[2] I need not refer to his results at length.

One of the deductions of clinical value that these experiments seem to warrant is that if you first ascertain the minimum lethal dose of any of these local anæsthetics and then the largest dose that can be given in the same species without causing serious toxic manifestations, you get what he terms the *danger ratio*. He found, for instance, that "a small fraction of the lethal dose of cocaine may produce syncope, while a larger fraction of the lethal dose of butyn is required. The clinician estimates toxicity by the dose that exposes the patient to serious central nervous disturbances, e.g., syncope, dyspnœa, &c. Such a dose is less than the lethal dose, but with each species (of animal) each anæsthetic and possibly each variation in concentration, this dose which we may call the *maximum clinical dose*, may be a different fraction of the *minimum lethal dose*."

"Let us call this fraction the danger ratio. An anæsthetic with a low danger ratio is much safer for trial than one with a high, the risk of incautiously approaching the lethal dose being less." Hence with cocaine with low danger ratio patients are more likely to afford warning by manifestation of syncope or dyspnœa while one is still relatively far from a fatal dose; whereas we should expect butyn, with its high danger ratio, might afford less timely warning, because it is only when the dose more closely approaches the lethal dose that toxic symptoms would appear.

1 *Brit. Med. Journ.*, November 10, 1923.

2 "Cocaine and Its Substitutes." E. Watson-Williams. *Brit. Med. Journ.*, December 1, 1923.

COMPARATIVE EXPERIMENTAL TOXICITY: COCAINE = 100.

	Sollmann		Hirschel	Le Brocq ‡		Eggleston	Watson-Williams			Poulson	Cushny
	Rat*	Cat	Rabbit*	Mouse	Rabbit	Cat †	Mouse*	Rabbit *	G.-pig*	Collected	
Novocaine ...	40	30*	15	50	50	35	13	13	10	—	33
Stovaine ...	—	—	—	62	68	55	10	20	30	50	60
Eucaine ...	—	—	—	40	40	120	—	—	—	25	40
Alypine ...	—	—	—	125	135	150	75	100	70	100	100
Nirvanin ...	—	—	—	70	—	50	—	—	—	—	50
Tropacocaine	—	—	—	50	55	75	—	—	—	33	50
Holocaine ...	—	—	—	—	—	150	—	—	—	500	60
Apothesin ...	—	—	—	—	—	75	—	—	—		

	Nielsen				Biehne
	Rabbit †	Rat *	Cat *	Dog *	Monkey *
Butyn ...	100	200	90	81	100

	Sollmann		Watson-Williams			Nielsen			Biehne
	Rat*	Cat	Mouse*	Rabbit *	G.-pig*	Rabbit †	Rat *	Cat *	Monkey *
Butyn ...	250	100†	200	170	80	100	200	90	100

* Subcutaneous. † Intravenous. ‡ Method not stated.

Mr. E. Watson-Williams's table, to compare the experimental results obtained in various animals, by estimating the minimal lethal doses of a number of cocaine substitutes, by several observers; including some of his own experimental observations.

Mr. W. M. Beaumont's article on butyn[1] as a local anæsthetic led many in this country to investigate its merits, but as his experience was limited to ophthalmic practice, I turn to Dr. Bulson's earlier research on butyn for nose and throat work.[2] After citing the researches of Professor Sollman showing that butyn was as toxic as cocaine, if not more so, when injected in cats, "that butyn does not appear promising for injection, since its toxicity is materially greater than that of cocaine," Bulson describes his clinical experience of butyn as a surface anæsthetic. He believes that the combination with epinephrin provides more profound anæsthesia than butyn alone, and decreases the chances of toxic effects of butyn. Bulson advised 2 per cent. butyn solution for light anæsthesia, but for major operations a 5 per cent. solution combined with epinephrin was used by first swabbing the operative field and within two or three minutes applying to the operative field, and, if possible, over the source of sensory nerve supply, pledgets of cotton wool soaked with the solution but with the surplus squeezed out. About ten minutes later he found "the patient in practically all cases suffered no pain or even discomfort." Bulson states that thus used "many times for major intranasal operations 5 per cent. butyn has not caused the slightest sign of toxic disturbances." "However," he continues, "it is very probable that if ever butyn is used as extensively as cocaine, there will be cases of toxic effects reported."

1 *Brit. Med. Journ.*, January 13, 1923.

2 "A New Local Anæsthetic for Nose and Throat Work." *Annals of Otology, Rhinology and Laryngology*, March, 1922.

I have certainly felt diffident of the toxic risks in using butyn for operations where large surfaces have to be anæsthetized, and apparently with good grounds, since Bulson's recommendations must be considered dangerous. When Mr. Eric Watson-Williams, in connexion with his research, got into communication with Dr. Biehne, of Chicago, representing the Abbott Laboratories, the manufacturers of butyn, he very kindly furnished notes of five fatal cases from butyn, and in a further letter dated November 13 last, Dr. Biehne recommended that the strength of butyn solution *should not exceed 1 per cent. for topical application to the nose.* We cannot fail to appreciate this frank and prompt communication of regrettable results, not always associated with commercial enterprise.

These letters to Mr. E. Watson-Williams (see pp. 32, 33) contain notes of the greatest importance in connexion with butyn, some highly favourable, some the reverse, but all helpful.

Butyn, like cocaine, novocaine, stovaine, in lethal doses causes convulsions and death from respiratory paralysis. Let us disabuse our minds of the fallacy that these cocaine substitutes are safe, unless the dosage is carefully estimated. Emil Mayer,[1] in a preliminary report of this Committee for the Study of Toxic Effects of Local Anæsthetics, has collected reports to date of forty-two deaths following the use of local anæsthetics occurring *within the last three years*, independent of those reported on by former committees of the Association. The deaths reported:—

Anæsthetic	Number
Stovaine	1
Alypin	1
Procaine or Novocaine	3
Novocaine and Cocaine	10
Apothesin	4
Butyn	4
Butyn and Cocaine	1
Cocaine	18
	42

Seeing that cocaine is used with far greater frequency than any of its substitutes, and that many of the substitutes are not in general use, we are not exaggerating in stating that none can be called safe.

How misleading such tables of figures may be is demonstrated by E. Watson-Williams' analysis of the figures furnished by the same Committee in 1920[2] when fifteen deaths were attributed to cocaine alone, yet of these, five were obviously due to accidental overdosage, in six more the dose was unknown, while in four cases death was attributed without supporting detail to dosage of gr. $\frac{1}{50}$, gr. $\frac{1}{8}$ and gr. $\frac{1}{4}$ (two) respectively.

However valuable in other regions, I ask does butyn afford any advantages as an intranasal surface anæsthetic as compared with eucaine which I believe is safer and less irritating, or as compared with any other substitute?

Orthoform and anæsthesin I have found valuable in relieving the pain from tuberculous ulceration; the more prolonged analgesia and avoidance of danger inseparable from the use of cocaine in such conditions are noteworthy features.

Cocaine.

It is clearly impossible to estimate correctly the comparative value of cocaine substitutes without due consideration of the relative efficiency, and

[1] *Journ. Amer. Med. Assoc.*, vol. lxxxi, No. 11, September 15, 1923.

[2] Mayer, *Journ. Amer. Med. Assoc.*, 1920, p. 315.

toxicity of cocaine itself. I date my usage of cocaine from 1886. For routine nasal examination and for minor procedures in the consulting room in earlier years of practice up to about 1890, I usually employed 10 per cent. aqueous solutions for surface anæsthesia in the nose, and although I was not incautious, on several occasions patients became faint and had to be laid out flat and restored with sal volatile, &c. Perhaps I was not sufficiently careful to avoid any being swallowed, a very important precaution. Believing that half the volume in double strength is safer, for at least twenty-five years I habitually employed in routine examination 20 per cent. solutions, spraying in 5 to 10 minims without adrenalin, and only very rarely indeed did patients display any symptoms of distress due to cocaine for light anæsthesia. I now use 2 to 10 per cent.

For exploration of the nasal sinus I apply 20 per cent. cocaine with about one in 10,000 or 20,000 adrenalin, followed by solid cocaine crystals in adrenalin chloride 1 in 1,000 sufficient to dissolve the cocaine, and cautiously applied on a very small cotton-wool probe inserted in the middle meatus and against the anterior sphenoidal sinus wall; 3 gr. of cocaine crystals being a full dose never exceeded.

I have performed many thousands of sinus explorations with cocaine and adrenalin applied as described and sometimes under conditions the reverse of favourable; for instance, by the kind invitation of colleagues, cases were thus explored at Bournemouth Hospital, and again last summer four were explored at Manchester, all out-patients, and in every case the patients got up and went home an hour or so later, without any untoward symptoms occurring.

In the larynx for cauterizing or removing small benign or tuberculous growths, &c., I usually spray about 20 per cent. cocaine followed by swab applications and the same for bronchoscopy, only of course applied more extensively.

Looking back many years I can only recall three cases of serious faintness following strong cocaine applications; the worst case was in a highly nervous, very weakly and anæmic colleague, over 60 years of age, whose sinuses I explored; he was somewhat breathless and faint for over an hour, but nothing worse. Once I found a patient who liked the elation following cocaine enough to enjoy being cauterized, but I never met one who contracted or seemed tempted to contract a habit, though many have expressed a dislike to cocaine.

There are two adjuvants which are of great value in lessening the risks attending the use of cocaine in surface anæsthesia.

First, the preliminary or simultaneous use of adrenalin. But as suprarenal extract was first described by Schafer in 1895 many of us had been using cocaine extensively before adrenalin was available.

Secondly, in 1888 De Haviland Hall pointed out that alkalinization of cocaine solutions, by adding a trace of sodium bicarbonate immediately before application, greatly increased the anæsthetizing potency of a given strength of cocaine solution.

For thirty years cocaine has been used extensively in every throat clinic the world over; imagine the thousands of patients in this country alone, for whom cocaine is used every week, often enough by those who have no special guidance in the dosage and the best method of applying it; yet how many cases of serious accidents can we total? how many cases of narcomania can we trace to this extensive surgical usage? It is true that deaths have occurred from careless

accidents of giving excessive doses beyond the acknowledged limit of safety, but it is equally true that deaths have been falsely attributed to cocaine, while in some other instances so-called deaths due to cocaine are beyond credence, or based on reports devoid of essential detail. It is a grave error to overstate the case for or against the clinica use either of cocaine or of any of its substitutes.

Thus far I have yet to hear of the substitute which enables us to dispense with the use of cocaine as an intranasal surface anæsthetic.

Mr. R. Foster Moore.

I will endeavour to be as brief as possible in what I have to say, for it seems to me that this discussion will best serve its purpose if the experience of everyone here, to the exclusion of theroretical considerations, can be made available to our knowledge, for the combined experience encompassed by this meeting must represent many thousands, or perhaps hundreds of thousands, of observations.

I judge from the published title that it is cocaine substitutes, and not cocaine itself, which are to form the subject of discussion. I imagine, however, that we shall all feel disposed to use cocaine as our standard of reference and to speak of substitutes in terms of this adopted standard.

The following are the most important drugs which have been used for local anæsthesia :—

(1) Holocaine ; (2) α- and β-eucaine ; (3) eupthalmine ; (4) novocaine ; (5) anesthesin ; (6) subcutin ; (7) antinesin ; (8) alypin ; (9) stovaine ; (10) tropacocaine ; (11) butyn.

I have details here with respect to all of these drugs as regards their reputed anæsthetic powers, toxicity, liability to become drugs of addiction, action on the corneal epithelium, &c., &c. It is clearly impossible to give them now and I do not think it would assist us in our discussion.

Local anæsthetics are used in different ways and in different strengths by the members of the various sections represented, but there are one or two points which are common ground about which valuable information may be forthcoming.

First, I would suggest the important question of addiction, as to whether any one present has certain knowledge of cases in which addiction has resulted from his own use of cocaine for surgical purposes, or as a result of his prescribing it, or whether he has knowledge of any case of addiction in respect of any of the cocaine substitutes.

This latter point is of considerable importance, for these drugs are little known, and, were cocaine not available, it might well be that one or other of them would be found to have an effect which appealed to the narcomaniac. It may, however, be said that there is at present some degree of inducement to use them in this way, for there are not the penalties attached to the possession of them that hold in the case of cocaine, and hence, if it can be shown that in spite of their accessibility there is no abuse of them, it would be evidence of some weight that they are not addictable drugs.

A second point of common ground is the degree of toxicity of the various drugs. Other important points for our guidance have been put forward by Dr. Watson-Williams.

From the ophthalmic point of view we have to consider whether there is a drug, or combination of drugs which, when weighed against cocaine, is its equal or superior in respect of the following points :—

(*a*) Degree of anæsthesia and the rapidity with which it is attained.

(*b*) The amount of irritation caused, remembering that some of the drug may get inside the eye during an operation, and afterwards cause iritis.

(*c*) Whether it is more or less liable to cause desquamation of the corneal epithelium.

(*d*) Whether it is as well absorbed if the eye is not quite free from injection.

(*e*) Toxicity.

(*f*) Stability, whether it can be sterilized by boiling.

(*g*) Whether it stimulates the sympathetic nerve.

(*h*) Its liability to become a drug of addiction.

May I state briefly the chief uses of local anæsthetics in ophthalmic work?

(1) *Instillation Anæsthesia.*—This is the most important method of use, solutions of various strengths being dropped into the conjunctival sac. Anæsthesia produced in this way is made use of for tonometric examinations, for removal of foreign bodies from the conjunctival sac or cornea, for cauterization of the cornea, for syringing the lachrymal ducts and, by a long way the most important, a group which we may include under the head of "intra-ocular operations on uninflamed eyes except where the patient is so young as to render general anæsthesia desirable." It is this last group of intra-ocular operations which I believe is the crux of the whole question for the ophthalmic surgeon, for I am sure there are a number of cocaine substitutes which are sufficiently efficacious for surface anæsthesia.

(2) *Infiltration Anæsthesia.*—This method is used in plastic operations on the lids for Meibomian cysts, in a few cases for intra-ocular operations on inflamed eyes and enucleation of the eyeball, where a general anæsthetic for some reason is not available, and for injecting into the orbicularis as a preliminary to a cataract extraction, to produce temporary paralysis of the muscle and so prevent squeezing on the part of the patient.

It seems to me that for all these purposes novocaine in 1 per cent. or 2 per cent. solution is on the one hand so efficient, and on the other so little toxic, that cocaine does not enter into competition with it for this part of ophthalmic work, and I would suggest that time will be saved if from the point of view of infiltration it be eliminated from the discussion; we have, however, to consider whether it may take the place of cocaine when used by instillation.

(3) *Used as a Solid.*—Cocaine hydrochlorate is used in its solid form for topical application in incising Meibomian cysts and in muscle operations. The point to consider here is to what extent the solid form of other drugs can be used in the same way, or whether novocaine injections are not equally satisfactory for these purposes.

Another quality of cocaine is its power of stimulating the sympathetic nerve, producing dilatation of the pupil and widening of the palpebral fissure.

It is sometimes claimed as an advantage of other drugs that they have no action on the pupil, but I fancy most observers will consider both this action and the manner in which cocaine can be used as a test for paralysis of the sympathetic nerve as an important quality.

Information with regard to other drugs in this respect would be valuable.

It has been suggested to me that the stimulating action on the sympathetic nerve might prove to be a test of the power of a drug to act as a stimulant of the central nervous system, and that so it might become a test of the liability of a drug to become a drug of addiction—a valuable test should it be substantiated.

My own experiences of cocaine substitutes are as follows:—

(*a*) I have already expressed my opinion of novocaine for infiltration.

(*b*) I have not used it for instillation.

(*c*) I have found holocaine in 2 per cent. solution satisfactory for superficial anæsthesia by instillation, but it causes distinctly more smarting than cocaine of similar strength. I have not used it for intra-ocular operations.

(*d*) I have used 2 per cent. and 4 per cent. stovaine by instillation both for superficial anæsthesia and for cataract operations; it is more irritating than cocaine, it gives a satisfactory superficial anæsthesia, but I have thought it unsatisfactory for cataracts, and on two occasions have had to complete the operation with the aid of cocaine.

(*e*) I have used butyn a few times in the form of gelatine lamellæ and 2 per cent. drops for superficial anæsthesia only; the anæsthesia seemed satisfactory but smarting was rather marked.

I know of no certain case of addiction to any of these drugs as a result of their clinical use by myself or others; the most I can say of one case is that I suspect the cocaine habit was contracted as a result of the use of cocaine ointment for a fistula in ano.

Mr. F. N. Doubleday.

The British Dental Association[1] recently issued to all their members a circular letter, asking categorically: "Do you regard the use of cocaine (as distinguished from novocaine and other cocaine substitutes) as indispensable in your practice. Of 2,327 replies received, 1,912 said "yes," and 410 "no"; seven were indefinite in their answers. I am among the "noes." My experience is that novocaine injected hypodermically produces as rapid an onset of anæsthesia, and as profound a loss of sensation as cocaine. Looking back over my notes for the last sixteen years I find from a paper published in 1907[2] that we were then experimenting with β-eucaine hydrochloride. Since then I have employed β-eucaine lactate, stovaine, quinine and urea, novocaine, alypin, apothesine, butyn, and various other drugs, and combinations of drugs, with and without adrenalin. Some have been tested again and again at intervals, and the most important ones have been retried in the last month, with a view to this meeting. The result leaves me with a firm belief that novocaine is the most efficient cocaine substitute.

I use two E tablets, containing 0·02 grm. novocaine and 0·0005 grm. of adrenalin, freshly boiled in 3 c.c. of Ringer's fluid, making a $1\frac{1}{2}$ per cent. solution. This is the preparation subsequently referred to as novocaine, the dosage of it being given, in speaking of particular points.

No one present this evening will deny that dental operations form a means of accurately estimating the anæsthetic properties of a drug. What then will this novocaine solution do? I will give you three examples.

(1) A single injection of 1 c.c. into the neighbourhood of the nerve entering the apical foramen of a tooth will, within one minute, so anæsthetize a live sensitive tooth pulp as to enable it to be painlessly removed, or, alternatively, will allow a sensitive cavity in dentine to be cut without pain.

(2) Fifty mils injected into the soft tissues on either side of the alveolar

1 *Brit. Dent. Jour.*, Oct. 1, 1923, p. 1133. These figures are corrected to the day of this present discussion.

2 F. N. Doubleday, "Local Anæsthesia," *Guy's Hospital Gazette*, March 9, 1907, p. 98.

process will, within one minute, permit of the removal of any tooth, except a mandibular molar, without discomfort to the patient.

(3) Two to three c.c. injected into the neighbourhood of the inferior alveolar nerve will produce complete hemi-anæsthesia of the mandible, permitting painless removal of any mandibular tooth of that side. This anæsthesia takes from five to thirty minutes to develop according to the skill with which the solution is placed into the region of the inferior dental foramen. Although I have frequently removed buried lower third molars involving the use of hammer, chisel and electric engine for over an hour at a time, these have all been done with novocaine and the patients are still my friends. It is easy to make such statements here, but I believe that if a clinical demonstration meeting were to result from our discussion this evening, we dental surgeons should learn much from watching the employment of local anæsthetics by throat, orthopædic and other surgeons and that you could learn something from us.

Most of the other cocaine substitutes which I have mentioned yield similar operative results, but with less certainty and with more marked after effects. For example, last Wednesday, in view of this discussion, I injected into a patient 1 c.c. of a 1½ per cent. solution of β-eucaine lactate to de-sensitize a carious lower incisor. The anæsthesia took nearly five minutes to develop, and within ten minutes the patient was complaining of very marked irritation in the region of the lower lip. These sorts of complaints are never made with novocaine. I think they are due to the instability of the drug. For example, it is interesting to boil apothesine with tap water, and see how easily it precipitates.

There are a few patients who show marked susceptibility to novocaine. For example, on January 23, 1914, Miss D., aged 19, had an injection of 1 c.c. of novocaine with adrenalin for the de-sensitization of dentine; immediately after the injection she showed marked symptoms of collapse, with sweating and a low-tension pulse; in about ten minutes she recovered without further ill effects.

On February 15, 1918, she had a similar submucous dose of novocaine with adrenalin, but this time with 1-30th gr. of strychnine sulphate added. She showed no toxic symptoms, nor did she do so after subsequent similar injections on March 4, 1919, April 13, 1920, May 1, 1923.

Miss B., aged about 28, had an injection of 150 mils of novocaine on April 12, 1914, and showed symptoms similar to those of the last patient. She became so averse to novocaine that she had no more local anæsthesia until September 22, 1921, when she had novocaine, adrenalin and strychnine for the extraction of a tooth without incident. This was repeated on November 13, 1922, and November 6, 1923, in each case without ill effect.

In my own practice these are the only patients definitely marked as being susceptible to novocaine, and in each case the addition of strychnine checked their symptoms. In the last sixteen years at Guy's I have constantly used novocaine for my hospital patients, without any of them experiencing serious ill effects, although many of them are patients whose physical condition is poor, and some of them were suffering from serious organic diseases. Although I have done some hundreds of local anæsthetizations, each year for several years, I have seen no serious ill effects, nor have I known of any patient developing a drug habit from the use of cocaine, or other local anæsthetics. Examining the dental literature of this country for the last five

years I find four recorded cases of death. In all of them an unqualified practitioner was employing a proprietary preparation of cocaine, and the cause of death was in two cases recorded as "heart failure," in one as "shock" and in one as "cocaine poisoning."

Dr. Blomfield.

My contribution to this discusion, speaking as an anæsthetist, must be very brief. The use of these drugs in my hands is confined to injection; I do not use surface anæsthesia. I—and I believe most of my colleagues too—have entirely abandoned the use of cocaine. I think the drug almost entirely used for local injection is novocaine, in strength up to 2 per cent. And in that connexion it may be permissible to point out the enormously large amounts of novocaine which can be, and commonly are, injected without, apparently, the production of any untoward symptoms. One sees its apparent harmlessness especially when it is injected into the sacral canal, even up to 200 c.c. But, as Mr. Rood showed me, occasionally, when these large quantities are injected there are alarming symptoms at the time, though I believe no fatality has been recorded from this. I mean generalized convulsions, affecting nearly all the muscles. One such case I saw seemed to be going through a severe epileptic fit; but these convulsions always passed off without subsequent harm.

The only other drug of which I have much experience is stovaine. We choose this when it is a case for spinal injections. Some of the fatalities from stovaine reported do not, on inquiry, accuse that drug. But stovaine is much less frequently now productive of bad symptoms, since we have learned that its chief danger is its lowering of the blood-pressure, and that can be counteracted by the head-down position of the patient very soon after the drug has been introduced.

Sir William Milligan.

Otological work does not permit of the same range of utility for the employment of local anæsthetics as does laryngological, rhinological, ophthalmic or dental work. This is largely due to the configuration of the organ of hearing, to its geographical position in the body and to its somewhat remote accessibility.

Such local anæsthetics as cocaine, β-eucaine, stovaine and novocaine, have all had champions at one or other time, but for practical purposes cocaine has easily held the field. The advent of any new anæsthetic calls for critical examination from several points of view: (1) Is it as effective or more effective in its action than those already in use? (2) Is it reliable? (3) Does it possess toxic properties, and if so, how does it compare with other local anæsthetics?

My remarks this evening are mainly directed to the therapeutic action of the new synthetic anæsthetic butyn, a product of the butyl series, the technical name of which, possessing as it does fifty letters, I shall not even attempt to pronounce.

So far as aural work is concerned, it may be used applied directly to the tissues in fluid or in solid form, by subcutaneous injection, or by means of infiltration. To be effective in aural work, it must be allowed to come into close contact with the mucosa of the middle ear cleft, either by swabbing or by the local application of small pads of non-absorbent wool soaked in a

watery solution of the drug. Its range of utility is therefore largely narrowed down to those cases of middle ear disease where the tympanic mucosa is exposed either as the result of disease or from artificial perforation of the membrana tympani.

Instilled into the meatus as a preliminary to the removal of polypi or granulation tissue, it proves quite as satisfactory as cocaine, but I do not think more so.

The pharyngeal orifice of the Eustachian tube is available for the local application of the drug, or a few drops of an aqueous solution may be driven up the lumen of the tube in cases where the use of bougies is called for.

Applied locally to the mucosa of the middle ear or Eustachian tube orifice, it will be found to produce anæsthesia within from one to two minutes. Such anæsthesia is sufficient to arrest the pain following the application of such caustics as chromic acid or pure phenol, but not sufficient to afford relief if a stronger caustic, such as tri-chloracetic acid, be employed. The repetition of the application of butyn solution every three minutes until four applications have been made produces, however, a more satisfactory and profound anæsthesia which lasts about half an hour. My experiments have been made with 1, 2 and 5 per cent. watery solutions. The fact that a slight hyperæmia is induced, which, however, may be controlled by adrenalin, and that the drug has no ischæmic effect, are serious disadvantages, not, however, so serious as in nasal work, but nevertheless disadvantages. The anæsthesia has, however, the advantage of being more profound and more lasting than post-cocaine anæsthesia.

With regard to its toxic properties, when applied locally as above described, I have no experience, not having had any patient who was in any way inconvenienced by its use. When applied to the intact membrana tympani in watery solution as a preliminary to that painful proceeding, incision of the drum-head, I have not found it of much use, certainly of no more use than cocaine applied in the same way. If, however, dissolved in aniline oil as cocaine is so dissolved in Gray's solution, it has undoubtedly marked anæsthetising effects.

In painful affections of the external ear, or external auditory meatus, such as furunculosis, periostitis, &c., from the nature of the integument it cannot, if applied locally, be expected to be of much service, nor is it of any use in my experience. Injected subcutaneously, however, in from $\frac{1}{2}$ to 1 per cent. solution, it produces, within ten minutes, an anæsthesia sufficient at any rate to incise a furuncle without much discomfort, although personally I would very much prefer chloride of ethyl or nitrous oxide.

For the production of infiltration anæsthesia a 0·5 per cent. solution gives quite satisfactory results and produces a deep and lasting effect. I have not as yet, however, attempted to open the mastoid cells under butyl infiltration-anæsthesia, but preliminary to incising a sub-periosteal mastoid abscess it answers quite well. The number of cases where it is not possible to secure anæsthesia, where there is some idiosyncrasy making the patient intolerant, is up to the present in my experience non-existent.

In aural work butyn as compared with cocaine does not appear to me to offer any particular advantages, indeed it rather exhibits certain disadvantages. Its failure to cause contraction of the tissues in an organ like the ear, in which there is but little room to work, is a real and serious objection, while its tendency to induce hyperæmia and therefore more bleeding, seriously prejudices the many other excellent properties the drug possesses.

Judging, however, from my experience of butyn in aural work, I have found (1) its action to be more rapid, more powerful and of longer duration than that of cocaine; (2) its potency to be nearly twice that of cocaine, but in the amount actually required less toxic than cocaine; (3) the advantage that it is not damaged by keeping or by boiling; (4) the fact that it does not appear to engender a "habit"; (5) it can be used effectively in lower concentrations than other synthetic anæsthetics.

On the other hand, butyn has the great disadvantages of: (1) not possessing ischæmic properties; (2) of having a larger fraction than cocaine of the M.L.D. so far as the production of dangerous symptoms is concerned: (3) of being incompatible with chlorides; (4) of being more costly.

In the early days of practice I had from time to time patients who presented symptoms of cocaine poisoning, which in some instances gave me great anxiety. These mishaps were, I have no doubt now, due to faulty technique, as for years past, and with a very much larger experience, I have had no untoward results at all. I have, however, had the unfortunate and distressing experience of having had two patients who contracted the cocaine habit—one a barrister, who constantly used a nasal spray and developed such mental symptoms as necessitated his being confined in an asylum for a time, although he is now quite well; and the other a medical man who, as the result of vascular changes, suffered from a severe hemiplegia which unfortunately proved fatal.

Weighing the advantages and the disadvantages of butyn and cocaine in the scales as local anæsthetics, I must confess to a preference for cocaine.

Mr. E. Watson-Williams.

It is my intention to confine my remarks to butyn, of which we are hearing much. By the great courtesy of Dr. Biehne, of Chicago, representing the proprietors, I have received recent information on this subject of much interest and importance, and I propose to read to you the substance of two letters from him.

This anæsthetic was selected after numerous trials and experiments with a number of analogues, and animal investigation was begun in 1919. It appeared on the market late in 1921. Since that time there has been a modification of view on one important point—its toxicity, sufficiently indicated below.

As we know, animals with relatively small brains will survive much larger doses of cocaine, weight for weight (and of most cocaine substitutes) than those with relatively larger brains, and the latter survive larger doses than man. The view was put forward that butyn was not "selective" in this sense, but possessed a more uniform toxicity with different species. While, therefore, in experimental work on rabbits and dogs butyn appeared as toxic as cocaine, or more toxic, it was suggested that in man butyn was *less* toxic than cocaine. Further experiments on monkeys have shown that this view cannot be maintained, and that butyn must be regarded, gramme for gramme, as at least as toxic as cocaine.

In Bulson's paper in March, 1922, and again in an advertisement dated March, 1923, 5 per cent. solution is the strength advised for topical applications to the nose. Since that time the recommendations have been modified.

First Letter from Dr. Biehne.

"*August* 2, 1923.

"Details of five deaths following the use of butyn:—

No. 1.—Woman, aged 35. Good health, except a goitre. Nasal septal resection; morphine 1/6, atropine 1/200 hypodermic. 'Cocaine sprayed in the nose, followed by

5 per cent. butyn on tampons. Death with convulsions, in ten minutes. Intra-cardiac epinephrin unavailing. No post-mortem.

No. 2.—Boy, aged 16. Nasal septal resection; morphine 1/6, atropine 1/200, hypodermic. '1 per cent. cocaine sprayed in nose. This was washed out and 5 per cent. butyn on tampon. Notwithstanding efforts for resuscitation, boy died in about twenty minutes. Post-mortem: Enlarged thyroid, persistent thymus, general enlarged lymph glands.' Diagnosis: Status lymphaticus.

No. 3.—Woman, aged 38. Removal of tonsils. Morphine $\frac{1}{6}$ half an hour before operation. '4 per cent. cocaine on tampon to pillars and base of tongue. Then 2 drm. of $\frac{1}{4}$ per cent. butyn injected into right tonsil. Patient became nervous, but following removal of corsets seemed to be relieved. Left tonsil was then injected in a similar manner with 2 drm. of $\frac{1}{4}$ per cent. butyn. Before operation could be started patient fainted; then convulsions.' Efforts at resuscitation failed. Death due to paralysis of respiratory and circulatory centres.

No. 4.—Man, aged 26. 'Tonsillectomy in doctor's office. Tonsils injected with unknown amount of butyn solution prepared by adding 2 drm. of 5 per cent. solution to 3 oz. of distilled water and 12 drops of epinephrin': i.e., 2/5 per cent. solution. 'After injection of one tonsil patient became apprehensive and stated he feared he could not stand the sight of blood. He was reassured, however, and the other tonsil injected. In a few moments he fainted, passed into convulsions and died. No post-mortem. Case reported as death due to surgical shock.'

No. 5.—40 c.c. of 2 per cent. solution was used for sacral anæsthesia. Death in ten minutes. A case of overdose.

We do not recommend the use of butyn solution, particularly when combined with epinephrin for hypodermic injection in such vascular areas as the tonsil, nor do we recommend the use of concentrated solution for application to the nasal septum. Butyn is more efficient than cocaine: it acts more rapidly and the effect is more prolonged. Their toxicities, weight for weight, are about equal, but as butyn is much more efficient it should be used in more dilute solution. Individuals with status lymphaticus show an idiosyncrasy to butyn."

Second Letter from Dr. Biehne.

[Received after my paper in *British Medical Journal*, December 1, had gone to print.]

"*November*, 1923.

"Reports have just been submitted to us in which butyn 1 per cent. solution has been used in over a million cases by dentists for extraction purposes without a single untoward result. The physician in charge of the Ford Hospital in Detroit has used butyn $\frac{1}{4}$ per cent. solution in over a thousand tonsillectomies with perfect satisfaction and no toxic symptoms.

"We do not recommend the use of butyn in over 1 per cent. for topical application to the nose. Owing to the fact that butyn does not produce an ischæmic effect like cocaine it is much more rapidly absorbed. There have been no reports of untoward effects from the use of 2 to 5 per cent. butyn solution in the eye."

On the general question of deaths reported following the use of local anæsthetics, especially for tonsillectomy, I wish to emphasize the unwisdom of drawing conclusions from any but complete case reports. In a number of the reports I have recently examined, no mention is made, for example, of swabbing or spraying the fauces before injecting the tonsils or faucial pillars—an almost universal practice. If strong solutions are used on swabs, absorption here may be very rapid[1]; to give the dose only of the drug *injected* may be very misleading, and give the quite possibly false impression that small fractions of a grain of cocaine, novocaine, or the like, can cause death.

[1] $5\frac{1}{2}$ minims of 20 per cent. contain 1 grain of cocaine.

Mr. F. St. J. Steadman.

I have for several years past been employing local analgesia for various dental operations such as extraction of teeth, the removal of cysts and odontomes, the preparation of sensitive carious cavities for filling, the removal of serumal tartar, and so on.

I usually prefer to inject the larger nerve trunks away from the area of operation, rather than a submucous infiltration at the site of operation. For instance, for the extraction of a first permanent molar tooth, I inject the inferior dental nerve at the point where, in the pterygo-mandibular fossa, it enters the mandibular canal, rather than make a papillary injection in the immediate neighbourhood of the tooth; my reasons being that this site of injection is farther away from the area of operation and therefore less liable to be complicated by sepsis. In the maxilla, I have made many injections into the second division of the fifth nerve as it crosses the spheno-maxillary fossa, *en route* to the floor of the orbit.

I have not dared to use cocaine for these injections, since this drug is decomposed by boiling, and I am afraid of introducing sepsis, the consequences of which, in these deep punctures, might be very serious.

I have usually employed an isotonic 2 per cent. solution of novocaine. I am fairly satisfied with this drug, and I obtain a satisfactory anæsthesia in the vast majority of cases. Upon one or two occasions, I have, however, failed to obtain a satisfactory anæsthesia, or more rarely, no apparent anæsthesia.

In one case, a man of about 50 years of age, I injected upon five occasions, in none of which was the anæsthesia satisfactory. In three other cases I failed to obtain any anæsthesia. Each of these three patients were ex-service men who had suffered from shell-shock. Dr. Smith reports a few similar cases in which he has also been unable to obtain anæsthesia.

These failures to obtain a completely satisfactory anæsthesia are rare. I am, however, convinced that they do occur, and cannot be attributed to faulty technique in every case.

I have used a 1 per cent. non-isotonic solution of butyn in 200 cases. In one of these cases I failed completely to obtain anæsthesia, and in two other cases the anæsthesia was not satisfactory.

With regard therefore to the actual production of anæsthesia, I have found both novocaine and butyn satisfactory. The time to wait for anæsthesia varies according to the size of the nerve trunk and to the accuracy of the injection. For the larger trunks ten to fifteen minutes is usually necessary. In a few cases, however, anæsthesia has not come on at the end of half an hour, but will do so if further time—another ten minutes or so—be allowed.

I have for some time past been taking careful notes of my cases with regard to after-pain. I have given marks 1, 2 and 3 respectively, according to whether the patient has had slight, moderate or severe pain following the injection. I find that the after-pain is much reduced if one prepares the Ringer solution fresh each morning as follows:—

I distil two ounces of water daily, into which I place a modified Ringer tablet, made up of—

Calcii chloride	..	...	...	0·011 grm.
Potassii ,,	...	...	...	0·011 ,,
Sodii ,,	...	...	...	0·51 ,,

The required amount of this fluid is raised to boiling point and the novocaine tablets put in, so as to make 2 per cent. solution, and the whole is boiled again until the tablets are dissolved.

I find that it is advantageous to use not more than $\frac{1}{600}$ grain of adrenalin for each injection or series of injections.

I have prepared a table comparing the amount of after-pain with a 1 per cent. non-isotonic solution of butyn, a 2 per cent. solution of novocaine, using (1) a stock Ringer solution and (2) a freshly prepared solution as described above.

		Butyn	Novocaine (stock Ringer)	Novocaine freshly prepared distilled water	Nitrous oxide gas
Group I.	Upper molars	21·3	15·1	7·4	9·1
,, II.	Upper premolars	18·6	7·5	4·1	7·9
,, III.	Upper canines	13·3	18·1	13·3	5·1
,, IV.	Upper incisors	2·3	2·5	19·0	6·6
,, V.	Lower molars	38·0	30·1	5·5	20·1
,, VI.	Lower premolars and canines	10·2	12·6	7·6	13·0
,, VII.	Lower incisors and canines	0·0	0·0	0·0	7·5
,, VIII.	Upper teeth	16·6	12·4	8·6	8·1
,, IX.	Lower teeth	20·5	25·2	6·4	14·6
,, X.	Upper and lower teeth [1]	—	—	—	15·2

[1] The operation performed at the same time.

As a standard of comparison, since some at least of the after-pain must be due to the trauma of the operation and not to the injection, I have compared these results with the after-pain following nitrous oxide gas anæsthesia. It will be seen that butyn gives a considerable amount of after-pain. It is difficult to say why this is. It may be due to the fact that butyn is a sulphate and is decomposed by chlorides. It is possible that the sulphate is more irritating to the tissues.

I was surprised to find that a freshly prepared solution of novocaine gave less after-pain than nitrous oxide gas. Careful inquiry disclosed the reason for this, namely, that in many of the slight cases pain was experienced during the hour or so immediately following the operation, a period when the local anæsthetic is still potent.

Dr. W. Hill

said he was in very substantial agreement with Dr. Watson-Williams and Sir William Milligan with regard to the fact that cocaine, in competent hands, was not a dangerous drug. One should always test the patient's nose, at all events, by using a $2\frac{1}{2}$ per cent. solution of cocaine, and if in two or three minutes that did not cause palpitation of the heart, one could safely go ahead. If palpitation did result, a general anæsthetic or one of the cocaine substitutes should be used. A month ago there appeared in the *British Medical Journal* an article by himself on the latest of these substitutes, namely butyn. At that date he said that in America, where the drug had been used many thousands of times for surface anæsthesia, there had been no reports of any serious symptoms following its use. But he had not been so astute as Mr. Eric Watson-Williams, who had written to the proprietors for the information he required (see pp. 32, 33). Since then events had been moving very rapidly indeed in the matter, and reports were coming in stating that this drug, which was declared to be so very safe, was by no means safe. On the ninety-two occasions he

had used it he employed it in only 5 per cent. solution, and on no occasion did he get palpitation or any poisoning effect. Still, butyn he "damned with faint praise" because he found it an inconvenient drug to use, for two reasons: one was that mentioned by Sir William Milligan, that it was not ischæmic—a very important matter in nasal surgery; and the addition of adrenalin caused irritation, which was manifested in coryza, sometimes continuing for days; the second reason was that it was uncertain in its action—sometimes it failed to anæsthetize. He was not put off it because in exploratory puncture of the antrum there was some pain felt, because he thought there would be some pain whatever anæsthetic was used. Even in the case of cocaine he was told that there was pain on applying the galvano-cautery to the septum in rhinitis, and he thought the drug did not come up to what had been expected of it. He did not often get cases of cocaine poisoning, but when he did it consisted of a little palpitation or faintness, and most of these cases occurred in the old days when 20 per cent. of the drug was frequently used. In his own hands butyn was not a drug which caused toxic symptoms, and it was not until the seventeenth application that he discovered that it was uncertain in its action.

There were two other drugs which could be used for surface application in the nose and throat, namely, first, eucaine lactate. That had been known for years, but had not "caught on," because it was disappointing in its action. And he had been asked by the Committee of the Ministry of Health to try stovaine. This differed from novocaine and eucaine in being soluble in a 10 per cent. solution. Only that morning he applied stovaine of 10 per cent. strength in the case of a patient with nasal polypus, and when in a few minutes she came back for the operation she was panting, and said: "I thought you promised you would not use cocaine again; it made me faint before." She added that she had palpitation of the heart. Therefore, evidently this drug was not free from danger either. But he was still proceeding with it, with considerable caution. He admitted that he had used it in 10 per cent. strength in a patient who was susceptible to 2½ per cent. cocaine, and he would go on with the weaker solutions of stovaine to see whether the drug was good. But that also was non-ischæmic. He used butyn only for surface anæsthesia, believing that novocaine was sufficiently good and non-toxic for infiltration anæsthesia.

Dr. Albert A. Gray

said that his remarks would be limited to discussion of the effects of some of these local anæsthetics on the tympanic membrane.

When he was investigating the effect of solutions of aniline, he also tried eucaine, and found it very inferior to cocaine when used in combination with aniline for producing anæsthesia of the tympanic membrane. Since then he had tried stovaine and novocaine, and they, also, he had found much inferior to cocaine. But aniline itself was a substance which had the defects of its virtues, as it so easily penetrated the epidermis. At times some of his patients had blue lips after the application, though there were no symptoms of collapse; and if one wiped out the aniline afterwards there was no trouble. He had not found any of the cocaine substitutes useful in that region.

He had never had a patient who became subject to the cocaine "habit" as the result of its use medically. For the nose he now used an ointment containing cocaine—not watery solutions of the drug—and he combined a little

suprarenal extract with it. By this method the cocaine was very slowly absorbed. Evil results from cocaine turned largely on the rate of absorption of the drug; it was not only a question of the dose. He found cocaine used in this way very satisfactory for dealing with turbinate bodies, and even for septum operations; it penetrated very well, and remained in the situations a considerable time.

Mr. H. J. Banks-Davis (President)

recalled a case in his earlier days in which a man—the head draughtsman in an architect's office—became addicted to cocaine as a result of this drug being used to spray his larynx before any sight of it could be obtained. Fortunately for him a street accident confined him to bed and made it impossible for him to continue his visits, and he later confessed that he had been in the habit of making the most of his laryngeal condition because he "wanted the spray," and he was able in the exaltation which ensued to return to his office and make the "most splendid drawings." The accident probably, therefore, saved him from becoming a victim to the cocaine habit.

Mr. Musgrave Woodman

said that in 1917 he had a patient who came two or three times to have some minor polypi removed from the left side of his nose. On the first occasion he (the speaker) used solid cocaine rubbed in with wool. On the next visit the patient brought a little bottle, stating he was a manufacturer of novocaine, and he presented Mr. Woodman with it for use. On the next visit he did use it, but when coming next time he said he did not like it so well as the cocaine, adding "I will go back to the old stuff."

Mr. T. B. Layton

said that at the Ministry of Health the points in connexion with these local anæsthetics—apart from the anæsthesia—struck people according to their particular work. To the nose and throat surgeon the chief matter was the shrinkage value of it. He did not think enough had been heard in this debate from the ophthalmologists, but in their view the chief effect seemed to be the irritative one, which was not so important in throat work. He agreed with Dr. Watson-Williams and Sir William Milligan that the value of these substitutes turned largely on their suitability for surface application.

What had been said this evening by Mr. Eric Watson-Williams had made him a little anxious as to butyn. He had learned from the pamphlet supplied that butyn was twice as toxic as cocaine, therefore he had his reduced to half-strength as compared with the cocaine and novocaine he was using. He had been daily making 5 per cent. applications to the nose, without any ill effects. He started with injecting very small quantities of 0·5 per cent. solution, and gradually felt his way, increasing the amount until he had injected as much as half an ounce of 0·5 per cent. butyn. He had injected by infiltration for operations both upon the septum nasi and upon the tonsils, and by block anæsthesia by putting it into the spheno-maxillary fossa. He had not seen untoward results. Still, after Mr. Eric Watson-Williams' remarks, he would use more caution.

He was unable to distinguish clinically between the toxic symptoms of cocaine and those of its substitutes, and of those seen in mental states engend-

ered by fear and apprehension. He suspected this might explain some of the things of which members had spoken during this discussion. His seniors said these things used to occur, but they had not seen them for years. That might be explained by the fact that they had now learned their technique, and so they felt confident. Certainly the surgeon's confidence, as also his lack of it, communicated itself to the patient. He would not go so far as to say that fear could be assigned as the cause of these deaths from butyn which had occurred in America; but the question must always be taken into consideration.

It was noticeable that apparently only certain dental surgeons took the attitude that cocaine was absolutely essential for their work, and he had been glad to hear Mr. Doubleday's views. He knew one rhinologist who held the same view, namely, Dr. Graham Brown, of Melbourne, who had been in London the previous summer, and had taught him how to induce block anæsthesia. That expert would not use novocaine. He was much surprised at the amount of cocaine Dr. Brown used, of 1 per cent. strength, for a double maxillary sinus operation, the patient being a girl aged 18. But she was not in any way distressed. Dr. Brown was not only convinced the local anæsthetic must be cocaine, but that it must be Waite's anæsthesia. He (the speaker) felt sure it was the mental attitude of the dental surgeon which was the important factor in these cases. Dr. Graham Brown was careful to use the dentists' brand of cocaine for his work.

In the case of the nose, local anæsthesia was closely associated with hæmostasis, and this was not produced by butyn. He had been trying to find out how much adrenalin must be added to butyn in order to secure the same hæmostasis as in the drugs to which one was accustomed. He used 1 per cent. novocaine with adrenalin for injecting tonsils. He had put that in one tonsil of a patient, and in the other side 0·5 per cent. butyn, with four times as much adrenalin, and the hæmostasis on the two sides could not be compared one with another for efficiency, that caused by novocaine being the better. One would have to go on adding adrenalin until the amount used was quite considerable. The question of reactionary hæmorrhage ought also to be considered, as he considered there was somewhat more danger of that from local than from general anæsthesia so far as the tonsil was concerned. That was especially so if one had to continue adding adrenalin.

Mr. Mark Hovell

said that for some time past he had given up using cocaine for general nasal work. In cases requiring local anæsthesia he had used eucaine, 5 per cent. to 10 per cent. solutions. For the purpose of clearing a nostril of polypi he used 2 per cent. novocaine.

Dr. J. B. Horgan (Cork)

said he had used alypin. Some years ago he tried a 10 per cent. solution of it for surface anæsthesia in the nose. After using it twenty times, he had concluded it was far more toxic than was the same strength of cocaine, therefore he had discontinued its use. He did not know whether he had obtained an indifferent supply.

A point of interest was the method of counteracting cocaine poisoning. He had been in the habit of giving his patients a hypodermic injection of hyoscine—the compound A of Burroughs Wellcome—twenty minutes before

the operation when he was intending to use a reasonable amount of 10 per cent. solution of cocaine. He had discovered that to be a very safe antidote when there were symptoms of incipient cocaine poisoning. It had also been found that a 10 per cent. solution of calcium chloride, injected intravenously, was a very rapid and efficient antidote to the severe types of cocaine poisoning.

Dr. A. S. Green (Lincoln)

related a personal experience of both cocaine and butyn for nose treatment. His left antrum was opened, under cocaine anæsthesia. The anæsthesia was efficient, but he felt very ill after it. A year later he had a fungoid growth in the throat and base of the tongue. Again he underwent cocaine anæsthesia, and again he felt ill after it. The surgeon then asked him if at the next sitting he would like butyn to be used. The next sitting was for cauterizing the base of the tongue, and butyn was used. On the throat itself the anæsthesia was perfect, but it was inferior on the tongue base. He then had to have the antrum reopened with a rose-headed burr, and the butyn anæsthesia was perfect. The nose felt as if it was frozen, and he did not feel the bone being broken down at all. The time he received butyn he could enter his car straight away and drive thirty-five miles without discomfort.

Mr. Sydney Scott

said he had used an isotonic 5 per cent. solution of butyn as a nasal spray, in the same way as cocaine would be used, chiefly for Eustachian catheterization, and when he read Dr. William Hill's excellent paper, he felt that his experiences coincided with Dr. Hill's. He had found butyn useful in some cases in which cocaine had produced disagreeable symptoms, but he did not anticipate it could displace cocaine. Personally, he had never seen or heard of a case of addiction to the cocaine habit after its use.

He had found pure carbolic applied carefully to the tympanic membrane enabled patients to tolerate an incision of the tympanic membrane.

The teaching at St. Bartholomew's Hospital twenty years ago was that it was very dangerous to spray the nose with cocaine. Looking back, he could see that ill results were due to the fact that with the particular spray then used, one squeeze of the bulb could inject nearly a drachm of the solution, and the patients often swallowed some. He realized the danger of swallowing cocaine, and always made patients incline the face downwards to prevent it. It was very rare ever to see ill effects nowadays.

He had had one experience of momentary alarm from novocaine injected with adrenalin hypodermically, for treatment of malignant disease of the ear by diathermy. The patient suddenly held his breath, looked very ill, and had great pain in his chest. He said he had cramp in his back and in his leg. These cramps came on during the injection. At the time the disturbances were attributed to the adrenalin.

Mr. Herbert Tilley

said he had had a fairly large experience with novocaine. He had used it frequently for resection of the nasal septum and for intranasal operations on the antrum. It had proved very efficient, and he had not seen it give rise to any symptoms of poisoning, Fifteen times he had used it for urgent

tracheotomy, where a general anæsthetic would probably have precipitated disaster. In these cases there was extreme stridor, but he was able to open the trachea without pain. He had been struck by the large quantity of novocaine which could be used without causing any symptoms of distress; he had used ½ oz. of a ½ per cent. to 2 per cent. solution for various nasal and throat operations.

He agreed with those who felt that the profession had not yet a safe local anæsthetic which produced the combined qualities of anæsthesia with contraction of tissues. And, however safe and perfect as an anæsthetic any drug might be, if it did not possess ischæmic properties he did not see how it could take the place of cocaine in medical practice.

He agreed with Mr. Layton's remarks as to the psychological factor in the production of the so-called toxic symptoms of cocaine poisoning, and in twenty-five years he had had no case in his consulting room which had given him any anxiety. The nervous or anxious patient might possibly feel faint even after an injection of normal saline. But he believed cocaine to be very toxic if used as an injection as compared with a surface application. He had often wondered whether the epithelium of the mucous membrane did not largely destroy the toxic properties of cocaine. It was common knowledge that a 20 per cent. solution could be applied to the nasal or throat mucosa without hesitation: but if a few drops were injected serious symptoms of poisoning might quickly result. He related a personal experience of local anæsthesia with novocaine for removing an acute abscess beneath a "corn," and emphasized the difference between the complete absence of pain during, and for five or six hours after the operation, and the agony endured when the effect of the drug had passed off.

Dr. P. Watson-Williams (in reply)

said he was anxious in introducing the subject to point out that he was mainly concerned in outlining points for discussion rather than in expressing his own views or in any way prejudicing freedom of discussion. Mr. Sydney Scott had emphasized a very important point in referring to the danger of swallowing cocaine.

In regard to Mr. Tilley's suggestion that the cocaine in its passage through the tissues became altered, Mr. Eric Watson-Williams had pointed out that in the tissues it was slowly changed into its base, ecgonin, which was harmless. His own experience with novocaine had always been happy when used for injection purposes, both by infiltration and for nerve blocking; he had never known bad results from its injection.

He thought the profession had got into the wrong mental attitude. In regard to cocaine, if a better substance for the purpose were forthcoming it would be welcomed, but, seeing how valuable a local anæsthetic it was, it ought not to be black-listed. To seek to taboo it only on account of social abuse before effective restrictions were imposed on its sale to the public, was a wrong attitude for the profession, or any scientific body, to take up, and to be consistent we must equally limit the use of morphine, or even alcohol.

Section of Laryngology.

President—Mr. H. J. Banks-Davis, M.B.

Extensive Bony Growth of the Nasal Septum in a Woman 46 years of age.

By E. A. Peters, F.R.C.S.

Mrs. S. has noticed increasing nasal obstruction for two years, and for one year a hard projection in the region of the nasal process of the left superior maxilla.

A large smooth enlargement of the septum nasi is seen involving both the cartilaginous and bony constituents of the septum. Under a local anæsthetic an incision was made into bone of varying hardness, with free bleeding.

There does not appear to be involvement of other bones, as in leontiasis.

Suggestions as to treatment are invited.

DISCUSSION.

Mr. H. J. Banks-Davis (President) said that the growth was very hard and localized, and he did not think it had any association with leontiasis.

Mr. E. D. D. Davis asked how Mr. Peters proposed to remove the tumour, which he (the speaker) thought was a chondroma of the septal cartilage. Access by lateral rhinotomy would be difficult, and a Rouge's operation seemed to be more suitable. He (Mr. Davis) asked for opinions as to the operation which would give the best access to the growth. He had seen a recurrence of two enchondromata of the larynx in which the tumour had been scooped out of its capsule. If the capsule was not removed with the growth, a recurrence was probable.

Dr. W. Hill asked whether these neoplasms continued to grow. If not, one might bore a hole with a Curtis's nasal trephine, and, having thus given the patient space for breathing, leave the tumour alone. If growth continued, some more radical treatment was indicated.

Dr. Jobson Horne said that more information about the ætiology would be welcome. Had an operation been performed on the nose of this patient at any time? As the part involved appeared to be entirely the septum, the case was not in the same category as similar cases which had been brought before the Section in which the origin was the outer wall of the nose.

Mr. N. S. Carruthers said that he had recently seen a similar case. There had not been an X-ray examination previously, but he (the speaker) had attempted removal. The more deeply he had gone, the more bone he had encountered, therefore he had

desisted from completing the operation. The patient was a man, aged 53, who had been complaining of nasal obstruction for many years. He (the speaker) had given the patient an air-way through one nostril. On subsequent X-ray examination, it was surprising to find how far the bone had extended. There had been leontiasis involving both orbits, but the eyes were not proptosed.

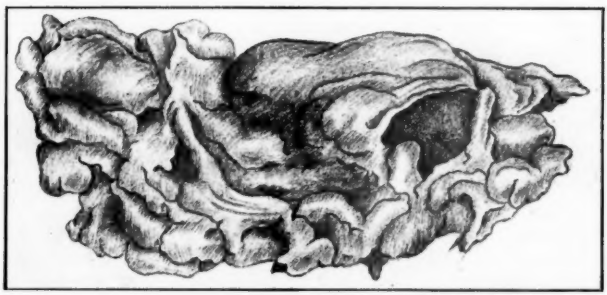
Dr. W. H. KELSON said that he had had a somewhat similar case, but in his case the growth affected the outer wall of the nasal cavity. He (Dr. Kelson) thought the disease was distinct from leontiasis. In his own case the chisel had struck hard bone, and then suddenly went through it into a mass of spongy bone. He understood that a recurrence was frequent, but in his own case the growth had not recurred.

Mr. PETERS (in reply) said that the Wassermann reaction in this case had been negative, and the skiagram had not shown any involvement of other bones of the face. He was hoping to cut away a piece for microscopical examination. He had made an incision with a scalpel, and the tissue seemed at first like cartilage. Next he had encountered soft bone, then bone of shell-like hardness. There was nothing to be seen posteriorly. The patient had had suppuration of the lachrymal sac two years ago, and now she had a small fistula there. During the three months in which she had been under observation there had apparently been no increase in size. He (Mr. Peters) hoped to be able to remove the growth and to exhibit the specimen. The nasal obstruction had come on so slowly that the patient had not complained of it.

Specimen of Extreme Metaplasia of Middle Turbinal.

By A. J. WRIGHT, F.R.C.S.

THE growth consisted of an omentum-like fringe hanging from the left middle turbinal. No other parts were apparently involved and the only symptom was unilateral nasal obstruction. The growth was removed on



Extreme metaplasia of middle turbinal.

January 3, 1923, and up to the present there is no sign of recurrence. Opinions are invited as to its nature.

DISCUSSION.

Dr. W. HILL said the growth seemed to be a villous condition, similar to that seen in connexion with an ovarian tumour.

Mr. MUSGRAVE WOODMAN said that macroscopically the striking thing was that Mr. Wright had removed the entire turbinal, with its villous terminations. Microscopically, there was an intact, thickened, ciliated mucous membrane over the surface and in the centre projections of solid acini. These were distinctly vacuolated cells, and those cells were the abnormal constituents of the section. Last year, when he (the speaker) had been specially interested in the subject, Professor Shattock had given him much help in the microscopical examination of these ethmoidal tumours, pointing out that the chief characteristic of the growths was the presence of these vacuolated cells, which he (Professor Shattock) associated with endothelioma, and had named "endothelial sarcoma." He had also pointed out that, clinically and pathologically, they ran a very variable course, some being fairly innocent, others moderately malignant. If removed in the early stage, apparently they did not recur, but in later stages they seemed to break out through the lining mucous membrane and invade bone. He (the speaker) had shown a similar type of specimen in which the growth went through the bone. He suggested that the present case was one of early endothelioma which had not yet broken through the mucous membrane.

Dr. P. WATSON-WILLIAMS said that he thought this specimen should go to the Pathological Sub-committee, so that a full investigation might be made of it for future guidance.

Mr. WRIGHT (in reply) said that he had never before seen anything like this specimen, and if anything could be ascertained concerning the pathology he would be glad. The pathologist whom he had consulted had said that the tumour was undoubtedly malignant, probably epithelial. The nose had been filled up with the growth, and he (the speaker) had decided to put the patient under an anæsthetic and make an investigation in order to see what structures were involved. He had found the growth all hanging as one mass, and when he had seized it with forceps it had come away, and was found to represent the middle turbinal transformed. He did not know whether to clear out the whole ethmoid, and finally he did not do anything. Now, a year later, there was no sign of recurrence.

It was important to learn, if possible, the nature of the growth, and to find the appropriate treatment for the various growths of the nose so that the operative measures might be suited to the various degrees of malignancy.

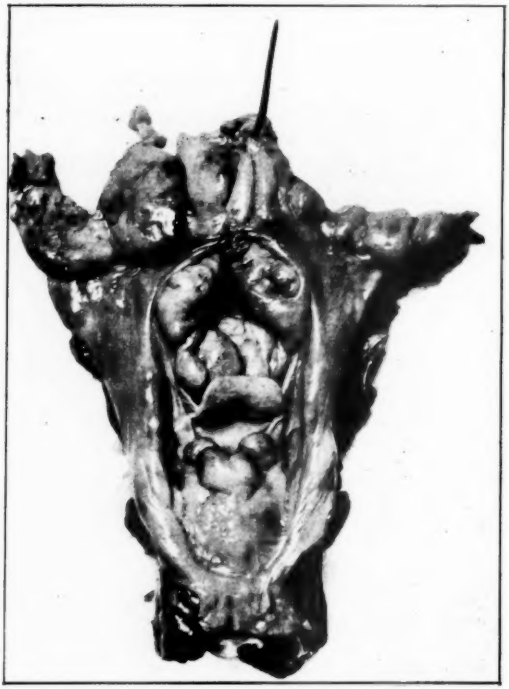
Postscript.—PATHOLOGICAL REPORT BY DR. GEOFFREY HADFIELD: "The growth consists of masses of actively growing squamous epithelium, showing hyperkeratosis, moderate keratinization of the surface layer, and well developed differentiation in the deeper cells. No histological infiltration of deep tissues made out. There is no doubt that if the lesion is not now malignant, it has all the characters of a pre-cancerous condition. From its histology it seems to me to be probably an example of metaplasia following a chronic inflammatory lesion and possessed of high potential malignancy."

Dr. G. W. NICHOLSON, has also seen this microscopical section and reports as follows: "The section of ethmoidal mucous membrane presents a villous appearance caused by chronic inflammatory changes. The epithelium is squamous with large papillæ extending into the stroma. This change is the result of a process of metaplasia common in the nose and its sinuses. There are no signs of malignancy to be seen."

Specimen, Section and Drawing of a Case of Mycosis Fungoides involving Pharynx and Larynx.

By A. J. Wright, F.R.C.S.

THE patient from whom this specimen was taken was shown by Dr. Kenneth Wills before the Section of Dermatology on October 18, 1923. Details of his early history can be found in the *Proceedings*.[1] He was a man, aged 34, and had first shown signs of the disease in September, 1920. He first came under my observation on October 22, 1923, with huskiness of the voice. At that time small tumours were present in the false vocal cords, but the pharynx was clear. Two



Mycosis fungoides of pharynx.

weeks later the nasopharynx was entirely blocked, the soft palate being pushed forward, and dyspnœa and stridor were present. Death took place from exhaustion and asphyxia on December 13, 1923. In addition to general enlargement of the lymphatic glands and multiple tumours in the skin, pharynx and larynx, the mucosa of the bronchi was involved, causing obstruction.

[1] *Proceedings*, 1923, xvii (Sect. Derm.), p. 3.

Mr. WRIGHT said that tracheotomy had not been performed, though its advisability had been discussed. The palate had been in contact with the back of the tongue, and the patient had been unable to swallow. There had also been signs of respiratory involvement lower down. Extensive deposits had been found in the hilus of the lungs, compressing the bronchi. Tracheotomy would probably not have prolonged the patient's life for more than a day or two.

Foreign Body removed from Right Bronchus.

By C. F. BEEVOR, M.B.

PATIENT, a child, aged 7 years, playing with a toy trumpet on Christmas Day, 1923, was thought to have swallowed the "squeaker." Parents became alarmed by persistent cough. Patient was eventually X-rayed at Addenbrooke's and sent up to University College Hospital. The skiagram shown was taken and the foreign body removed by Brünings's tube and forceps. This was done on Sunday, December 30, five days after the accident. Air entry to the right lung was very feeble and there were a few rhonchi. The child was perfectly well again in a few days' time and returned to her home in Cambridge.

DISCUSSION.

Dr. W. HILL said that he thought the result was a very good one, but he considered that the foreign body had been in the right *secondary* bronchus, and that the title of the case should indicate that fact.

Mr. NORMAN PATTERSON said that, years ago, he had had a similar case at Golden Square. Bronchoscopy had been performed in the dark room, and the foreign body had been removed. During the whole procedure the noise from the squeaker had continued. [Mr. Patterson described, by means of a drawing, how, when the foreign body slipped several times, he had attacked it transversely, and had eventually removed it in that way.]

Mr. H. J. BANKS-DAVIS (President) (after describing a similar case) said that the presence of unilateral bronchitis or bronchial catarrh in a child should lead one to suspect and examine for a foreign body.

Dr. H. SMURTHWAITE said that he had shown a similar specimen taken from a child's left bronchus. There had been no symptoms until five weeks after the swallowing of the foreign body, when a whistling noise developed on deep breathing. He (the speaker) had removed it with a specially made instrument through a bronchoscopic tube.

Mr. E. D. D. DAVIS referred to an identical case in which the X-ray screen did not reveal the foreign body. He (Mr. Davis) had removed it with Brünings's fenestrated bean forceps, which had gripped it very well.

Dr. IRWIN MOORE said he agreed with Dr. Hill that this foreign body had probably been in the secondary bronchus; it appeared from the radiogram to have been too low down to be in the main bronchus. He congratulated Mr. Beevor on the result.

Mr. A. J. HUTCHISON asked what happened if one failed to remove the foreign body. He (the speaker) had had a case fourteen years ago in which the child had been turned upside down and shaken, &c., and had then suddenly become cyanosed. Tracheotomy had been performed, and a groping effort made to remove the foreign body. By the time his (Mr. Hutchison's) aid had been invoked, the body—a metal pencil protector—was in the second division of the right bronchus. He could see it, but could not remove it, therefore he had been obliged to leave it where it was. Last summer, fourteen years after the accident, the foreign body was still in the same place, and seemed to have done no harm.

Mr. H. J. BANKS-DAVIS (President) said that Chevalier Jackson had stated that there were a number of children who had screws and other foreign bodies in their bronchi, and who did not seem to be harmed by them.

Mr. M. VLASTO reminded the Section of a case which he had shown three years previously, of a child who had swallowed a paper fastener, which he (Mr. Vlasto) had failed to extract from the œsophagus.[1] The opinion of the Section had been divided at the time as to whether the foreign body lay in the mediastinum or in the bronchial tree. He (the speaker) had seen the child two days ago, and the foreign body was still in exactly the same place. The child was perfectly well.

Mr. BEEVOR (in reply) said that he had used a Brünings's forceps with a central wire. When he had reached the bifurcation, he could see the foreign body ¾ in. down. The mucous membrane had been rather swollen. The foreign body might have shifted while the anæsthetic was being administered.

Dilatation of the Lower End of the Œsophagus, Secondary to Hiatal Œsophagismus, Cured by the Mercury Bougie.

By T. B. JOBSON, M.D., and E. W. SHEAF, M.B.

B. W., MALE, aged 20, agricultural labourer, admitted to the Royal Surrey County Hospital, on August 23, 1923. History of three years' difficulty of swallowing—"food seemed to stick half way down—the next meal pushed some of the previous meal down or was vomited."

No particular pain. Some loss of flesh, but not emaciated. No history of swallowing corrosive fluid.

X-rays showed retention of barium meal at lower end of œsophagus. Treatment by diet, bromide and atropine and rectal feeding produced no improvement.

Œsophagoscopy showed a dilatation at lower end of œsophagus about the size of an orange, lined with dirty white mucosa. At the lower end the contracted hiatal opening could be seen and a small bougie passed. Subsequently the hiatus was dilated up to the largest size Jackson bougie. As the amount of dilatation obtainable through the œsophagoscope is necessarily limited, we tried the mercury bougie at Mr. Mollison's suggestion. This was passed with the aid of the fluorescent screen with the patient in the upright position. It produced an immediate improvement.

In a few days the patient learned to pass it himself before each meal.

He rapidly gained flesh, had no more vomiting and left the hospital on October 9, 1923.

DISCUSSION.

Dr. W. HILL said he understood, from a conversation with Mr. Sheaf, that there had been a stricture at the level of the hiatus, and that at first this had only admitted a 4-mm. bougie, hence it was obviously an organic stricture. Dilatation had ensued from passing other bougies, so that eventually it had admitted the largest size Jackson bougie. This, however, had had to pass through a Jackson tube. The authors had succeeded in dilating an ordinary hypertrophic stenotic stricture of the pharyngo-cardiac portion of the gullet, and had then continued the process with a mercury-filled tube. Hiatal œsophagismus suggested spasm, and if there were spasm it would be of the fibrous diaphragm. He thought the stricture had been a hypertrophic stenosis which had been cured by dilatation.

[1] *Proceedings*, 1921-22, xv (Sect. Laryng.), Suppl. pp. i, ii.

Dr. BROWN KELLY remarked that this patient stated that he passed a bougie three times daily, and had been doing so for months. He (the speaker) suggested that more forcible dilatation be used, by means of a hydrostatic apparatus, or by an instrument working on the principle of the glove-stretcher. He had treated several similar cases with Gottstein's balloon, which was distended by pumping in water, and, so far, the benefit had been permanent in some, and in others had lasted for months. Recurrence should not constitute a discouragement, as the affection was a functional one. He (Dr. Brown Kelly) considered that the title adopted by the exhibitors described the case correctly. When there was doubt as to the presence of œsophagismus the question could be settled by observing the effect of gently touching the suprahiatal region with the end of the œsophagoscope. If firm closure at the hiatus followed, a heightened tendency to muscular response was indicated.

Dr. D. R. PATERSON (Cardiff) said he agreed with Dr. Brown Kelly as to the nature of this condition.

Dr. JOBSON (in reply) said that at that stage he did not express an opinion as to the pathology. Through the œsophagoscope he could see the hiatal fibres guarding the opening. He had been able to pass a small 4-mm. bougie through the œsophagoscope and the bougie had been gripped. Later he had dilated to 7 mm. Mr. Mollison had suggested the use of the mercury bougie, and it had acted very well. After the patient had used it for two days, the vomiting had ceased; the patient had since gained 2 st. in weight, and had resumed his work. After another month he would be advised to discontinue the use of the bougie. If symptoms then recurred, he (Dr. Jobson) would employ other mechanical means. The first attempt to pass the mercury bougie had been made with the patient standing in front of the screen, and the bougie had been seen to curl up at the orifice.

Slide showing Section of a Gland removed from a man with Chronic Laryngitis and an Enlarged Gland in the Neck (previously shown at the November meeting, 1923).

By W. H. KELSON, M.D.

THE section shows thickening of the walls of the blood-vessels and general fibrosis (? syphilitic) but no sign of tubercle or new growth.

The patient was a man, aged 77, who came complaining of hoarseness of fourteen months', and a swelling in the neck on the left side, of four months' duration. On examination the left vocal cord was seen to be pink in colour and a little swollen and sluggish in movement. In the anterior triangle of the neck on the left side was a hard but freely movable enlarged gland, about the size of a walnut. No lesion could be found in the nose, pharynx or post-nasal space.

DISCUSSION.

Mr. G. W. DAWSON said that he remembered the case; there had heen a hard, somewhat isolated gland, low down on the left side of the neck. Some Members had considered that there was evidence of malignancy, but most of those who had seen the case did not think that it was of a malignant nature. It was satisfactory to find that the swelling had only been inflammatory.

Dr. JOBSON HORNE said that when the patient had attended the meeting in November, 1923, one opinion had been expressed that the enlarged gland was independent of the chronic laryngitis. He (the speaker) inquired about the present condition of the laryngitis.

Dr. KELSON (in reply) said that since the date of the report he had not seen the patient, who had been so satisfied to learn that there was no malignancy about the gland, that he had departed at once.

Silver Tracheotomy Tube, shown to illustrate the average life of such an Instrument.

By Sir StClair Thomson, M.D.

THIS has been worn continuously, in a case of lupous stenosis of the larynx, for three years.

The patient could not be persuaded to submit to a tracheotomy until her stenosis was so extreme that she could hardly walk up one flight of stairs.

Tracheotomy was performed under local anæsthesia as low as was possible. As a result the lupus in the larynx completely healed, a good voice returned, but the stenosis, which still remained on cicatrization, prevented the patient from abandoning the tube which she has now worn for six years. During that time she has married and has had a healthy child. She indulges in dancing, skating, tennis and other sports. The tracheotomy tube, which is shown, has only been removed four times a year for cleansing purposes. It will be noticed that, at the end of three years, the silver is worn so thin that it is beginning to crack. In my experience this is about the average life of a silver tracheotomy tube.

Mr. C. Parker said that the effective life of a tracheotomy tube varied enormously. In some instances they had to be renewed in considerably less than three years, whilst on the other hand he (the speaker) had known them last for even longer periods. Much seemed to depend on the nature of the secretions bathing them in each particular case.

Case of Extensive Thickening beginning in the Ventricular Bands, and covering both Cords.

By A. L. McLeod, M.B.

Mr. L., aged 52. Shown to the Section, February, 1923. Extensive thickening beginning in the ventricular bands and covering both cords. Opinions of Members divided as to presence of tubercle, syphilis, or simple inflammatory thickening. Wassermann negative.

Seen on June 4: Right cord fixed; more extensive thickening.

Seen again November 30: Complained of marked dyspnœa. Larynx almost filled. Right cord fixed. Tracheotomy performed next day and piece of ventricular band removed for examination.

DISCUSSION.

Mr. Tilley said he regarded the condition as epithelioma. He did not think malignant disease could be excluded because ulceration had existed a year. He remembered a case which had been watched by Sir Henry Butlin, Sir Felix Semon and Sir Charters Symonds for six years, after which the patient had come to him (the speaker). He had operated and found malignant disease of the vocal cord. If the case now shown was not one of malignant disease, his (the speaker's) alternative view was that the condition was syphilitic perichondritis.

Dr. Jobson Horne said he could not see sufficient clinical evidence of malignant disease. The case, he thought, turned on the disputed question whether a primary perichondritis could occur in the arytenoid region. He hoped a further report would be forthcoming.

Dr. SYME said that his first impression of the case was that it was syphilitic. He had seen it a year previously and had then thought it was lymphangioma; he still held that opinion. He had seen such conditions involving the arytenoid region and the ventricular bands.

Angiomata of Palate.

By F. COURTENAY MASON, M.S.

PATIENT, female, married, aged 49. Swelling of palate existed in childhood untreated. When aged 28, she sought advice at the Throat Hospital, Golden Square, but no treatment was given. The swelling has caused no trouble until the last year, when the tumour of the soft palate produced dysphagia and that of the hard palate displacement of an artificial denture.

There is a cavernous angioma the size of a cherry involving the uvula and adjacent soft palate. A second growth, of similar nature but larger, is situated on the left side of the hard palate and alveolus, replacing the bone and the floor of the antrum. The patient has reached the climacteric.

DISCUSSION.

Mr. COURTENAY MASON said that he had brought the case because he hoped that some diathermy experts would express an opinion as to the value of that procedure. He had not as yet employed it in a case of angioma, and he wondered as to the risk of hæmorrhage. The anterior growth had caused considerable bony erosion and might have gone up into the antrum, and if so, it would involve more radical treatment than would a superficial growth. As the patient was a woman of fifty and the growths had only caused distress in recent years, the case was an interesting corollary to that of a child shown by Dr. Lowry at the December meeting.[1] He (the speaker) had proposed to use the galvano-cautery, but he was not certain that it would be suitable for the deeper growth in the hard palate.

Mr. H. J. BANKS-DAVIS (President) suggested that electrolysis would be the safest treatment; he did not think it would cause hæmorrhage.

Dr. IRWIN MOORE suggested that the case should be treated with radium by means of a special applicator placed in the mouth. Successful cases had been recorded by Rogers[2] in Toronto; also by New and Clark[3] (Rochester, Minnesota).

Dr. ELEANOR LOWRY said she had sent her patient, a child, away for six months to the country for treatment of his anæmia. She might possibly apply the galvano-cautery to the edges of the nævus before operating upon the tonsils.

Mr. NORMAN PATTERSON said that the galvano-cautery was dangerous in these cases.

Mr. COURTENAY MASON (in reply) said that he would investigate the possibilities of radium as suggested by Dr. Irwin Moore, and would inform the Section of the result.

[1] *Proceedings*, 1923-24, xvii (Sect. Laryng.), p. 17.
[2] *Journ. Canadian Med. Assoc.*, Toronto, 1912, o.s., xli, n.s., ii, p. 111.
[3] *Ann. Otol., Rhinol. and Laryngol.*, 1919, xxviii, pp. 1025-1037.

Ulceration of Left Vocal Cord (?) Epithelioma.

By W. H. JEWELL, M.D.

PATIENT, W. D., a male, aged 60, has been hoarse for nine weeks after having a cold.

The anterior two-thirds of the left vocal cord is ulcerated—the cord is active but lags on phonation. Wassermann: negative.

DISCUSSION.

Mr. O'MALLEY, Mr. WRIGHT and Dr. SYME said they considered that the growth was epithelioma.

Mr. N. S. CARRUTHERS also thought that the growth was epithelioma and that it was subglottic, a position which would account for the recent history.

Mr. ARMOUR BROWN said that one point against the diagnosis of epithelioma was the short history of hoarseness—only nine weeks. In the case of such an extensive lesion, if malignant, one would expect a longer history.

Mr. JEWELL, in reply, said that he considered the growth was an epithelioma and extended to the subglottic area, and he intended to perform thyrotomy.

Postscript: (February 12, 1924). Thyrotomy has now been carried out and the vocal cord, ventricular band and adjoining mucous membrane and perichondrium removed, *en masse*, as far back as the arytenoid.

Patient was able to swallow liquids in the evening of the day of operation and semi-solids on the following day. Highest temperature 99°. Progress has been continuous and he is now convalescent.

Pathological Report.—"Section shows a malignant new growth, a squamous celled carcinoma."

Case of Ulcer of Nasal Septum.

By PHILIP FRANKLIN, F.R.C.S.

PATIENT, male, aged 50. Extensive ulceration of left septal mucosa. Edge raised and œdematous. History of occasional epistaxis during nine months. Wassermann: negative.

DISCUSSION.

Mr. A. J. M. WRIGHT suggested that a little portion should first be taken from the thickened edge for microscopical examination.

Dr. W. S. SYME said he thought that the diagnosis lay between chronic tubercle and malignant disease; on the whole, he thought the ulcer was malignant.

Subsequent Data: Microscopic Report.—No evidence of tubercle or new growth. The ulcer and underlying cartilage and bone were removed by a sub-mucous resection. On examination one month later the area of removal was entirely healed.

Section of Laryngology.

President—Mr. H. J. Banks-Davis, M.B., F.R.C.P.

Case for Discussion.

Shown by T. Jefferson Faulder, F.R.C.S.

H. W. D., ex-gunner, aged 33. Produces an unusual kind of stridor. His history is that he was exposed to gas (mustard) at Ypres, in September, 1917, was then sent home, and was in the Military Hospital at Croydon for two months. On discharge he was sent to a camp in England on light duty. He states that in the early part of the following year (1918), while on this light duty, he lost his voice, and began to experience his present difficulty of breathing. For want of a better word this has been called stridor. It is audible at a great distance, and sounds like the noise of a pressure-pump or safety-valve of an engine. Nose and throat show nothing abnormal. The epiglottis is thin, flaccid, and abnormal. There is some congestion of the larynx, but no paralysis has been seen. There is no sign of ulceration either past or present.

DISCUSSION.

Mr. Herbert Tilley said he was wondering if this case would prove to be similar to the one he had described in the *Journal of Laryngology*, for January, 1920, viz., an intratracheal tumour which was removed by per-oral tracheostomy. The man had suffered from mustard-gas poisoning with such immediate tracheal symptoms as coughing, spitting of blood, &c., and within a few weeks he commenced to have increasing difficulty in breathing. When examined by the speaker, the larynx seemed normal and the upper four rings of the trachea could be seen, but nothing beyond these. He then got the patient to lean over, while he (the examiner) knelt on the floor below him; a method which brought the lower end of the trachea into view. There was now seen a ball-like tumour rising and falling with expiration and inspiration. Its removal by the direct method was accompanied by free bleeding. The patient was seen last year and had never had any further trouble from the condition. Professor Shattock cut a section of the tumour and it was found to be a granuloma. It was considered that an ulcer had been caused by the mustard gas, and that the granulations covering it had become pedunculated. He got Mr. Banks-Davis' patient to assume the same leaning-over position but no definite lesion was apparent in the lower tracheal region. As during sleep the breathing was said to be free and noiseless, Mr. Tilley suggested that the man be given an anæsthetic, and direct per-oral examination be made to see whether there was a tumour lower down.

Mr. H. J. Banks-Davis (President) remarked that he noticed the epiglottis was very thin, and flapping up and down, also that the voice was much altered.

Sir JAMES DUNDAS-GRANT said he supposed it would be agreed that this was a functional condition. Failing a positive result of X-ray or direct examination, his impression would be that it was due to "shell-shock." Apparently during sleep the vocal cords moved normally. The noise was louder than any tracheal stridor he had ever heard.

Mr. HAROLD BARWELL said the stridor in this case was not due to a lesion in the larynx, because it was of an expiratory character. His impression was that it was functional, though he did not know what was the mechanism of its production. He asked whether Dr. Hill had knowledge of any case in which paralysis of muscle between the tracheal rings had caused stridor.

Dr. WILLIAM HILL (in answer to Mr. Barwell), said it might be a spasm. In many cases of asthma the sound came from and was produced in the trachea as well as in the bronchi, and that was, presumably, spasmodic. Some tumefaction might be left after the poison gas, and some consequent stenosis.

Mr. A. J. WRIGHT said he also regarded the case as a functional one. Recently he had seen a servant girl with a somewhat similar condition. She was propped up in bed, and the medical attendant and mistress were anxiously hoping that he (the speaker) would be in time to perform tracheotomy. The condition proved to be entirely functional, and cleared up under appropriate treatment. The present patient worked very hard to produce his sounds, and there was sufficient projection in an open glottis to produce the sound if a sufficient blast of air was sent through.

Dr. IRWIN MOORE said he thought that in the case of this patient there might be granulations or a papilloma occupying the frænum. He also thought the cause of the noise might be the flaccid condition of the epiglottis. Mr. Faulder said that the sound ceased if the epiglottis and tongue were held forward.

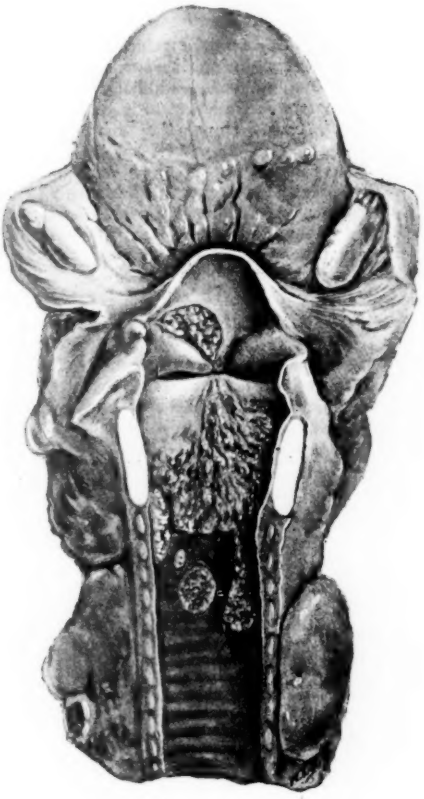
Mr. FAULDER (in reply) said that the man was much worse when he first saw him than now, and that when he entered into a conversation the noise ceased. When looking into the larynx with the mirror, one could see only the flaccid epiglottis with the tip apparently behind the arytenoids, and when the tongue was effectively drawn forwards, the noise ceased. The absence of scarring or evidence of stricture would exclude the possibility of it being due to stenosis, and he did not think the cause was paralysis. No one knew, in the sense of being an eye-witness, what the epiglottis could do, because the mere opening of the mouth presented a complication, and depressing the tongue or pulling it out was another. Still, it was certain the epiglottis could move through nearly two right angles. Twelve years ago he showed a case in which one could see the whole larynx and epiglottis without the aid of instruments, that of an old man who for nearly forty years had had rodent ulcer of the face, and it had destroyed both upper jaws, the whole of the hard and soft palate, all the nose, both sets of ethmoid cells, both internal pterygoid plates, one orbit and all its structures, and all the soft tissues of the parts he had named, hence larynx and epiglottis were visible without instruments. The epiglottis was normal; and he could make extraordinary movements with it, could put it in a direction downwards and backwards when he so desired. The present patient's epiglottis was not normal, but a flat flabby structure, and he (the speaker) thought the whole condition was due to the epiglottis; it seemed as if he had learned to dissociate one peristaltic act of swallowing from the rest. There was no sign of ulceration or scar tissue from the gassing, and this symptom did not appear until a good many months after his discharge from his first military hospital and when he was in a rest camp. He thought it was due to some suggestion or functional disorder. Those who were in France would have seen men exhibiting all the signs of gas poisoning when there was no gas at all, but only the ordinary odour. Also, they would have seen functional aphonia spread in an epidemic manner from one man to another. He thought this was an acrobatic trick acquired by a man in a rest camp, which was probably a prolific hunting-ground for suggestion.

Mr. E. D. D. DAVIS said it would do no harm to take the top off the epiglottis, as was sometimes done in the case of tubercle or malignant disease.

Case of Laryngeal Tuberculosis in a Child aged 4½ years.

By M. VLASTO, F.R.C.S.

THE total duration of the illness was three weeks. It started with an attack of croup and vomiting on December 14, 1923. The condition was diagnosed as diphtheria, and the child was admitted to the Eastern Fever Hospital on December 16. There was hoarseness, slight stridor with recession, but no distress. The fauces were red and swollen, but there was no dysphagia. No exudate. No other physical signs. Cultures for Klebs-Löffler bacillus all



negative. On December 29 and on January 7 there were attacks of acute dyspnœa, with stridor associated with mild pyrexia. The case was diagnosed as one of papilloma of the larynx, and the patient was transferred to the Queen's Hospital for Children. I saw the child on January 10. There was hoarseness and inspiratory stridor. I decided to inspect and treat the laryngeal condition by direct methods later on. The child died that night

of heart failure. The post-mortem showed extensive thoracic and abdominal tuberculosis with ulceration of the laryngeal and para-laryngeal structures.

The case is being reported, first on account of the rarity of tuberculous ulceration of the larynx in a child so young, secondly on account of its clinical interest in presenting a contrast between the course of this case and that usually obtaining in the adult.

The larynx and temperature chart are shown.

DISCUSSION.

Dr. JOBSON HORNE observed that the preserved larynx, through the round glass jar presented the appearance of superficial ulceration, and not that deep ulceration, tumefaction and destruction consequent upon the infiltration and eruption of tubercle, features so typical of laryngeal tuberculosis.

Many years ago, as the outcome of post-mortem research he (Dr. Jobson Horne) had been able to formulate the conclusions that when the disease presented those features in the larynx, cavitation had been established in the lung; further that in miliary tuberculosis of the lungs (the prevalent form in children and without cavitation), laryngeal tuberculosis did not occur. Hence the rarity of laryngeal tuberculosis in children.

However, in 1898, when the British Medical Association met at Edinburgh, he (Dr. Horne) demonstrated the larynx from a child aged 12 months who had died from miliary tuberculosis, and yet the larynx presented most typical features of laryngeal tuberculosis. The explanation was that a tuberculous bronchial gland had broken down into the adjacent lung and had established a cavity. Hence an exception that proved the rule he had deduced from post-mortem observations.

Mr. CYRIL HORSFORD said he had at present under his care a case which seemed, clinically, to be similar to this, namely, that of a child, aged 6. There was marked laryngeal stridor. He took the patient into hospital and, under chloroform, carried out direct inspection. There was no papilloma, but some thickening of both ventricular bands and inter-arytenoid spaces. He had not yet heard the result of the full examination, and before the report of Mr. Vlasto's case was published he would like to ascertain whether tuberculosis was present in his own case.

Mr. M. VLASTO (in reply) said that he would particularly like to emphasize one point of contrast between the clinical picture of this child and that in the adult—namely, the complete absence of dysphagia.

Case of Epithelioma of Vocal Cord, treated by Thyrotomy.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

FRANK S., aged 35, was first seen on account of hoarseness, on October 9, 1923. This hoarseness had been present for four or five years, and appeared to have commenced with attacks of bronchitis both during and since the war. He was referred to the Throat Department of Brompton Hospital by Dr. Burrell and it was then found that the whole length of the right vocal cord was occupied by a papillated swelling which extended well into the anterior commissure. There was diminished mobility of the cord. A diagnosis of epithelioma was made, and it was confirmed by microscopical examination of a portion which I removed by means of my intralaryngeal forceps.

On November 29, I performed laryngo-fissure, but in view of the fact that the growth extended into the commissure and probably across the middle line, I sawed through the thyroid cartilage, as recommended in such cases by Chevalier Jackson, avoiding any section of the soft parts until the alæ were drawn apart; the whole of the right cord and a portion of the left were then cleared out. I anticipated some difficulty with swallowing, but apparently the sphincter

laryngis acted efficiently and he was able to eat and drink next day. The voice is naturally extremely defective, but is gradually improving.

DISCUSSION.

Mr. HAROLD BARWELL said Mackenty, of New York, stated that infiltration into the anterior commissure was a contra-indication to removal of malignant growth by laryngo-fissure, but the present case was an example to the contrary, and did not support that dictum.

Dr. IRWIN MOORE said that Sir StClair Thomson had operated by laryngo-fissure on a large number of cases in which the anterior commissure was infiltrated and they were still alive. He asked whether Sir James anticipated some difficulty in swallowing in this case.

Dr. J. B. CAVENAGH asked how long an interval elapsed before Sir James removed the growth after taking away a piece for examination.

Sir JAMES DUNDAS-GRANT (in reply) said that in this case he carried out the method recommended by Dr. Chevalier Jackson, avoiding any section of soft parts until the alæ were divided and drawn apart. In cases in which the disease was confined to the middle of one vocal cord, it was best to open the larynx by means of a curved bistoury from within, so as to make sure of getting between the vocal cords from below. In this case the disease went across the middle line, and those who were at the Paris Congress would know Dr. Chevalier Jackson's counsel that it was advisable to saw through from the outside. He (Sir James) took out so much of the soft parts on both sides that he felt apprehensive as to the patient having some difficulty in swallowing liquids at least for a day or two.

The interval between taking away a portion for examination and the performance of the operation was longer than he would care to allow if it could be avoided; in this case it was probably a month. But he did not think this patient's interests suffered from the delay. The voice was chiefly produced by the ventricular bands, and it was as good as could reasonably be expected.

Case of Tracheal Stridor from Pressure.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

CHARLES C., aged 38, was first seen in October, 1923, complaining of dyspnœa when recumbent; this symptom was quite recent but he had had more or less huskiness of the voice since December, 1922. He had signs of tuberculosis in the chest and bacilli in the sputum. On laryngoscopic examination the vocal cords were seen to be infiltrated, but, below them, the view of the trachea was interfered with by the presence of an obstruction of a pale pink colour, the outlines of which were not perceptible, but it was probably a bulging of the anterior wall. To the left of the trachea, just below the level of the cricoid cartilage, there was a swelling perceptible on deep palpation. Direct laryngoscopy, so far as permitted by the state of the larynx, gave no further information; an X-ray examination was made; it suggested that the trachea was curved forwards by pressure from behind. The cervical vertebræ were carefully examined, but there was no evidence of caries such as might account for abscess formation. The patient subsequently reported that he could only lie on the left side and that when he bent his head over to the right the throat seemed to close. The swelling in the trachea gradually diminished to such an extent that it was possible to see past it and note that it projected chiefly from the anterior wall. On December 27 he coughed up a small crescentic piece of bone, the nature of which is not evident; he had felt something loose in the throat the night before. From that time the breathing improved and on January 1 it was obvious on laryngoscopic examination that the bulging from the anterior wall had considerably diminished and

allowed of fair breathing space. He has steadily improved as regards his stridor.

Dr. WILLIAM HILL said that the piece of bone coughed up might be the upper or lower border of the cricoid. Sir James Dundas-Grant had shown the patient here two or three months ago; he was the subject of tuberculosis.

Case of Extensive Lupus of the Upper Air Passages treated by Radium.

By H. M. WHARRY, F.R.C.S.

K. O., FEMALE, aged 28, attending Mount Vernon Chest Hospital.

History.—Tuberculosis of right apex, which has been quiescent for some years.

1918: Suffered from nasal obstruction.

1919: Her voice became hoarse and completely disappeared.

1920: Tuberculosis of larynx diagnosed. Wassermann reaction negative.

For four years she lived the life of a recluse for fear of infecting others, and also owing to her being unable to speak to anyone.

June, 1923: Seen by me; lupus diagnosed.

Condition when first seen.—General condition good. Tuberculosis of lungs quiescent. *Nose*: Cartilaginous part bulging as if from large polypi, but in this case bulging caused by extensive crusting within the vestibule. Posterior edges of vestibule and anterior ends of inferior turbinates were covered with large crusts, which also surrounded a perforation of the septum about the size of a threepenny-bit. The nose was almost completely obstructed by the crusts. On removal of a crust a swollen and bleeding surface was revealed. *Post-nasal space*: In right choana and resting against the septum was a rounded white tumour about the size of a hazel nut. *Pharynx and Fauces*: Uvula was rigid and much swollen, with granular surface. Granulations extended down posterior left faucial pillar and both right pillars and right tonsil bed. Pharynx was scarred from healed lupus. *Larynx*: Epiglottis scarred, deformed and apparently healed. Both ventricular bands and vocal cords were covered with fine granulations and there was swelling of interarytenoid region with granular surface. *Phonation*: Whole larynx stiff, and cords did not approximate. *Voice*: Nil; whisper very feeble.

November 5 to 9, 1923: Treated by Dr. Teichman at Radium Institute.

November 21, 1923: Crusts escaped from nose; airway established.

December 5 to 19, 1923: Voice returned; uvula and fauces improved.

January 14 to 18, 1924: Treated by Dr. Teichman with radium. Whole condition much improved. Tumour of right choana broke down and disappeared. Further improvement in state of larynx. Voice easier, but still a little husky.

DISCUSSION.

Dr. WILLIAM HILL said that it had not been usual to treat lupus in the air passages by radium on account of its very indifferent action, unless it was carried to the point of producing necrosis. In this present case it had been used practically as a cautery, and when pushed to that degree one never quite knew where the process would stop; the same applied sometimes to diathermy in the air passages. He had been chary of using diathermy in the larynx because of the cartilages. As, however, radium seemed to have had a good result in this case. he supposed such a view would have to be revised. If cauterizing agents were required, there were several which could be better controlled than radium, and cutting operations were very precise.

Sir JAMES DUNDAS-GRANT said he would like to hear details as to how the radium was used. Excellent results had been recorded from light treatment, uncovered by glass, such as was so much used at the London Hospital.

Mr. NORMAN PATTERSON said he hoped to show, at the next meeting, a case which had been treated by artificial light. The results in that case had been extraordinarily good.

Mr. HERBERT TILLEY said he would remind those Members who were present at the Paris Congress, in 1922, of the cases of lupus of the nose which they had seen treated by diathermy, and without, so far as could be judged, any results in the way of bone necrosis. Patients came to the out-patient department, cocaine was applied to the diseased areas, and the diathermy current passed through the metal seat of the patient's chair. The lupoid material in the nose was removed with very great efficiency, and there seemed to be no damage to neighbouring bones or cartilage.

Dr. OSKAR TEICHMAN said this case was treated in the routine way. During the two years he had been connected with the Radium Institute he had not seen a case of necrosis.

This patient was first treated in November, 1923, when she received an irradiation of the larynx externally for twenty-five hours, with the equivalent of 156 mgr. of radium bromide, screened with 2 mm. lead, so as to produce the gamma, or deep, radiation. The nose was treated for two hours with 25 mgr., screened with 1 mm. silver, so as to get some of the beta destructive rays also. On January 14, 1924, she received an irradiation of the larynx externally by plates, 110 mgr. for fifteen hours, screened with 2 mm. lead. Then she received six hours' irradiation with 25 mgr., screened with silver, 1 mm. to the uvula, right nasal fossa and the right nostril. The radium salt was in a little tube, held in position, after cocainization of the spot. This treatment was carried out at the Institute fairly frequently, and he knew of no cases of necrosis. The treatment was elaborated by Mr. Hayward Pinch, Director of the Radium Institute, who, the speaker believed, had practised it eleven years.

Mr. WHARRY (in reply) said that, from the clinical point of view, radium had a great advantage over any other method he had seen. The diseased area seemed to heal up without the formation of scar tissue. In one case he had seen recently, which had been treated by scraping, there was complete obliteration of one side of the nose by scar tissue. In the present case, however, after the first application of radium, the crusts came right off, and left a smooth, pink surface, which gradually shrank down, and eventually the nose condition cleared up. Recently the patient had had a slight relapse, but he thought that would disappear; he (Mr. Wharry) had had another similar case with a relapse, and that had cleared up.

With regard to the larynx, the infected surface became œdematous and was covered with a sticky secretion, and during that reaction some mucous membrane appeared to grow over the surface, but it shrank down later. That had been going on in stages after each application of the radium. The whole aspect of the case was entirely altered in a short time, though previously to this treatment the condition had been in process for four years. The voice returned and the patient could breathe through her nose fairly well six weeks after the first application of the radium.

Tumour of the Left Vocal Cord.

By T. JEFFERSON FAULDER, F.R.C.S., and F. C. ORMEROD, M.D.

Mrs. A. M., AGED 60. History of hoarseness which is said to have followed an operation for removal of a tumour (lipoma?) from the shoulder five years ago. The hoarseness is very marked and has persisted continuously with the exception of a short period three years ago when the voice returned. Previously to the operation five years ago tumours—probably papillomata—had been removed from the skin over the knee and the scalp.

On examination there are found to be a papilloma attached to the uvula and a smooth, rounded swelling of the left vocal cord chiefly involving its anterior two-thirds. The cord appears freely mobile but is somewhat hindered in adduction by the tumour. Nothing abnormal was found in the chest and there were no tubercle-bacilli in the sputum.

DISCUSSION.

Mr. TILLEY said he regarded the condition as simple papilloma, and thought that removal in the ordinary way should have an excellent result. If it were considered to be anything else, an alternative diagnosis lay in the direction of epithelioma, but such a large growth of that nature would have infiltrated the cord, which was now moving freely.

Six months ago he had had an apparently identical case in a man 50 years of age. He thought it was malignant, and he took the growth off, and it left only a little chemosis where it had been attached. It was sessile, and was removed by the direct method. To judge from subsequent appearances, one would not know that there had been anything amiss. It was said that the present patient had papillomata on other parts of the body. A patient was sent to his department from the Gynæcological Department of the hospital — a woman from whom a uterine fibroid had been removed—and she was hoarse on account of fibroma of the vocal cord. In that case, as in this, he thought the association in the same patient was no more than a coincidence.

Mr. FAULDER (in reply) said that the case resembled one shown before the Section a year ago. To-day the tumour was smaller than he had yet seen it. At the present time she had a papilloma of the uvula. The case shown last year was diagnosed as fibroma, and that patient also had a papilloma of the uvula. He asked what happened to these cases if nothing was done. This patient has had a hoarse voice five years, and the other patient he mentioned had been hoarse six years, and refused treatment, yet in her no growth seemed to have developed.

Mr. H. J. BANKS-DAVIS (President) said the case deserved a further exhibition at the June Congress.

Tumour on Right Side of Larynx.

By H. J. BANKS-DAVIS, M.B. (President).

WOMAN, aged 43, with tumour on right side of larynx. Exhibited at the November meeting, 1923, as a case for diagnosis. (Cyst ?)

Laryngo-fissure performed on January 5, and a tumour of considerable size was removed from the right side of the larynx and arytenoid region. The tumour, which shelled out easily, looked like a fibroma. The woman left the hospital to all intents and purposes relieved of her obstruction. She has recently returned with perichondritis in the site of the original tumour. Wassermann negative. Microscopic specimen shown.

DISCUSSION.

Mr. H. J. BANKS-DAVIS: I showed her at the November meeting. Mr. Vlasto assisted me in a thyro-fissure. The growth turned out to be larger than it seemed. The pathologist was certain it was malignant, and at the meeting Mr. Dawson held the same view, but I do not think there is the same certainty now. She has a recurrence at the site of the original growth, and perichondritis, great difficulty in breathing, and granulations on the larynx; the latter I could easily remove with Mackenzie's forceps, as I have done before. I shall be glad of assistance in regard to the diagnosis.

Dr. W. HILL said it was admitted that there were columns of epithelium in the section, and a large amount of œdematous structure. He did not know whether the tumour was made up of ordinary enlarged glands from the ventricle.

Mr. G. W. DAWSON said it was a very large tumour occupying the right side of the larynx, and if it had been malignant it probably would have ulcerated by this time, because of its size, but it had not done so.

Mr. HAROLD BARWELL thought the tumour was possibly septic.

Sir JAMES DUNDAS-GRANT said it looked like adenoma.

Mr. H. J. BANKS-DAVIS (President) (in reply) said that Mr. Elworthy considered it should be regarded as a low-grade malignant type of growth. An aspirating needle showed that it was solid. He (Mr. Banks-Davis) thought it likely that the patient would have been better off if he had done nothing, but she pressed for an operation owing to occasional stridor. To-day the left side had not a normal appearance. Four months ago he tried the effects of mercury and iodide of potassium, but without result.

Tumour in the Posterior Wall of the Pharynx.

By H. J. BANKS-DAVIS, M.B. (President).

MAN, aged 45, exhibited at same meeting (November, 1923).

He had a tumour in the posterior wall of the pharynx pressing down the epiglottis and occluding the airway. Thinking that he had a posterior pharyngeal abscess I incised it and found it was solid, resembling a lipoma.

Wassermann reaction was positive.

The patient has been treated only with iodide of potassium and the tumour has almost entirely disappeared.

DISCUSSION.

Mr. H. J. BANKS-DAVIS: I thought it was a retro-pharyngeal abscess, and I took the patient in for the purpose of opening it, as he had much difficulty in breathing, but it was solid and I did nothing more. Wassermann reaction reported to be positive. He has not had N.A.B., but iodide of potassium only. The swelling has almost disappeared. If it had been gumma, it would have ulcerated, and it would have left a scar.

Mr. T. JEFFERSON FAULDER said that Sir Robert Woods had reported a case of retropharyngeal tumour in which there was said to have been no difficulty in diagnosing it from abscess; therefore, he (the speaker) reported a case of his own in which it could not be settled which it was. What was the proper incision by which to approach this case? Sir Robert Woods' case was operated upon from in front of the sterno-mastoid; he (Mr. Faulder) did his operation from behind the sterno-mastoid, which was the correct route for chronic abscess or retropharyngeal tumour. The tumour in his case seemed to be underneath the prevertebral fascia, and it was 6 in. or 7 in. long. He did not see why a gumma should not develop in the fibrous tissue there.

Mr. E. D. D. DAVIS said an incision at the anterior border gave much better access. A retropharyngeal abscess usually pointed at the posterior border of the sterno-mastoid and could be approached from this position.

Mr. NORMAN PATTERSON said he did not agree with Mr. E. D. D. Davis's view. In a case of his own an attempt was made by the house surgeon to open in front of the sterno-mastoid, but it failed, and he himself had to open posteriorly. If one made a long incision and pulled the sterno-mastoid forward, it was easy to get a good view of the retropharyngeal region, and one did not then interfere with the carotid sheath.

Mr. H. J. BANKS-DAVIS (President) (in reply) said that at one time it was taught that in all these cases the opening should be made from the neck, as the cases became septic. But it had been found that the abscesses could be opened from the mouth with perfect safety. In a case of retropharyngeal abscess in a baby, with much œdema, he made his incision in the neck, behind the sterno-mastoid as he thought, but he was really in front of it. The patient did quite well, however.

Laryngeal Growth (? Cyst).

By JOHN F. O'MALLEY, F.R.C.S.

D. R., FEMALE, aged 33. Complains of hoarseness of thirteen years' duration. Recently sent to see me at University College Hospital as a case of a "polypus of the voice-box." A smooth semi-translucent mass is seen on the left side, on the upper aspect of the vocal cord and overhanging its free border. It prevents the approximation of the two cords on phonation, hence the blurring of the voice.

Exhibitor would like opinions (*a*) as to its proper nomenclature on pathological lines; (*b*) its exact attachment; and (*c*) suggestions for best method of removal or treatment.

DISCUSSION.

Dr. JOBSON HORNE, answering the exhibitor's questions, said the nomenclature suggested, "polypus of the voice-box," was excellent. The attachment seemed to be to the anterior commissure. As he considered it an innocent growth, he would remove it by the indirect method with a pair of Krause forceps.

Mr. HERBERT TILLEY suggested that a cocaine solution and some ordinary curved probes should be provided for cases of this kind so that they might be more easily inspected. He could not see the tumour in this case.

Mr. G. W. DAWSON said he saw the tumour; it was flapping about underneath the vocal cord.

Mr. HAROLD BARWELL said that removal should not be particularly difficult by the indirect method, and he would remove it in that way, as suggested by Dr. Jobson Horne.

Mr. J. F. O'MALLEY (in reply) said that it was his intention to try to effect removal by the indirect method. Curved forceps should get on to the main mass; he did not think the attachment would be very broad, and no severe lesion would be likely to follow the attempt to remove it. In reply to Mr. Tilley, he said he thought cocaine was always obtainable in the examining room.

(?) Carcinoma of Anterior Portion of Floor of Mouth with another quite separate Growth (? Carcinoma) in the Larynx.

By NORMAN PATTERSON, F.R.C.S.

MALE, aged 60. For six months has noticed small lump on right side of tongue for three months, swelling on left side of neck, slowly increasing. Pipe smoker.

Present condition: Large indurated area on floor of mouth right side, tongue and alveolus involved, ulcerated, very hard. Tumour involving epiglottis and ary-epiglottic region. Large mass of glands high up on left side of neck. Sections are being cut and will be shown at the meeting. The exhibitor is indebted to Mr. Robert Milne for permission to show this case.

DISCUSSION.

Sir JAMES DUNDAS-GRANT said he understood that this case was not as typical of epithelioma as the appearance led one to think it would be.

Mr. NORMAN PATTERSON (in reply) said that the patient was sent to him by a surgeon for treatment by diathermy. The growth was on the right side of the mouth, and the mass of enlarged glands was on the left side of the neck. On examining the larynx he found a secondary growth involving the epiglottis and the structures round the orifice of the larynx. All the lesions seemed to be typically malignant; still, he had sections from them cut, taking what he thought to be a characteristic section from the floor of the mouth, and another from the epiglottis. The report stated that there was no evidence of carcinoma, and that the Wassermann was negative. What was the condition? It was not considered to be actinomycosis. He would be glad to give any further evidence possible, and would try to show the patient on another occasion.

Case of Distortion of the Larynx producing Tracheal Obstruction due to Cicatricial Contraction following an Abscess.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

PATIENT, a male, aged 35, complained of difficulty in breathing and loss of power of concentration. His difficulty began with the occurrence of an abscess in the neck when 12 years old, attributed to a fall.

At present there is a stridor of tracheal rather than of laryngeal character, worse when the head is bent to the left and more audible on inspiration than on expiration. On laryngoscopic examination the larynx is seen to be contorted as if bent over to the right, the left vocal cord being entirely hidden by the ventricular band, while the right vocal cord, which is visible, is seen to be immobile; presumably the left cord moves normally. There is a deep cicatricial pit behind the sterno-mastoid at the level of the thyroid cartilage and another in front of the muscle, but much lower down. From the upper depression there is apparently a short fibrous band passing to the thyroid cartilage and probably drawing the right cornu of the hyoid bone downwards. The exhibitor would like opinions as to whether liberation of the larynx by operation on the fibrous adhesions is advisable.

DISCUSSION.

Mr. MUSGRAVE WOODMAN said that the larynx was drawn over, and that he would advise operative treatment, though it would prove a difficult operation. There was a deep and tense band passing obliquely from the larynx towards the inner end of the clavicle, and when the sterno-mastoid was drawn outwards, it was found to be drawing up the subclavian vein, and it was closely connected with the internal jugular vein, and probably with the dome of the pleura. The larynx having been straightened, how could it kept so? It was of no use to sew up and allow a blood-clot to form, as there would be recurrence. One might take a layer of fibrous tissue from the fascial planes, turn it in, and protect it from fibrous tissue. Or there could be carried out a method which was originally suggested by Mr. Wilfred Trotter, which the speaker had used several times for approaching the side of the pharynx and the œsophagus, namely, sewing the sterno-mastoid down to the prevertebral muscles in front of the carotid sheath and internal jugular vein; then there would be a layer of muscle outside the sterno-mastoid passing down to the prevertebral muscles, and one could see the larynx from the side. The great difficulty would consist in safely isolating the larynx, dividing the fibrous tissue, and getting the condition rectified.

Sir JAMES DUNDAS-GRANT (in reply) said that the stridor was tracheal, and particular attitudes had to be adopted by the patient to enable him to breathe. He considered that the cicatricial mass involved the recurrent laryngeal nerve, as there was no movement of the right vocal cord. He hoped to show the case again.

Microscopical Section from the Epiglottis of a Middle-aged Male Patient, with Supposed Tuberculosis.

Shown by Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

THE epiglottis was enormously enlarged and folded in from the left side, while below there was an enormous œdematous swelling in the aryepiglottic region. The section is reported to have the appearance of a basal-celled epithelioma.

This is a distortion which I have not seen in tuberculosis but only in malignant disease. The section shows this to be an epithelioma "of sorts," possibly a less malignant type, like rodent ulcer. A pathologist says it is the basal-celled type of epithelioma. This is, I understand, a type resulting from proliferation of the cells deeper than the Malpighian layer; the Malpighian layer would give a prickle-celled carcinoma or epithelioma.

Diathermy Removal of Epithelioma of Left Tonsil, Soft Palate, Faucial Pillars and Tongue.

By DAN MCKENZIE, M.D.

THE patient is a lady, aged 70. The operation was performed on June 27, 1923. The external carotid was ligatured and a chain of small glands removed. In the diathermy puncture of the tissue between the neck wound and the growth in the pharynx the internal jugular vein was accidentally punctured by the diathermy needle, and had to be ligatured.

The growth in the throat was removed in one mass, except for an infiltrated area in the lateral pharyngeal wall behind the tonsil, which was destroyed by the diathermy as far as possible.

During convalescence the patient was found to have glycosuria. It did not affect the healing of the wounds in the slightest degree.

DISCUSSION.

Sir JAMES DUNDAS-GRANT said that there was no return of the disease, nor sign of glandular involvement, and, so far, the case seemed successful.

Mr. H. J. BANKS-DAVIS (President) asked what was the object of tying the external carotid and jugular if removal was to be by diathermy?

Mr. NORMAN PATTERSON said that in a case of high blood-pressure to be treated by diathermy, ligature of the external carotid at the time of the operation reduced the risk of hæmorrhage. But he now seldom tied this vessel, since he used a button instead of a sharped-edged electrode. Ligature of veins seemed likely to increase hæmorrhage rather than to diminish it.

Mr. MUSGRAVE WOODMAN said that in the first case of this kind in which he operated the carotid was not tied, and there was severe hæmorrhage. Now, however, he seldom tied the carotid, and there was no trouble. In the opinion of general surgeons the vein also should be tied; either both should be tied or neither. When only one was tied there was much bleeding and oozing, he thought. If hæmorrhage was anticipated, it would be mainly secondary. If the carotid were tied at the date of the operation, secondary hæmorrhage would supervene in from seven to ten days, and if secondary hæmorrhage seemed likely to occur, it would be wise to tie the vessel seven to eight days after the primary operation, to circumvent the collateral anastomosis which so quickly resulted after ligature of the external carotid.

Section of Laryngology.

President—Mr. H. J. Banks-Davis, M.B.

DISCUSSION ON SUPPURATIVE DISEASES OF THE FRONTAL, ETHMOIDAL AND SPHENOIDAL SINUSES.

I. ILLUSTRATIVE CASES AND SPECIMENS.

Case of Suppurating Frontal Sinus treated by External Operation.

By E. Musgrave Woodman, M.S.

Three years ago patient had an attack of pain on the right side of the face, and a further attack of pain on the left side, lasting for several days, six months ago. Both attacks were accompanied by muco-purulent discharge down the back of the throat. He complained of mental dullness in the morning on waking, and when the discharge came away the condition cleared up.

On examination pus was found in the left post-nasal space and the left spheno-ethmoidal fissure. The left frontal sinus was dull and the left antrum was dim. X-ray examination showed the left frontal sinus dull and the right dim. On washing out the left antrum, flakes of mucus were found.

I operated upon him on January 2, 1924. Thick pus was found in the sinus and a dehiscence in the septal wall revealed the fact that the right frontal sinus was full of pus. A drainage tube from the left frontal sinus to the nose was kept *in situ* for fourteen days, but no external drainage was carried out.

Specimen Demonstrating an Ivory Exostosis of the Floor of the Frontal Bone.

Shown by E. Musgrave Woodman, M.S.

The upper part of the specimen is part of the right frontal bone. The tumour forms a flattened oval mass measuring 5 cm. in height, 2·5 cm. in thickness and 3·5 cm. antero-posteriorly. Its surface is smooth on the whole but presents irregular depressions and nodular outgrowths. It grows by a stout pedicle from the interior of the frontal sinus, which can be seen from the right side of the jar to be filled completely by bony tumour tissue. The tumour is extremely hard in consistence, and on microscopic examination after decalcification is found to be composed entirely of bone.

A male, aged 30 years, had suffered from discharge from the right eye for four years. On March 17, 1910, he had an acute dacryocystitis on the right side, necessitating incision. On March 22 the patient was admitted suffering from epileptiform convulsions and high temperature pointing to meningitis. The nose was found to be full of pus, and a tusk was seen projecting into the upper part of the right nostril. All the sinuses on this side were full of pus. He died two days later.

At the post-mortem examination a large subdural abscess was found underlying the whole right hemisphere. This had originated from suppuration in the remains of the frontal sinus, the infection extending through the dura along the track of a small vein. When *in situ* the bony tumour projected from the frontal sinus down through a much dilated infundibulum, and occupied a great part of the right nasal cavity. The middle turbinal process was stretched over its mesial aspect and the lower turbinal was pressed outwards by its lower end.

Case of Localized Ethmoidal Suppuration operated upon by the Sluder Method of opening the Ethmoid and Sphenoid intranasally.

By W. T. GARDINER, M.B.

PATIENT complained of constant pain in the region of the right mastoid process which occasionally extended to the occiput and into the right shoulder. X-ray examination showed localized opacity in the ethmoid. The middle turbinal was removed and the posterior ethmoidal cells and the sphenoidal sinus opened by Sluder's method. Pus evacuated at operation.

Two Cases of Sinus Disease treated by the External Operation.

By HERBERT TILLEY, F.R.C.S.

(1) MALE, aged 68. Chronic, left side, purulent nasal discharge, associated with left frontal headache.

(To demonstrate the absence of any deformity when the external operation for complete obliteration of the sinus has been performed on a frontal sinus of average dimensions.)

(2) Female, aged 58. Suffered from some headache on the left side associated with purulent nasal discharge. Diagnosis : All the sinuses on left side affected by chronic suppuration with subacute inflammation of the frontal sinus. External operation on frontal sinus and intranasal removal of left ethmoidal labyrinth and removal of anterior wall of sphenoidal sinus. Operation two years ago.

Fibrous Osteoma simulating Mucocele of Frontal Sinus and Ethmoid.

By WALTER HOWARTH, F.R.C.S.

E. C., AGED 20. Ten days before admission left eye protruded slightly, whilst patient had a cold. This subsided. Two days before admission left eye

protruded again ; proptosis increased, and there was redness and œdema in the region of the inner canthus with some chemosis and impairment of the eyeball movements.

Operation revealed what was apparently a large mucocele expanding the whole of the ethmoid and the lower portion of the frontal sinus. This is well shown in the X-ray photograph.

The contents were practically solid and very dense. The lower part that bulged into the nose is exhibited, together with microscopic sections and the X-ray film.

The sections show (1) the bony envelope lined with inflamed mucous membrane, many flattened mucous glands being present ; (2) the bony tumour with much fibrosis.

Osteoma causing Mucocele of Frontal Sinus.

By WALTER HOWARTH, F.R.C.S.

H. L., AGED 40. Underwent many snaring operations for nasal polypi at hands of local practitioner. When seen in August, 1923, extensive suppurative ethmoiditis was found, and radical operation advised. Whilst waiting for admission he noticed that left eye began to bulge forwards and outwards.

On admission in December, 1923, a firm irregular swelling was felt to project from the left ethmoid and frontal sinus into the orbit. This was also seen on the X-ray film. The sinus above is seen to be large and expanded.

At the operation the osteoma exhibited was found to be occupying the whole of the ethmoid and the lower part of the frontal sinus ; above the osteoma there was a large mucocele of the frontal sinus, the contents of which were of the usual thick glairy, gelatinous nature.

Case of Acute Sinus Disease in a Child of Five, involving the Ethmoid and Antrum.

By E. WATSON-WILLIAMS, M.C.

O. T., FEMALE, aged 5, was brought up for swelling of left eye, February 29, 1924.

History.—For some months she has had intermittent discharge from the left nostril. Three weeks ago she had a bad cold, and since then the discharge has been worse and continuous. Five days ago the left eye began to swell (on February 24, 1924) and this swelling has gradually increased and the child has become ill.

Examination.—The child is "stupid," restless, and peevish ; will not answer questions. Gives a cry from time to time suggesting meningitis. No head retraction. Temperature 100° F.

Eyes.—Left eye is pushed definitely out and down, and the appearance of proptosis is increased by œdema of both lids. These are not tender, but suggestive of a sarcoma rather than inflammation. Conjunctiva normal.

Nose.—A purulent discharge flows from the left nostril, which is reddened. Nothing abnormal in ethmoidal or lachrymal regions.

Treatment.—Adrenalin spray 1/5000 to nose; ether anæsthesia. On the nose being wiped clean, a purulent discharge was seen welling up from the middle meatus. The middle turbinal and ethmoid region were soppy, but showed nothing to suggest the presence of a neoplasm. The middle turbinal was clipped away with punch forceps, and I found that this exposed a rather large posterior ethmoidal cell, from which pus appeared to be coming. The antrum was next explored through the middle meatus, and found to be full of pus so thick that it was with difficulty drawn into the syringe. A small opening was made high in the inferior meatus to drain the antrum: as I had found already enough to explain symptoms, I did not think it necessary or justifiable to enter the sphenoidal sinus. Lumbar puncture: negative.



Skiagram.—The child was admitted at the end of the day's operating, and the skiagram was not taken till a later occasion. It shows the opacity of the left ethmoid and antrum, compared with the right. There is no trace of frontal sinus. The relation of the permanent and milk teeth to the antrum, and the relative levels of the floor of this sinus (which is at least $\frac{1}{8}$ in. higher) and of the nose, are also well shown. There is no obvious bone deformity.

Course.—The temperature fell next day, but rose on the fourth day, with some bronchitis. The nose discharged freely at first, but this is diminishing.

The eye has steadily improved till it shows now (March 7) slight ptosis only. General condition fair. Recovery uneventful.

I am indebted to Mr. Harty, under whom the child was admitted to hospital, for permission to use the notes of the case.

Case of Sinus Disease with Inflammation of Supra-orbital Nerve.

By J. ALDINGTON GIBB, M.B.

J. W. J., MALE, aged 43, miner's supervisor.

History.—In May, 1923, the patient noticed a slight swelling over the left eye, about which he consulted his doctor. He was given powders which relieved it, and the swelling disappeared.

In the middle of December, 1923, the swelling recurred and the pain on this occasion was more severe. The headaches were worse at night and interfered with his sleep. He noticed that there was an unpleasant discharge, slight in amount, coming down the back of his nose into his mouth.

Came to hospital, January 12, 1924. Admitted.

Examination.—Complained of pain extending from left frontal region to vertex and back to external angular process on left side. Slight pain on right side also, over the orbit. Fluctuant and tender swelling over left orbital margin in region of frontal sinus.

Upper and middle turbinates enlarged and blocking exit from frontal sinus. The middle turbinate on the left side was hard up against septum. Pus seen in post-nasal space. Tenderness marked over left frontal sinus. On trans-illumination: both antra dark, especially the left. Left frontal sinus dark.

Temperature, 100·4° F.; pulse 120; respirations 20.

On January 13: Pain more localized to left frontal region, and slightly over the right orbit.

Operation.—Under ether and chloroform. Left frontal sinus opened. Supra-orbital nerve seen to be somewhat larger than normal, with some œdema and congestion associated with it. No pus found in the frontal sinus. Burr passed into nose from the sinus, and tube inserted. Some pus found in the ethmoids. Portion of supra-orbital nerve removed. Maxillary antrum burred. Tube removed next day, and frequent alkaline douches given. Recovery uneventful and uninterruped.

January 30: Ethmoids perfectly clear. All symptoms gone.

February 2, 1924: Discharged cured.

The interest of the case lies in the intensely inflamed condition of the infra-orbital nerve, which was pulled out of its canal.

Ethmoidal Exostosis.

Shown by T. JEFFERSON FAULDER, F.R.C.S.

REMOVED by operation from a female patient, aged 20, in 1909. One part of the growth lay in the right orbit and caused exophthalmos, diplopia, and reduced $\frac{5}{6}$ vision. The other part lay in the nose, causing much obstruction. The patient suffered also from headache and epiphora. The tumour is densely

hard. During the operation the floor of the frontal sinus was opened. There was no suppuration in the nose.

Patient was seen five years later. There was no recurrence, and vision was almost completely restored.

Case for Diagnosis.

By J. F. O'Malley, F.R.C.S.

L. T., FEMALE, aged 29.

Headaches for six months over left eye and spreading to left mastoid. Pain present on rising and lasts throughout the day.

History of discharge from nose at times, but none seen on examination.

X-ray shows absence of frontal sinuses.

Is this a case of spheno-palatine ganglion neurosis or of localized ethmoidal suppuration?

X-ray plates shown.

Revolver Projectile impacted in the Ethmoid.

Shown by Dan McKenzie, M.D.

THIS youth accidentally shot himself in the nose while playing with a toy revolver, which he had, very ingeniously and with great pains, rendered dangerous.

The scar of the entrance wound lies to the left of the mid-line of the point of the nose.

The projectile, which is a piece of lead fashioned by the patient, is in two pieces and lies, impacted, in the left ethmoid quite close under, if not actually penetrating the cribriform plate—or rather the floor of the anterior cranial fossa lateral to the cribriform. Here it can be felt with a probe.

I should be glad of opinions as to treatment. I am inclined not to interfere at present but to await its becoming detached.

X-ray photographs and the revolver exhibited.

II. DISCUSSION.

Mr. MUSGRAVE WOODMAN

said that suppuration of the upper sinuses as enumerated in the title constituted border-line territory. Above, the physician held sway, and often confirmed his diagnosis on the post-mortem table. From below the rhinologist looked up along the dark passages and around corners. Between the two lay a territory as yet very imperfectly explored. Such could only take place by collaboration and accurate histological examination on the part of those who dealt with either side. Comparison of the frontal sinus with the gall-bladder was an apt one: the latter was a cavity drained at its lowest part by a tube leading to a larger cavity, and its illnesses largely depended on the conditions in the duct and the morbid processes which surrounded that duct. Dr. Logan Turner was insistent that before any operation on the frontal sinus was contemplated a complete X-ray examination should be undertaken to disclose its pathological anatomy. Frontal lobe abscess, of which he was exhibiting a specimen, was a somewhat rare complication. An important complication was osteomyelitis, which was well known as a sequel of any operation in the region of the frontal sinus; it rarely arose after operations on the antrum, and but rarely in connexion with procedures on the ethmoid. The mucous membrane lining was continuous with that of the nose, the bony walls were continuous with the nasal sinuses, and the abnormality was in the blood supply. There were no veins in the frontal sinus itself. The free anastomosis existing between the angular veins, the superior longitudinal sinus, and the emissary veins of the meninges, suggested a vascular connexion which had much to do with the occurrence of osteomyelitis in the frontal sinus. With regard to neuralgia of the frontal sinus, many of these cases did not come to the rhinologist. One case he quoted was that of a man who was well until five years ago, when he began to suffer from nasal catarrh. Every subsequent winter he had a sense of oppression and discomfort over the right side of the forehead. Last winter it culminated in a violent attack, and he sought advice. He (Mr. Woodman) at the operation, found pus in the frontal sinus under considerable tension.

Concerning the methods of treatment of frontal sinus conditions, those who knew the intranasal approach to the sinus were aware of the cases in which that was suitable, and what were its limitations; they also knew its complications. But it was very desirable to know, without bias, what type of case was most suitable for the external operation. The best type of external operation had now been placed beyond question; it was that devised by Mr. Walter Howarth, a member of the Section. He (Mr. Woodman) regarded it as a perfect operation. Was an external operation necessary in all cases? What were its complications? Did osteomyelitis arise from it? It was less likely to arise, as, by this operation, the bone was approached through the floor. It was desirable also to know what were the results of the operation and the difficulties of drainage which had to be encountered.

With regard to the ethmoid bone, it was a spongy bone, and a source of permanent and latent infection. Infection spreading up the nose receded again and left infective organisms in the loculi. The ethmoid was divided obliquely into an antero-inferior portion and a postero-superior portion, which

were distinct, influencing respectively the frontal sinus in front and the sphenoidal fossa behind. The normal ethmoid bone had a large number of perforations, due to the passage of vessels through it. The difference between the dura mater on the floor of the anterior fossa and of the posterior fossa was very considerable. Frontal suppuration would cause extradural abscess, or frontal lobe abscess, but it did not often cause generalized meningitis. He had seen two cases of encephalitis lethargica in which every sinus in the head was full of pus, and the patient was seen to be dying. The physician and rhinologist had met in consultation, but no post-mortem examination was allowed. A series of investigations were much wanted on these meningitis cases in which death ensued following sinus involvement; many of them had had persistent sinus trouble which had been unsuspected.

Mr. Woodman said that he was convinced that cavernous sinus thrombosis arose primarily from the ethmoid, not from the sphenoid. It would seem to be the right treatment to begin a complete exenteration of the ethmo-sphenoid on one side, and so approach the cavernous sinus and turn out the clots. In the ethmoid a subtle form of neuralgia was met with, and its origin was apt to be missed. He related a case of it. It was desirable to know whether it was necessary to remove all the ethmoid cells, or whether the opening up of a certain number was sufficient. In the former event he discussed the best procedures. The sphenoid was a plain cavity; he showed a slide of it, indicating the close proximity of the palatine ganglion, the vidian nerve, and the cavernous sinus to each other. The slide showed how disease in the posterior cells could exert its influence. The sphenoid he considered was more often maligned than it deserved to be; the source of trouble was more often in the ethmoid. The importance of the sphenoid lay in its relationship to the optic foramen and the vidian canal. Sluder's experiments showed that the first and second divisions of the fifth nerve could be reached by cocaine, and therefore by sepsis, as by drugs. He exhibited slides to illustrate the danger of operating on the ear for sphenoidal disease when sphenoidal pain was referred behind the ear. Suppuration in the sphenoidal cavity might cause dilatation of the pupil and visual symptoms.

In conclusion Mr. Woodman referred to the connexion of the sinuses with the pituitary. It was now well known that a source of sepsis anywhere in the body had a marked influence on the thyroid gland; therefore, in view of the permeability of the sphenoidal sinus by drugs, he asked whether it was not probable that in latent or active infection of that sinus the pituitary itself would be affected. He thought there was a wide field for research in connexion with the pituitary gland; and that meningitis and neuralgia also stood in need of a good deal of investigation.

Dr. W. T. Gardiner.

(Lantern demonstration showing steps of the Sluder method of opening the ethmoid and sphenoid intranasally.)

The operation is performed under local anæsthesia. The nose is packed with equal parts of 10 per cent. cocaine and adrenalin. The packing remains in the nose for half an hour. At the end of that time the packing is removed and a thinly covered probe moistened with one drop of 90 per cent. solution of cocaine is passed into the middle meatus until it lies laterally to the posterior end of the middle concha. In this position it lies over the spheno-palatine ganglion. It is allowed to rest there for ten minutes. A similar thinly covered probe is passed upwards into the nose far forward as high up as it will go. It comes in contact with the lateral nasal nerves and is also left there for ten

minutes. The small Sluder knife is then passed up into the middle meatus laterally to the anterior end of the middle concha as far up as it will go. The knife is then rotated at a right angle so that the point faces inwards. It is then drawn downwards with a strong inclination forwards. The knife is withdrawn. The larger Sluder knife is then introduced into the olfactory sulcus, with the cutting edges downwards, until passing upwards it reaches the cribriform plate. The handle is then rotated through a right angle so that the cutting edge of the knife lies against the ethmoid. It is then pulled forward and by this means cuts into the posterior end of the first cut. The knife is withdrawn and reintroduced in the same position a little farther back and is again rotated outwards and drawn forwards. The middle concha is thus detached from the ethmoid, within 2 mm. of the cribriform plate. It is removed by a snare or Luc's forceps. The operation may be stopped at this stage if it is desired only to establish drainage of the ethmoidal openings. If it is desired further to open the ethmoid, the hook-shaped knife is again introduced until it touches the cribiform plate, turned outwards towards the ethmoid, and is drawn downwards in a series of parallel cuts, always working from above downwards. After removal of the middle concha by this method, a good view of the anterior face of the sphenoidal bone is obtained. To open the sphenoidal sinus, the hook-shaped knife is introduced into the nose, passed along the roof, keeping as close to the septum as possible, and gently breaking through the anterior wall. When the hook is engaged, it is then drawn forcibly forwards, breaking down the anterior wall of the sphenoid. To open the posterior ethmoid cell thoroughly, the knife is again introduced into the sphenoid, turned outwards through a right angle and drawn forwards. The opening into the sphenoidal sinus may then be enlarged by suitable punch forceps. The after-treatment consists in menthol inhalations for forty-eight hours and then in syringing the nose with a 0·25 per cent. solution of phenol in liquid paraffin, introduced in a ½-oz. glycerine syringe.

Dr. Douglas Guthrie

gave a demonstration of specimens and lantern slides illustrating the variations of the cribriform plate and its relation to the upper nasal sinuses.

He said that a few weeks ago he set out to find the degree of friability of the cribriform plate and the relationship of the anterior ethmoidal cells to the cribriform plate, also to see how easily the cribriform plate could be fractured. For the latter purpose he introduced a Sluder knife into the noses of six cadavers, and he found he could penetrate the anterior part more easily. Hitherto it had been supposed that the posterior portion was the more friable. The anterior part contained many large foramina, and it could easily be penetrated on each side of the crista galli.

With regard to the relationship of the ethmoidal cells to the cribriform plate, Dr. Guthrie showed several photographs. He had examined 200 skulls and found that in 74 per cent. of the cases the ethmoidal cells rose above the cribriform plate and that the olfactory groove was deep. In the remaining 26 per cent. the olfactory groove was shallow and the ethmoidal cells rose only slightly above the cribriform plate. In this latter type the cribriform plate was unduly broad. In 18 per cent. of skulls examined, the crista galli was unusually thickened, in some cases it was so thick as to convert the olfactory groove into a tunnel. This thickening was not always accompanied by a cell within the crista galli; in some cases it consisted only of cancellous bone.

Mr. Herbert Tilley

said that none of those who had heard the openers of the discussion would go away without having their field of vision broadened. Dr. Gardiner's demonstration was full of useful and practical hints as to the treatment of ethmoidal disease.

With regard to Mr. Woodman's reference to osteomyelitis, it might be of interest for the Members to see a lantern slide of the calvarium in which that complication (as a result of chronic suppuration) was first described; he (the speaker), brought it before the Portsmouth meeting of the British Medical Association in 1897, and at that meeting, Dr. Luc, of Paris, also described a case illustrating the same condition. The patient, a woman, lingered nine months, and in sheer desperation at the last operation he made a transverse section of her scalp, such as was employed in the post-mortem removal of the roof of the skull. Necrotic and infected areas of bone were taken out and as much free drainage as possible was established. Autopsy revealed metastatic abscesses in different regions of the body.

The question had been raised as to the comparative value of the internal and the external operation on the frontal sinus, and Mr. Woodman asked for opinions on this point. He did not think it would ever be agreed that one could treat all cases in the same manner. There were patients, like the second one he had shown that day, in which the operation must be the external one, and in which there would necessarily be some deformity. In this instance there was a large sinus, acute inflammation of the bone and œdema of soft tissues of the lower frontal region. Two years later she suffered from severe headaches, and he (Mr. Tilley) opened the left sphenoidal sinus and removed the corresponding ethmoidal cells.

Mr. Woodman said he did not know of a case of osteomyelitis which did not start in connexion with operations on the frontal sinus. Two years ago he (the speaker) operated on a chronic suppurative maxillary antrum, and within a few weeks inflammation had spread to the frontal sinus, and the patient died of meningeal complications after extensive osteomyelitis. Dr. Dan McKenzie had recorded a similar case.

As to the danger of osteomyelitis, he thought when the sinus had been opened one should avoid using any sharp curetting instruments at the junction of the anterior and posterior walls, otherwise the operator was apt to open some of the small diploæ and allow infective material to enter. The sinus should be irrigated, and infected tissue gently removed with small gauze swabs. If those precautions were observed, he believed there would be fewer cases of osteomyelitis in the future. Fatal osteomyelitis could occur without any operative interference and he had seen five cases of this type.

Sphenoidal sinus disease he regarded as very common; he thought that some of the cases of so-called chronic post-nasal catarrh were really due to sphenoidal sinus suppuration. One of the most elusive and yet valuable symptoms was headache, it varied in degree and in its situation. Sometimes it was at the vertex, sometimes over the occiput. In 1905, when he opened a discussion on sphenoidal sinus suppuration (British Medical Association, Annual Meeting at Leicester), he instanced two cases amongst eight in which the chief symptom was intense pain in the ear and mastoid region. Another most important indication was dry pharyngitis, which might be unilateral in its distribution. When there was recurrent hoarseness associated with thickening of the interarytenoid fold to which crusts of dry mucus were

adherent, he often found the conditions due to suppuration of the posterior ethmoidal and sphenoidal sinuses.

Mr. E. Watson-Williams

said that so far as his experience went, a chronic frontal sinus disease could usually be relieved by a per-nasal operation to secure free drainage of the sinus into the nose. There remained, however, a definite number of cases in which drainage was either good from the outset or in which such an operation did not relieve. In some, the skiagram and operative findings alike failed to explain why this was so. In others, however, there appeared to be a pocket or loculus which did not drain readily into the main sinus. An exaggeration of this last condition might occur, with a very lateralized extension which might even escape notice at operation, unless specially looked for.[1] He had brought skiagrams of such cases, because he very much wished to learn what type of external operation the members of the Section thought most suitable in such cases. The Killian operation left a dead space behind the "bridge": the osteo-plastic operation might also do this. It did not appear that Howarth's operation was certain to relieve frontal sinus disease in a case in which free drainage of the sinus had previously existed. While a complete removal of the whole front wall and floor of the sinus avoided this, the resulting deformity in such cases with large sinuses was considerable, although, if a lateral extension of the sinus existed, this must doubtless be faced.

With regard to the fourth case, this appeared to be of unusual interest on account of the age of the patient—5 years. Acute suppurative disease in children of tender years could not be so very rare; this was the third case on which he had operated within twelve months.[2]

Dr. A. Logan Turner

presented some figures dealing with intracranial complications. In the spring of 1923 he had sent out a questionnaire to the surgeons in the country asking for certain information upon this question.

These complications were divided into two groups: (1) spontaneous; (2) post-operative. He received 125 returns; of these seventy-seven were spontaneous and forty-eight post-operative complications. Spontaneous complications occurred mainly in patients under 30, in two cases in patients under 10 years of age. Thirty-four cases were complicated with acute and subacute sinusitis, and forty with chronic sinusitis. He would like to know the total number of sinus cases which occurred in the practice of those surgeons who sent in returns, i.e., in what percentage complications arose in sinus suppuration, in order to compare them with intracranial complications arising in aural suppuration. Spontaneous complications occurred when only one sinus was affected, in 37 per cent., and in 62 per cent. when two or more sinuses were affected.

The sinus responsible for the complication was ascertained in seventy-one cases: frontal forty-one, sphenoidal nineteen, ethmoidal nine, maxillary two.

[1] Vide *Lancet*, 1923, i, p. 1056.
[2] See *Proc. Roy. Soc. Med.* (Section for the Study of Disease in Children), 1923, xvi, pp. 81-84.

The next table (shown) was compiled from the post-operative complications. One surgeon who sent answers said he thought it would be found that the post-operative complications exceeded the spontaneous, but that idea was not borne out by the figures, though he wondered whether all the post-operative complications had been returned. Forty-eight were post-operative, 89 per cent. after chronic sinus suppuration, 10 per cent. in acute sinus suppuration. In forty-three of the cases of multiple sinus suppuration with complications one or more of the affected sinuses were left untouched by the surgeon. Therefore there was a tendency for post-operative complications to arise: (1) where several sinuses were affected, and (2) where certain of the sinuses were left unoperated upon. Complications occurred in twenty-two intranasal operations, and after twenty-six external operations.

Osteomyelitis occurred in 58 per cent. operated cases; in ten of the twenty-two intranasal operations, in eighteen of twenty-six extranasal operations, showing that the danger of osteomyelitis was not confined to the extranasal operation; and he agreed with Mr. Tilley that osteomyelitis occurred after operations on the antrum; it might begin in the wall of that cavity. Osteomyelitis followed frontal sinus operation in three of eight intranasal operations, and in twelve of twenty external frontal operations. In the answers sent in, the type of external operation was frequently not stated.

Mr. J. Aldington Gibb

said he showed in the other room a patient who came to hospital suffering from intense headache, frontal, vertical, and sometimes occipital; it was so tender that the patient could not bear the forehead to be brushed by the hand. He said he had not slept for eleven days, and the pain had been present two months. There was a slight puffiness above the frontal sinus. Trans-illumination showed dullness in the antrum, and dimness in the frontal sinus. Pus seen in middle space of left naris. He did not feel sure there was frontal disease, therefore he merely took away a small part of the anterior wall. He found the antrum free from pus, but the supra-orbital nerve was intensely inflamed, and appeared about the size of a large silk ligature, and was purple in colour. He pulled it out of the supra-orbital canal. He bored a hole in the fronto-nasal duct large enough to drain it with a good-sized rubber tube, and opened the maxillary antrum. Twenty-four hours later the tube was removed, the patient was quite free from pain, and there was no longer any discharge from the nasal cavity. Pain in the mastoid did not seem to him to be always due to sphenoid disease; he had seen many cases of pain there entirely due to maxillary disease, and it cleared up after treatment directed to the maxillary antrum. He had known pain in the external auditory meatus which was due to the presence of tooth stumps in the upper jaw. The pain was relieved, not cured, by extracting the stumps. The maxillary antrum was opened and treated.

Sir StClair Thomson

said it was desirable to bring out the distinction between traumatic and spontaneous osteomyelitis of the frontal bone. In his own clinic there had been a considerable number of what he called spontaneous cases. In a large number of those the history they gave was that they had been in a swimming bath or in the sea. Most of his cases had been dealt with successfully by Mr. Hope. But the post-operative osteomyelitis was an

entirely different story. In his earlier days he had several of these cases and they did not recover; others had, however, recorded post-operative cases in which the disease was checked. Like Mr. Tilley, he had not yet seen a post-operative case of the disease recover.

He differed on one point which it was very important for the juniors to remember. He referred to the comparative disregard Mr. Woodman seemed to have for opening the cranial cavity; that gentleman said that with precautions and the application of a little iodine there was no cause for alarm. This might be so when there was no nasal suppuration about. When attempting to remove malignant disease in the nose, he (the speaker) had more than once opened the cranial cavity without disaster. Some of the members had seen him do a Moure's operation in November, 1922. Here, while tracing up the malignant disease to its source, which he had thought to be in the ethmoid, he found himself wandering inside the cranial cavity. For the growth originated in the pituitary body, yet the patient had no meningitis, and she was still alive. But it was quite a different story to operate on a septic nose, with pus about; if in these cases the cerebral cavity was opened, there followed a meningitis which was inevitably fatal. Hence opening the cerebral cavity should be religiously avoided.

Dr. W. S. Syme

said that it was difficult to know what part of this extensive subject to discuss, subject to a time limitation. He felt that if one had to operate externally on the frontal sinus, Howarth's operation was the best. He had done many intranasal frontal sinus operations, but under protest in his own mind, as he objected to an operation in which it could not be clearly seen what one was doing. He believed that if sufficient drainage could not be obtained by breaking down the anterior ethmoidal cells, then, if the suppuration continued, an external operation should be done, and preferably that devised by Mr. Howarth.

In connexion with operation on ethmoids and sphenoids, he had been much interested in Dr. Gardiner's demonstration, because he (the speaker) had procured Sluder's instruments, and had tried the operation and did not feel satisfied with it. Even now, after Dr. Gardiner's demonstration, he did not see any reason to adopt his method or to vary his own procedure. In many cases in the past he had treated sphenoid and ethmoid disease by Lack's method, i.e., passing a Moure's ring curette under the cribriform plate, well back, and then working downwards and forward, removing the middle turbinate, and opening up the ethmoidal cells. Then he passed into the sphenoidal sinus. Dr. Gardiner said he opened into the sphenoidal sinus from the centre of the anterior wall. The skiagram Mr. Woodman had put on the screen very well illustrated what he (Dr. Syme) wanted to say. If one perforated the sphenoidal sinus from the centre of the anterior wall, some day the bellying down of the roof would be perforated, and he felt great respect for the dura mater covering the floor of the cranial cavity. The safest place in which to open the sphenoidal sinus was the external angle of the anterior wall; the bellying did not occur in the external superior angle, and if a long spoon were put into that angle, using pressure downwards and inwards, one was as safe as one could be. Then the curette could be put into the side of the sphenoidal sinus and the curetting be done forward, because the posterior and other ethmoidal cells were on a plane external to the external wall of the sphenoidal sinus. Whenever the ethmoid was dealt with, thoroughness should be the aim. The only danger

he had seen in these cases was when the ethmoidal cell had been opened with punch forceps, and septic foci had been left behind in hidden corners; intracranial complications had then resulted.

Mr. H. J. Banks-Davis (President)

said he agreed that these cases were often operated upon too soon. Whenever possible he always allowed patients to have treatment in hospital for a few days preceding operation.

A short time ago he had an unfortunate case. An ordinary healthy looking man had suppuration of the ethmoidal cells; he had had polypi removed repeatedly in the out-patient department. He arrived one night, was admitted, and was put down on the operation list for the next morning. He (the speaker) removed some polypi, and curetted the ethmoid. He next day received a telephone message saying that the man had jumped out of bed, knocked the nurse down, and was difficult to control. He rapidly became comatose and died in forty-eight hours. Post-mortem, pus was found not only at the base of the brain, but also over the cerebral hemispheres, obviously a virulent infection. If a few days' previous treatment with lavage had been carried out, he felt that this calamity might not have happened. There was a dehiscence in the cribriform plate, and obviously infection had spread through this to the meninges. He wondered that this complication was not more frequent in these septic ethmoidal cases.

Mr. G. W. Dawson

said he agreed with the President. In his earlier days he had a similar experience. A case was admitted one night, and he operated next day. The man had a very suppurative nose, and acne rosacea, with pustules. After the operation the patient became infected with meningitis. Since then he had never attacked a dirty ethmoid, and he did not do anything until polypi had been removed, some injections given, and the nose cleaned. It was bad treatment to operate on a nose which was foul and streaming with pus, without a preliminary clearing up.

Dr. J. B. Horgan

said the guiding principle in attacking suppuration in the spheno-ethmoidal region was to attack it by the route which was most direct and gave the best access with safety. He felt grateful to Dr. Gardiner for his lucid explanation of the Sluder method, but he disagreed with the principle on which that operation was designed. He (the speaker) had extensively operated upon spheno-ethmoidal sinuses by intranasal methods as commonly advocated, and he had learned to regard them as inefficient in several ways. First, one had to sacrifice more or less of the turbinal body or bodies, especially the middle one. Secondly, one had to work in a field which was somewhat obscured by hæmorrhage, with monocular vision, and at a farther distance than by the transantral method, the one he adopted. It could be seen that the approach to the ethmoidal region across the antrum was easier and more correct than any intranasal method could be. The floor of the orbit and the lamina papyracea came together at an inclined plane, which enabled one to use instruments there with great ease, rapidity and safety. He opened the ethmoidal sinus at the posterior-superior internal angle of the antrum, after he had opened the antrum, and with Luc's forceps it was easy to penetrate into the posterior ethmoidal

cells, the point of the forceps being directed towards the parietal protuberance on the other side of the head. A large opening was made by breaking down the ethmoidal cells adjacent to the instrument. If the sphenoidal sinus was diseased, it could best be opened externally to the spheno-ethmoidal recess. He had used Moure's ethmoidal curette for two years, and found it a very useful instrument. He could completely exenterate the ethmoidal sinus forward to the frontal process of the maxilla within two or three minutes. He studiously avoided the middle turbinal; if it was grossly diseased, he deferred removing it until the end of the operation, when he could know his removal would be exact, and that he would not subsequently injure the region internal to it.

With regard to the method advocated, he would point out that in the great majority of cases the antrum was diseased in cases of ethmoiditis, and that he had almost invariably found that the antral mucosa was diseased, as well as the ethmoidal cells. By this method he could inspect the antrum, and to a great extent he could work with binocular vision. The fronto-nasal duct was cut across, the bullæ removed, and the frontal sinus if necessary was opened on the floor of the sinus. Another advantage was, that hæmorrhage could be completely controlled, and the whole area could be disinfected with B.I.P. after the operation.

His chief reason for advocating the method was that it had yielded him universally successful results; and it was a great advantage to be able to look into the nose and see all the turbinal bodies in position, and the nose appearing normal, in spite of the fact that the sphenoidal and ethmoidal sinuses had been exenterated.

Mr. Musgrave Woodman (in reply)

said he was obliged to Mr. Tilley for drawing his attention to the occurrence of osteomyelitis without operation.

In answer to Mr. Eric Watson-Williams as to his case of frontal sinus suppuration, it seemed that he had not tried Mr. Howarth's operation; if he did so, he would obtain a clear view into the sinus, and be able to remove localized patches of disease. If the whole was diseased, he must skin-graft the cavity. In another fifty years he did not think the Killian operation would be required at all.

He was obliged to Sir StClair Thomson for calling attention to the danger of meningitis in connexion with ethmoidal disease. He gave as his own experience, that in one or two cases of malignant disease he had exposed the dura of the anterior fossa freely, and in one case he accidentally cut it. Cerebro-spinal fluid came out. He stitched up the wound, put on iodine, saw that it was well drained, and the patient had no rise of temperature. It was also septic; one did not find malignant disease in the upper part of the nose without sepsis. But every care must be taken not to perforate the cribriform plate, or any part of the floor of the cranial cavity.

Dr. Syme's remarks about the danger of perforating the pituitary fossa were important.

He was indebted to Dr. Horgan for his description of his method of exenterating by the transantral route, which one was accustomed to take in malignant disease.

CORRIGENDUM.

Proceedings, vol. xvii, No. 8, June, p. 51, line 7 from bottom, for "'Mr. Banks-Davis'" read "Mr. Faulder's.'

Section of Laryngology.

President—Mr. H. J. Banks-Davis, M.B., F.R.C.P.

Case of Sarcoma, originating in the Alveolus.

By J. Aldington Gibb, M.B.

Patient, a female, E. H., aged 67. Home duties.

History.—Swelling of the upper jaw first noticed in September, 1923; more unsightly than painful, and its gradual increase in size caused the patient to come to hospital, November 21, 1923, when she complained merely of the swelling of the jaw, stating that it was not painful. No bleeding.

Examination.—Left nostril closed anteriorly with a papillomatous tumour, no bleeding on examination, no ulceration. Right nostril clear. On trans-illumination the left antrum was dark, with light trying to get round the sides. Right antrum clear.

Operation.—December 17, 1923. Under ethyl chloride and ether. An incision was made along the alveolar margin from the right canine fossa towards the left side extending for about 4 in. along the alveolar margin, and the flap was reflected upwards. A fairly large tumour was exposed which bled profusely. This was dissected away and was found to have four separate roots or attachments: (1) In region of right canine fossa, (2) in left canine fossa, (3) in left nostril (seen by anterior rhinoscopy), (4) in left maxillary antrum. The tumour was removed *in toto* as far as possible, and the resulting cavity packed. The tumour was sent for examination, and the pathologist's report was "round-celled sarcoma" (slide sent). Cavity was syringed out with H_2O_2 followed by saline. Patient got up on December 24, and Christmas dinner was taken comfortably on the next day. Sloughs were removed at varying intervals. She was discharged from hospital, clean, on February 10, 1924.

DISCUSSION.

Dr. W. Hill said it would be safe to apply radium, to make sure there were no little foci at the site. Sir Henry Butlin pointed out that some of these cases were not very malignant at first, though others were clinically very malignant. The degree of malignancy in any given case could not be determined beforehand.

Mr. Musgrave Woodman agreed it was desirable to have a section examined; he feared a recurrence on the lip; it was raw and tender, and that was a good site for the radium.

Mr. E. D. D. Davis pointed out that the area on the lip was a radium burn.

Mr. H. J. BANKS-DAVIS (President) said the Erlangen treatment was an excellent one for such cases. He had shown the Section a case of sarcoma of the antrum which was treated two years ago by this method after operation, and so far there had been no sign of recurrence. With regard to the usual terms employed, "dark" or "translucent" under transillumination, between these two extremes he thought the word "dim," as used by Mr. Woodman at the last meeting of the Section, as worthy of general adoption.

Dr. DAN MCKENZIE said he had shown two cases of the kind, one ten years after operation, the other seven years after. One need not be too anxious about these cases of sarcoma of the nose. If the tumour were completely removed and the patient watched, it could be kept from killing her. In the event of recurrence, it could be arrested by cauterization, as Price-Brown, of Toronto, had shown many years ago.

Dr. GIBB (in reply) said that the growth originated in the alveolus, and it was a round-celled sarcoma. The patient showed a recurrence in the alveolus, and a tube of radium was put in, which burned the lip. The antrum was quite clear of growth, and the nasal growth was completely removed. He afterwards wished he had removed the alveolus. He thought radium would quickly get rid of any recurrence.

Case of Carcinoma on Right Side of Tongue.

By J. ALDINGTON GIBB, M.B.

G. A. T., AGED 64. Pensioner.

History.—Swelling first noticed in June, 1923, as a small lump on the right side of the tongue near its middle. This began to enlarge, and he consulted a doctor in July, 1923. He was treated as a case of glossitis till October when he was sent to the Prince of Wales Hospital, Tottenham, London. There his blood was tested, and the Wassermann was apparently found to be negative, for no anti-syphilitic treatment was given. Also a small gland (submental) was excised and examined, and report was sent to the patient's doctor that the condition was inoperable. Eventually I was asked to see the case and so he agreed to another operation.

Examination.—An ulcer occupies the middle third of the side of the tongue, extending from the posterior pillar of the fauces to within a little of the tip, and also along the floor to the alveolus. Its floor is of wash-leather colour, its edges are overhung and irregular. Mobility is not impaired. Hard around and tissues infiltrated. Numerous small hard glands in sub-maxillary and submental regions, and some deep cervical glands can also be found, hard and movable. The ulcer is not movable and has been growing at a fairly rapid rate. No loss in weight noticed by the patient.

Wassermann reaction both on blood and on cerebro-spinal fluid is negative.

Operation.—Admitted November 24, 1923.

December 3, 1923: Wide excision of the ulcer by diathermy knife, and the surrounding tissue treated with the diathermy button.

Following this the tongue became very swollen, and patient had great difficulty in breathing for two days, after which rapid improvement took place.

December 18: Ordinary diet.

December 29: Discharged. All sloughs away.

January 26, 1924: Seen as out-patient.

DISCUSSION.

Mr. LAWSON WHALE said it did not seem clear from the notes what the diagnosis of this case was believed to be. Was it regarded as gummatous, or as a neoplasm? If the latter, it must be of enormous size. Nothing was said as to it being syphilitic, except that the Wassermann was negative.

Dr. DAN MCKENZIE said that if epithelioma of the tongue was to be removed by diathermy so that there should be no recurrence, it would have to be a formidable procedure. Ten months ago he removed the whole of the left side of a tongue for an ulcer on the margin, fairly far back. He first tied the carotid, and removed the glands and fascia at the side of the neck, treating the neck wound with diathermy. Even that removal was not wide enough, as recurrence took place on the other side of the neck. Removal of the glands seemed to limit the spread of epithelioma in that direction. The whole tongue should be removed by the diathermy knife; also the glands on both sides of the neck; then such cases only in this way could be attacked with some hope. Hitherto the ordinary surgical treatment of cancer of the tongue had been a failure, as recurrence occurred in all cases, sooner or later. In any future case he might have of this kind, he would aim at removing the whole of the disease-bearing area, doing two or three operations if necessary.

Mr. MUSGRAVE WOODMAN said that when serious growths in the mouth had to be tackled, the question of the glands in the neck must be faced, otherwise diathermy would be brought into discredit. Twenty years ago Sir Henry Butlin insisted on the necessity of removing all the glands on the side of the neck on which the tongue was affected. There was a good result in the present case, though Dr. Gibb did not show that it was epithelioma. There was still a hard gland under the jaw, and sooner or later that was likely to give trouble. In one or two cases in which he (the speaker) had removed the local disease successfully, two years later a gland "flared up," and within a month the case became inoperable.

Mr. J. B. CAVENAGH said this patient had had a considerable part of the tongue removed, yet there was little interference with speech, and articulation was still very good. After diathermy a softer scar was left than after operation with the knife.

Mr. W. IBBOTSON said he assumed that block dissection of glands was meant. There were many glands that were not palpable, and some that could not even be seen easily.

Mr. NORMAN PATTERSON said that one frequently found, even after removing the sterno-mastoid and clearing out the triangles, that there were glands behind the jugular and that structure had to be removed. Why, in this case, was a section not taken from the growth which was removed from the tongue?

Mr. H. J. BANKS-DAVIS (President) said the absence of palpable glands did not mean that no glands were involved. The actual enlargement felt was no criterion whatever that other glands were not in the process of involvement, and this should be borne in mind.

Dr. GIBB (in reply) said that the gland was removed by Mr. Howell Evans, and the case was found to be definitely one of carcinoma. It had been considered to be inoperable. The Wassermann reaction was negative. Carcinoma of the tongue was often said to rest on a syphilitic basis. He had found glands shrinking in similar cases and becoming hard. At Edinburgh, for instance, three weeks after diathermy, a gland was removed. The surgeon had intended to do a mass dissection, but on dissecting out the largest gland, three weeks afterwards, he found the cancer cells very much shrunken, and quite enclosed by fibrous tissue. He (the speaker) thought the intense heat used had let something loose in the tissues which had some effect on the lymphatic system, perhaps blockage, or had caused a secondary effect on the growth in the gland. He asked a surgical colleague to supply him with a carcinoma of the breast which he had excised, and then he got an analytical chemist friend to make an analysis under heat from the cancer cells. The chemist obtained a lipoid, and the speaker asked him

to look for lecethin lipoid; he obtained four grains of it from that carcinoma, and an extraordinary amount of fat, almost half a pound of liquid yellow fat, which did not give the reaction he would expect in normal fat. Therefore he got the chemist to make him some lecethin lipoid from an egg. Not long afterwards he saw another patient with carcinoma of fauces and pharynx, more extensive than in the present case; there was recurrence some weeks afterwards and treatment by lecethin lipoid injections was carried out, with the result that the ulceration had now completely cleared up. There was also a reduction in temperature—a result supposed to be due to the lecethin. In mixed infections, with carcinoma, he had given lecethin lipoid, with the result that the temperature had come down to normal, this bearing out the contention that lecethin raised the body immunity. The lipoid being found in the cancer cells, the probabilities were that lecethin lipoid was loosened from the cancer cells as a result of intense heat. He was now carrying out injections, and if any member would care to try the same thing after what he had said, he would be glad to know the result.

The theory he held was, that cancer was not a specific disease, but was an irritation-disease, i.e., an irritation could be set up by endogenous or exogenous poisons. In all the cases to which he had referred the patients had very bad teeth, and the last patient had had every tooth removed. The medical officer of health for the county of Kent said there had been an enormous increase of cancer there during the last few years. Lecethin lipoid was found in the suprarenal glands, and the action of it there was to raise body-temperature and to act on the katabolic side of metabolism, and there was some movement of tissue change. And if epithelial cells were to originate an aberrant form of growth as a result of irritation, it was not unreasonable to suppose that lecethin lipoid might in some way moderate the action of cell growth.

He held a further theory, namely, that as cancer now attacked people of a younger age than formerly—there were many cases in the thirties—it was probable that the endocrine glands became tired, and that the lecethin lipoid was deficient, and therefore that irritation of epithelial cells had a greater effect. When he was on active service his blood pressure was at first 130 mm., but in fifteen months it had increased to 178. It was not too much to expect that the period of the war had a bad effect on the resistance of the people.

Case of Occlusion of the Left Choana.

By Norman Patterson, F.R.C.S.

Patient, a male, aged 19. Came into hospital with acute right otitis media. Routine examination showed a complete occlusion of the left choana. The patient has been a mouth-breather as long as he can remember. The hearing in the left ear is apparently normal.

DISCUSSION.

Mr. Herbert Tilley asked whether this was a membranous or a bony atresia, or a combination of both. If the patient had been operated upon, what had been the method employed. He (Mr. Tilley) had had only three cases of the kind. The first showed the great tendency of the operated atresia to become blocked by granulution tissue, and how difficult it was to keep patent the newly formed posterior choana. In the second and the third cases he passed a chisel through the affected nasal cavity and chipped away the posterior region of the vomer, after having removed the congenital diaphragm of bone and membrane. In those two cases no after-treatment was necessary, as there was marked tendency for granulation tissue to proliferate and ultimately cicatrize.

Mr. Norman Patterson (in reply to Mr. Tilley) said that he had not operated upon the case, and he could not say whether the partition was membranous or bony. In two cases he had carried out the operation which had been mentioned by Mr. Tilley.

Mr. P. FRANKLIN showed some colour prints of unilateral occlusion of the posterior choana by membrane and bone. The membranous occlusion was coloured the characteristic blue grey, whereas the bony occlusion was a pale pink. He said that whereas the former type might be perforated in one or more places, the latter was generally intact.

Mr. H. J. BANKS-DAVIS (President) said that many years ago he showed a woman from the Middlesex Hospital who had such an occlusion. After repeated treatment it closed up. It always returned until she wore a rubber tube which was passed through the nose into the naso-pharynx, which enabled her to breathe with ease and carry out the necessary treatment.

Sir STCLAIR THOMSON said he had never had any difficulty about recurrence. With post-nasal forceps he punched out a considerable piece of the posterior border of the septum; the septum did not re-form. Mr. Wright read an interesting paper at Glasgow on this subject (published in the *Journal of Laryngology*, 1923), making several important observations to show that apparently these patients did not suffer from ill-developed chests, that their palates were not arched and they did not become deaf. The present patient heard well. He found deafness was very rarely associated with this. It was agreed that mouth-breathing might lead to laryngitis, but not all who were mouth-breathers had laryngitis, and people wearing a tracheotomy tube did not necessarily get bronchitis.

Mr. HERBERT TILLEY pointed out that it was partial rather than complete nasal obstruction which produced the mischievous effects on the ear and the skeletal development of the thoracic cage.

Dr. ALBERT A. GRAY expressed his agreement with Sir StClair Thomson; he said that the number of operations done for the relief of deafness was very great. Some thought that people with noses full of polypi did not get deaf, others that they did. He hoped that fewer operations would be done on the nose for the relief of deafness.

Mr. A. R. TWEEDIE said he noted that the otitis media was on the right side, whereas it was the left side of the nose which was occluded, but he suggested that it was this partial occlusion of the nose which led to nasopharyngitis and involvement of the tube. As regarded intranasal treatment, with a view to the relief of deafness, he considered such procedure should be limited to those cases in which there was some improvement after inflation. It was then fair to suppose that the condition of the middle ear could be improved by rectifying the intranasal affections; in these cases this rectification was as important as was dealing with adenoids for similar conditions.

Dr. W. HILL said he thought one reason these patients did not show the more obvious defects which would be expected was, that the obstructions were congenital, and those obstructions did not seem to exercise the same effect as when they occurred later in life. He himself had been puzzled at the contradictions encountered.

Mr. F. J. CLEMINSON asked whether there was any asymmetry in the facial development of this patient. He thought that one of the factors governing the symmetrical growth of the face must be that the nose should fulfil its physiological purpose and be used for breathing through, or otherwise it would not develop at the same rate as the rest of the face. He thought deformity of chest in young children with nasal obstruction was produced by the effort of the child to get air through the nose so long as it was possible. In cases of complete occlusion of the choanæ the child would appreciate the obvious impossibility, and so make no attempt at all; this chest wall would not in such a case be deformed.

Mr. J. B. CAVENAGH said it was extraordinary that, after a life-long absence of ventilation in the left nasal cavity, the quantity of secretion should not be cut down; it was apparently continuing all the time. Was the totally occluded side liable to infection?

Mr. J. F. O'MALLEY said he had a case in which the patient came on account of sepsis of the right side of the nose. She had extensive antral and ethmoidal trouble, and during the operation he discovered that she had an occluded posterior choana, which he regarded as responsible for it to a large extent. It was very difficult to get a satis-

factory result, and finally she ceased visiting the hospital before the end of the treatment; he had meant to open the posterior choana. One could not be dogmatic in a general way, but he thought that where it was found that there was marked deafness from time to time, associated with an exacerbation of nasal catarrhal trouble, or a cold in the head, if any obstruction discovered in the nose was removed, the hearing would improve.

Dr. P. WATSON-WILLIAMS said the question as to how far nasal obstruction was a cause of chest deformities and how far it was the cause of catarrhal ear complications, had not been settled. Mechanical obstruction of the choana was almost always unilateral, and therefore only partial, if the other side of the nose was normal. So long as there was no catarrhal condition there might be no chest deformity nor clinical manifestations of the obstruction other than intranasal. It was, however, a very different matter if infective catarrh became superadded. Hence the frequent occurrence of marked septal deflection in which one side of the nose was practically occluded, without resulting chest deformity, no difficulty in breathing, and no ear infection. Usually purely mechanical conditions were of small import unless there was superadded infection.

Mr. G. W. DAWSON said that it had not been stated why these patients sought relief. It was not for the nasal obstruction, but for the catarrh, which necessitated the constant use of the handkerchief. On examination the floor of the nose was found to be full of mucus. The bone causing the obstruction was frequently very thick, and the best method in these cases was to enter and remove the septum with a chisel ½ in. in front of the obstruction.

Mr. PATTERSON (in reply) said that this patient's face was symmetrical; he slept with the patent nostril uppermost.

Case of Lupus of the Palate and Face successfully treated by Artificial Light Baths.

By NORMAN PATTERSON, F.R.C.S.

PATIENT, a girl, aged 19. The trouble commenced when she was aged 9. Attended children's hospital, then Charing Cross Hospital. (? X-ray treatment.) Came to the London Hospital August 13, 1923. *On admission*: Butterfly scarring of face, with freckles and X-ray telangiectasis. Active lupus nodules; periphery of scarring; considerable nodular infiltrations of soft palate.

Blood-count natural; 90 per cent. hæmoglobin.

Light treatment. Mercury vapour light to whole body commenced August 16, 1923; fifteen minutes at first, gradually increased to one hour, continued daily for six months.

Present condition: weight 7 st. 7 lb.; face clear of lupus; palate now shows no sign of the disease.

Second Case of Lupus of Upper Air Passages treated by Radium.

By H. M. WHARRY, F.R.C.S.

PATIENT A. H., a boy aged 16. Had suffered for several years from lupus of the upper air passages. First seen by me in June, 1923.

Condition at that Date.—Nose: Lupus of right nasal fossa. *Fauces:* Lupus of uvula and anterior and posterior pillars of both fauces. *Tongue:* The

whole of posterior segment was covered with extensive heaped-up granulations. *Pharynx:* Some scarring. *Larynx:* Epiglottis and both vocal cords and ventricular bands affected, also inter-arytenoid region. *Voice:* Hoarse.

Has had several treatments with radium, and the whole condition has much improved.

DISCUSSION.

Sir STCLAIR THOMSON said that lupus appeared sometimes to get well under any treatment, or even under no treatment. He had sent cases in early days to the Radium Institute, but did not get one cure. Anything which produced good scar tissue was generally a permanent cure. He still believed nothing secured this so well as the galvano-cautery. Mr. Patterson's case was quite arrested under artificial light, but so was the other (Mr. Wharry's) under radium. These two cases should be shown again. He had had cases which remained well for many years and then broke down.

The cautery needed several sittings. It was useful, when the lupus was threatening to come through from the nose into the skin, or from the inner lip to the skin, to pass the cautery from the inside deeply; he had felt the cautery point almost coming through the skin. It produced fibrosis from the inner side and if thus used prevented the occurrence of external scar.

Mr. HERBERT TILLEY explained that Sir StClair Thomson's remark was the result of his being reminded of a case which he (Sir StClair) had once shown before the Section. The patient had very extensive lupus of the soft palate, pharynx and epiglottis but she got quite well after a stay of six months in the country and without local treatment of any kind.

Mr. NORMAN PATTERSON (in reply) said that for extensive lupus in the nose he obtained the best results from repeated curetting, rather than from the cautery.

Mr. WHARRY (in reply) said that the radium treatment of lupus was only in its experimental stage. The case he showed was very extensive, and there had only been sixteen applications of radium. The local condition had cleared up remarkably. The pattent was very hoarse, but now spoke fairly well. In a previous case, after four years of silence, the voice had returned in six weeks. He intended to continue to send cases of the kind for radium treatment. This patient was now in the reactionary stage. During this stage the radium picked out every half-healed little patch of lupus, and made it obvious, when otherwise it could not be distinguished by the naked eye. He thought it was a satisfactory form of treatment especially in restoring function.

Removal of Thyro-Glossal Duct and Fistula; Ten Years after Operation.

By DAN MCKENZIE, M.D.

THE patient is a girl now aged 16 years. The operation was performed when she was 6. It consisted in the removal of the fistulous opening and tract by dissection, the thick-walled duct being followed up to the hyoid bone, which was divided in the middle line. The skin incision was closed, it is to be noted, by the subcuticular suture, and primary union followed, the immediate scar being linear and inconspicuous.

As will be seen, the scar now is broad and ugly, and the operator confesses to disappointment. Obviously, what has happened is that the scar has not only kept pace with the natural growth in bulk of the neck, it has actually outstripped it. The moral seems to be that one ought to defer the removal of the thyro-glossal duct until adult life. After all, the deformity, even when a fistula is present, is but trifling.

Otherwise, the operation has been so far successful in that no recurrence has taken place. Apparently the plan of dividing the hyoid, which has been recommended for these cases, may be relied upon. It is curious to observe, however, that in none of the cases in which I have operated has it been possible to trace the duct upwards into the substance of the tongue, still less as far as the foramen cæcum. It appears to tail off and come to an end just behind the hyoid. At all events the thick duct wall disappears at this point, apparently blending with the periosteum of the bone.

Mr. H. J. BANKS-DAVIS (President) said he did not think there was much disfigurement in the scar. He referred to a case he showed of thyro-glossal cyst in a child, which he dissected out, closing up the wound with collodion. It healed by first intention but two days afterwards she had a temperature of 104°, and a very rapid pulse. He had learnt now that if the wound in these cases was stitched up, thyroid secretion would escape into the blood stream, with resulting thyroidism. In all cases a tube should be inserted for the first twenty-four hours.

Case of Epithelioma of Maxilla cured (?) by Radium and Diathermy combined.

By ROBERT WORTHINGTON, O.B.E., F.R.C.S.

Mr. A., aged 67, was first seen by me in October, 1922. He had had "nasal catarrh" for many years, and, for some months, discharge of blood and pus from the left nostril. He had recently had some very carious septic teeth extracted from the left upper jaw, and since then he had noticed a painful swelling in the left canine fossa spreading into the cheek.

He had obviously a suppurating left antrum, but I was uncertain if the swelling outside was inflammatory or new growth.

First Operation.—Intranasal antral operation. Tissue curetted from within antrum—section showed this to be granulation tissue. A piece of tissue was removed from the canine fossa—sections showed this to be *epithelioma.*

Second Operation.—In conjunction with Mr. D. Miller Muir I treated the tumour in the canine fossa by diathermy. In the resulting cavity 50 mgm. of radium bromide were inserted packed in gauze and left for twenty-four hours.

Healing uneventful. Outer wall of antrum and portion of alveolar margin separated as a sequestrum.

In January, 1923, a nodule of growth appeared in the cheek. This was treated by two radium needles containing 33 mgm. radium bromide, introduced from within and left *in situ* thirty-one hours; followed on the next day by a dose 2/3 B. of X-rays filtered by 3 mm. aluminium.

This combined treatment resulted in a mild erythema of the skin as seen three weeks later. Six weeks after the treatment the recurrence had completely disappeared.

So far there has been no further recurrence.

Case of Syphilitic Cranio-tabes, simulating Frontal Sinus Disease.

By J. ALDINGTON GIBB, M.B.

PATIENT, male, aged 15.

History.—Swelling over right frontal region, lanced by his doctor several

weeks ago. Also small fluctuant swellings under scalp in vertex and occiput. Patient had been told he had tuberculosis. Had an operation for removal of glands of neck seven years ago.

Family history.—Mother: one miscarriage; one child dead (breech presentation); one child healthy.

Condition on admission.—Ears: normal. Nose: sunken bridge. Throat, normal. All other systems, normal.

February 18, 1924.—Wassermann reaction: strongly positive.

Treatment.—February 19, 1924: Neo-kharsivan, 0·3 grm.; mercury inunctions; potassium iodide, gr. viij, t.d.s., p.c.

February 28, 1924.—Swellings smaller; less fluctuant.

March 6, 1924.—Neo-kharsivan, 0·3 grm., continued.

March 7, 1924.—Temperature, 102·4° F.

March 17, 1924.—General condition greatly improved. Frontal and occipital swellings now almost disappeared and barely palpable. Neo-kharsivan, 0·3 grm.

April 11, 1924.—Neo-kharsivan, 0·3 grm.

April 13, 1924.—Contramine, 0·25 grm.

Congenital syphilis of bone is not rare, but the particular form in which it appeared in this case rendered confusion with frontal sinus disease easily possible.

The case was first treated at the Ramsgate Hospital, and the incision seen between the superciliary ridges was made there.

The actual condition when seen by me was as follows:—

Well marked swelling of superciliary ridges and glabella, with some superficial fluctuation. At the margin of the hair in the middle line was a small fluctuant swelling, and similar conditions were present in the neighbourhood of the anterior and posterior fontanelles. The incisor teeth were not notched, but were not wedge-shaped and suggested syphilis.

At first sight the condition might have been mistaken for one of frontal sinus disease, but my original diagnosis was syphilitic cranio-tabes, and the results of treatment have amply supported it.

Mr. J. F. O'MALLEY said he had a case with swelling over the frontal sinus and disease in the nose. On incising the frontal sinus he came upon diseased bone, and he drained the frontal sinus. There was no satisfactory result over some weeks. Then he had the blood tested, and it gave a positive Wassermann reaction. On putting this man on to iodide of potassium treatment, the condition cleared up.

Case for Diagnosis.

By CYRIL HORSFORD, F.R.C.S.

PATIENT, a male, aged 57, by occupation a carman.

History of hoarseness for one and a half years. No loss of flesh.

On examination.—Larynx shows irregular thickening of the right vocal cord with a granulomatous mass over posterior part of cord and an excavation or ulcer anteriorly. Left vocal cord mostly congested, but on apposition fits into the irregularity of the opposite cord.

Patient was sent to me without complete report from the Tuberculosis Department, St. Pancras Dispensary.

DISCUSSION.

Mr. H. J. BANKS-DAVIS (President) said that there was obvious subacute laryngitis, quite apart from anything else.

Mr. LAWSON WHALE said he had obtained a good view of the case, and saw a definite sessile granulomatous tumour projecting from the middle of the right cord, something like the granuloma seen in tuberculous laryngitis.

Dr. P. WATSON-WILLIAMS thought the case suggested diffuse pachydermia laryngis. He considered there was a definite humping over the anterior part of the left vocal process, and on the opposite side a deep notching of the vocal cord. Against that diagnosis was the appearance of a cicatricial band or adhesion between the anterior sixth of an inch of the vocal cords in front, suggesting a previous ulceration. He suggested a Wassermann test, and asked if the case might be seen again. It might be the late result of syphilis, or some other ulcerative condition.

Dr. L. POWELL said he saw the humping mentioned, and thought it might be exaggerated by the cord being missing in the posterior part. Recently he had had a similar case, and had nipped a piece off the hump; this relieved the breathing. He had had sections cut, which were found to consist of fibrous tissue. The present case he considered to be syphilitic.

Mr. CYRIL HORSFORD (in reply) said that he saw the patient for the first time six days previously, and there was no evidence of tuberculosis. He thought there must have been some disease of the vocal cord previously, as there was a history of eighteen months of hoarseness; he did not suppose it was a growth during all that time. In the anterior part of the right cord there was an excavation, and in the posterior part corresponding, a firm and distinct swelling. The excavation was evident when the cords were opposed, and the right cord was much infiltrated. The congestion and slight irritation seemed to be due to the irritation of the lump on the opposite side. He thought it was a case of growth superimposed on chronic inflammation. Tuberculosis caused more destruction of the voice than this man showed: and malignant disease would produce a harder voice than the patient had.

He would have a Wassermann reaction done, and show the man again, carrying out no treatment in the meantime.

Postscript.—Since the above meeting the following notes have been obtained: "Wassermann reaction negative. Slight evidence of phthisis both apices. Tubercle bacilli in sputum."

Section of Laryngology and Section of Otology.

COMBINED SUMMER MEETING.[1]

Chairman — Mr. H. J. BANKS-DAVIS (President of the Section of Laryngology).

DR. A. A. GRAY gave a demonstration of "Stereoscopic Lantern Slides for Teaching Purposes."

Tortuosity of the Internal Carotid in Relation to the Pharynx.

By A. BROWN KELLY, M.D.

(ABSTRACT.)

THE internal carotid occasionally appears in the lateral part of the pharynx as a pulsating bulging. It causes no symptoms. The writer pointed out years ago that it was due to tortuosity of the artery and not to an aneurysm, an enlarged ascending pharyngeal or aberrant vertebral artery.

For statistical purposes notes were collected of cases in which the vessel projected into the pharynx (eighty-five cases), and of those in which it produced visible pulsation but no change in the contour of the part (sixty-five cases).

Of these 150 cases, fifty-nine were males and ninety-one females. It was found that prominent vessels were commoner in women than in men (fifty-six females and twenty-nine males); that they were met with mostly in the young (three children were under 5 years, and altogether twenty-seven were under 15 years) and in the aged (twenty-two subjects were over 60 years): and that the condition was bilateral in one-third of the cases, and when unilateral it was oftener on the right side.

It was suggested that the tortuosity might be explained by the mode of development of the internal carotid. This artery originates from the third aortic arch and the dorsal aorta and at the junction of these two component parts a pronounced bend is formed. Normally the artery is straightened out, but if this is incomplete, undulation or tortuosity will persist. In support of this view sketches of dissections were shown, in all of which the first bend of the tortuosity was mesial or forward, the direction thus corresponding to that of the bend in the embryonic vessel; it was also pointed out that the ninth nerve crossed the bend in the embryo while in the adult it crossed the artery at the commonest site of tortuosity; and that tortuosity was frequently bilateral and thus suggested a common cause, which in the young was likely to be of a developmental nature. In advancing life, as in other arteries, there is a natural tendency to tortuosity of the internal carotid, and the fact that women were more prone to this change than men seemed to be in accordance with the view that a relaxed artery favours the development of dilatation and tortuosity although the blood-pressure is habitually low.

DISCUSSION.

Mr. MARK HOVELL said he had never seen a swelling of the size shown in the author's illustration, therefore he concluded he had not seen a tortuous internal carotid; but he had many times seen a small vessel coursing on one or both sides

Note.—The papers unless otherwise stated are published *in extenso* in the *Journal of Laryngology and Otology*.

internal to the posterior pillar, which he had thought to be the ascending pharyngeal, passing in front of the constrictor, instead of behind it. Once he was sent for in order to see a patient who was said to have an aneurysm, but he found it was simply a case in which this artery was in front of the muscle, instead of behind it.

Mr. E. D. D. DAVIS referred to two cases similar to the one now described by Dr. Brown Kelly, in neither of which there had been any symptoms. The abnormal carotids were found accidentally. And there had been two cases in which peritonsillar abscess had caused severe hæmorrhage. One boy nearly died of the hæmorrhage from such an abscess. He developed hemiplegia, and twelve months later the tonsils were removed with a sharp guillotine; and at 14 years of age Mr. Trotter removed the aneurysm of the internal carotid. The other case was reported by Mr. A. J. Hutchison, of Brighton; a peritonsillar abscess was punctured, and the patient died of hæmorrhage. Sébileau published in a French journal a paper quoting eight cases of fatal hæmorrhage from the internal carotid, and in each case punch forceps had been used for removing the tonsil.

Mr. W. IBBOTSON asked whether the walls of the vessels in this case were diseased. Dr. Brown Kelly classified cases according to whether there was pulsation or prominence; he (the speaker) took it that if a vessel were prominent it would be pulsating.

Mr. F. W. SYDENHAM said that fifteen years ago he operated on a posterior and inferior turbinate with curved cutting forceps, and half an hour afterwards the patient bled very profusely. He recovered, and it was thought that a small branch had been cut. A week later there was again profuse bleeding from the nose, so that it had to be packed. A week later still there was again furious bleeding, therefore he tied the facial and the ascending pharyngeal. But at the end of a week there was so much bleeding that the patient became blanched. He then tied the common carotid, and after that the patient did well. Nine months later the ligature came away through the scar, and the patient had remained well ever since. He thought the original operation must have damaged an abnormal internal carotid.

Dr. BROWN KELLY (in reply) said he thought there would be more danger from working in the naso-pharynx than in the pharynx. In the second anatomical specimen the artery high up formed an angular projection, which would be visible only in the naso-pharynx, where the use of such an instrument as Loewenberg's forceps would be dangerous. With regard to the distinction between pulsation and prominence, a prominence was observed where there was persistent bulging, whereas in pulsation there was a temporary bulging during systole only. It was conceivable that all these cases were congenital, or that there was present a congenital factor, which in older people had probably lain latent.

Mr. H. J. BANKS-DAVIS (Chairman) asked whether Dr. Brown Kelly thought these pulsating vessels at the side of the pharynx were always the internal carotids and never the ascending pharyngeals.

Dr. BROWN KELLY (in reply) said that that was his opinion.

Mr. BANKS-DAVIS said that this statement, coming from Dr. Brown Kelly, was very important.

Throat and Ear Defects from the Standpoint of the Elementary School Child.

By ELEANOR LOWRY, M.B.

(ABSTRACT.)

THE author described some of the difficulties under which School Medical Officers have to examine the children in schools and treat them at their minor ailment centres. Quoting from the last report of the Board of Education, she said that no less than eighty-nine education authorities in England and Wales had no provision for the treatment of tonsils and adenoids, and only five towns

were mentioned as having aural clinics under the care of specialists. She urged the need that existed for early expert treatment for throat and ear defects especially for otitis media, which if properly treated at first would save so much unfitness and waste of life in later years.

DISCUSSION.

Mr. J. S. FRASER asked why these children were not more thoroughly examined in the schools; surely the singing could be stopped during the ear examinations. Hearing tests could be carried out by the school teachers if the room were kept quiet, one ear being occluded while the other was examined, and the child turned sideways to obviate lip-reading. Deafness in children was frequently stated to amount to only 1 per cent. or 2 per cent., but he believed the percentage to be much higher, as about 5 per cent. of children had otorrhœa. Otorrhœa might not always cause severe deafness, but no child with a running ear had normal hearing in that ear. Those who had to make these examinations ought at least to be provided with proper instruments for examining the oral pharynx, nose and ear. Much of the nasal obstruction which was attributed to adenoids was due to enlarged turbinals.

Sir JAMES DUNDAS-GRANT said that work such as Miss Lowry's would be of great benefit to the children who were the men and women of the future. He was afraid, however, that minute examinations might reveal too much, and that undue significance might be attached to minor defects. Still, school doctors were trained to discover essentials, and much good was done in these school clinics.

Mr. DOUGLAS GUTHRIE said that, as he was connected with a school clinic, he appreciated the difficulties of the school medical officer. One of those difficulties was that of getting the family doctor to do anything in the matter: either to operate, or to send the case to someone who would do so. Nevertheless, months after, the adenoid child seemed to be better. Here was one piece of research which those connected with schools could do; they could follow up these cases and find out what became of them. Sometimes children were seen who obviously, from the specialist's point of view, required a mastoid operation, but nothing was done, and after adopting conservative treatment everything cleared up.

Miss LOWRY (in reply) said that many cases were brought forward by school teachers. Her strictures were not only directed to London, where the conditions were better than elsewhere, and were improving yearly, but there must be many children whose ear condition was overlooked by the teachers, and a child might hear well with one ear, but be stone deaf in the other. Following up cases was complicated if one had to send a case away for transillumination or for a mastoid operation. The best clinics were those attached to hospitals, or clinics in which the work was done by those holding hospital appointments, who could take in children themselves.

Displacement of Antro-nasal Wall in the Treatment of Atrophic Rhinitis.

By W. S. SYME, M.D.

(ABSTRACT.)

THE method of displacement employed is the following:—

The antrum is opened by way of the canine fossa. The opening is enlarged downwards and forwards so as to obliterate the anterior angle. With a chisel the antro-nasal wall is divided along its lowest part, and then at right angles to this anteriorly and posteriorly. The anterior limb is made well forward, even beyond the limit of the external antral wall if necessary. The nasal mucosa is

not perforated. With a periosteum elevator or with a pad of gauze the wall is mobilized and pressed inward so as to bring it into contact with the septum. It is kept in this position by firm packing on the antral side. The packing is changed every two days for ten or fourteen days. The nasal cavity is douched daily and a formalin spray is used.

Twenty-three patients were operated upon, twelve bilateral, six left-sided, five right. Sixteen were females, seven males. The ages ranged from 40 to 8. Eight patients were under 20. In the series of thirty-five antral cavities evident antral disease was present in twenty.

Results.—Sixteen of the twenty-three patients returned for examination. In fourteen cases there is neither crusting nor fœtor. In one there is still crusting and fœtor after two months. In one there is slight fœtor but no crusting after six months. In seven there is still some fluid discharge, in nine there is not. Eight patients state that the sense of smell has returned

DISCUSSION.

Mr. W. IBBOTSON asked whether there was a defective septum in the unilateral cases. If so, would not submucous resection have diminished the patency on the atrophic side, without doing the more risky operation on the submaxillary sinus?

Mr. CYRIL HORSFORD said his procedure was to split the septum and push it over, and he found it did much good. After this had been done, one patient remained free from ozœna. In unilateral atrophic rhinitis in which there was a deviated septum, he suggested that the bones and cartilages should be divided and the septum replaced in the new position, making a cavity where the convexity had been, not resecting any bone.

Mr. W. H. HOWARTH said that Dr. Syme's procedure seemed to give extraordinarily good results. The high percentage in which he found suppuration in the antrum was very interesting, and possibly threw some light on the ætiology of the condition. Probably the commonest cause of atrophic rhinitis was suppuration in one or more of the accessory sinuses, often persisting from childhood, and Dr. Syme's statistics supported this idea. If the ethmoid were explored in these cases, evidence of suppuration would probably be found there. He intended to give this operation an extended trial.

Sir JAMES DUNDAS-GRANT said that he was interested in the results of this somewhat radical treatment. Lautenschläger had been credited with its introduction. A modification which seemed to obviate the necessity of continuous plugging was one an account of which he had abstracted in the *Journal of Laryngology*. The antrum on each side was opened and a strong wire pushed through from one antrum into the other by way of the septum, then it was turned round and pushed back in such a way that the knot was in the nasal fossa. He had, however, not tried it.

It was necessary to classify ozœnas; they differed probably according to the kind of suppuration giving rise to them. Many cases were secondary to sinus disease; some had no association with it. There should be considerable activity in diminishing the size of the nasal cavity; this could be done in various ways. The effect of stimulating it by the submucous injection of small quantities of paraffin made it appear that there was some dynamic action brought into play. Another means was the introduction of a small piece of the person's cartilage under the mucous membrane of the septum.

Sufficient attention was not given to the action of douches on the nose. If hypotonic solutions were used they caused swelling of the mucous membrane. The use of plain water to which a little ether had been added acted well in soms cases by causing such swelling.

He said that Mr. Charles Parker in a paper on ozœnas stated that in some unilateral cases there was extreme deflection of the septum, the fetid crusts forming in the wide hollow on the concave side of the deflection. In these cases the cartilage of the septum should be resected, turned round and replaced; this seemed to be an ideal method of dealing with them.

Dr. BROWN KELLY congratulated Dr. Syme on his results. He believed in unilateral ozœna, but not in unilateral atrophy. When an atrophic process was present in the nose it affected the narrow as well as the wide side, but ozœna might be confined to the wide side. Therefore Dr. Syme was justified in not doing a resection, but rather in trying to reduce the width of the open side. There were two difficulties, however, to which Dr. Syme had not referred, viz., the thickness of the walls of the antrum often found in atrophic rhinitis, the cavity apparently not being fully developed; and the great discomfort to the patient caused by the application of dressings for ten days.

Dr. A. A. GRAY said he agreed that there were different kinds of ozœna. One was the hereditary kind, which passed from parent to child, generation after generation. He thought that differed from the type due to an infection. It would be well worth while having a special discussion on ozœna.

Dr. LOGAN TURNER said he agreed with Dr. Syme that there was not, as a rule, unilateral atrophy. He (the speaker) had been surprised that such a large number of cases had associated sinus disease; Grünwald had held the view that in all cases of ozœna there was a focus of suppuration. He also endorsed Dr. Gray's suggestion that it would be useful if the Section discussed the subject of ozœna.

Mr. H. J. BANKS-DAVIS (President, Section of Laryngology) said that in the case of an atrophic rhinitis associated with antral disease, obviously, opening up the antrum and leaving a large opening ought to make the condition worse. He did not think the disease could be much associated with narrowness of the nostril, because often it occurred in young girls with normal noses.

Mr. SYDNEY SCOTT asked Dr. Syme how he excluded sphenoidal sinus infection; he (the speaker) found it difficult to say whether there was no sinus infection in a particular case. He was sure infection of the sphenoidal or ethmoidal cells could be overlooked. Did Dr. Syme trust to X-rays as an aid?

Dr. J. ALDINGTON GIBB said that twenty years ago a patient came to him with atrophic rhinitis, and both sphenoids were found to be affected. He opened and drained both thoroughly. He saw the patient this year, and the condition of the nose had then greatly improved; there was still some atrophy, but the nose was moist and free from crusts.

Dr. SYME (in reply) said he had treated a fair number of cases by pushing the septum over to the other side by means of Asche's operation. In some he found the inferior turbinal atrophied on the narrow side and ozœna might occur from this procedure. He would be glad to hear, later, what experience Mr. Howarth had had with the procedure which he had described in his paper. He did not claim that the method was his own, but the important point was that the whole antral wall was displaced. He did not see why one should not operate on the whole antral wall. Ethmoids had been a difficulty. He thought many cases of atrophic rhinitis were secondary to sinus disease. In cases in which there was ethmoid disease and discharge still remained, he intended in future to add the ethmoidal operation. He feared that in passing a wire, as described by Sir James Dundas-Grant, there was danger of perforating the septum. He did not see how one could keep the displaced wall in position by inserting cartilage. It would certainly be destroyed. Some form of easier packing might be devised. Ozœna was a very distressing and non-social condition, and for its cure patients would endure almost anything.

Cartilaginous Tumours of the Larynx: A Study of all the Cases Recorded.[1]

By Irwin Moore, M.Ch.

(ABSTRACT.)

These tumours are so rare in connexion with the larynx that after the most thorough search of the literature only fifty cases can be found recorded since they were first described by Heusinger in 1822. Only two specimens of chrondroma of the larynx can be found amongst all the London hospitals and museums, one in the Hospital for Diseases of the Throat, Golden Square, and the other in the Museum of the Royal College of Surgeons. During the period of sixteen years since the formation of the Section of Laryngology, only one case has been exhibited (by Lambert Lack and Stanley Green in 1908). At our clinical meeting I am exhibiting a further and post-operative case of a medical man, aged 55, who consulted me in August of last year.

Terminology.—Heusinger (Eisenach), in 1822, described these neoplasms as "chondroides," but since at that time no proper classification existed, various kinds of growths, some having only a cartilaginous hardness, such as fibromata, and even carcinomata were indiscriminately grouped with true cartilaginous tumours. Müller (Berlin), in 1836, was the first who attempted to correctly define these groups from their histological aspect, and he adopted the term "chrondromata," or "enchrondromata" for the whole group. Virchow, in 1863, recognizing the deficiency of this title—from the point of view of the origin of such tumours—adopted the term "chrondromata" as representing the entire group, and further subdivided these into "ecchondroses," or "ecchondromata" and "enchondromata." He limited the term "ecchondroses" to growths of true cartilaginous structure, purely hyperplastic in character and arising from an *already existing* or primitive cartilage, and confined the term "enchondromata" to tumours which were *not produced from existing cartilage*, but arose from a non-cartilaginous matrix. This classification was also approved and supported by Cornil and Ranvier in 1869, and it has held good to the present day.

Statistics.—Amongst the large number of laryngeal growths referred to in the monographs of the earlier writers up to 1876, viz., Bruns, Elsberg, Stoerk, Schnitzler, Tobald, Oertel, Schroetter, Hopman and Fauvel, not one case of chrondroma was recorded. Semon also, in his statistical tables of 10,747 benign growths, collected between 1862 and 1888, does not mention one case. Alexander (Berlin) in 1900 compiled from the literature twenty-seven cases of cartilaginous tumours of the larynx recorded up to this date, and added two of his own. Mansfeld (Königsberg), in 1909, added a further nine cases, whilst New (Rochester, Minnesota), in 1918, brought the number up to forty-seven.

It is said to be characteristic of a cartilaginous growth springing from one of the laryngeal cartilages that it extends inwards into the larynx and obstructs the air-way. Morell Mackenzie, however, recorded a case in which the tumour was situated partly inside and partly outside the larynx, and extended only in its external portion. Also, other cases have been recorded by Rosenberg and Waggett. To these may be added the case I am exhibiting at

[1] This paper when published *in extenso* (see footnote p. 1) will include illustrations and abstracts of all the cases recorded, from a study of which the symptoms, diagnosis, prognosis and treatment of these neoplasms have been summarized.

our Clinical Meeting, in which the growth originated from, and was confined to the outer surface of the thyroid cartilage.

In a case recorded by Ehrendorpher (Wien) the growth was situated on the pharyngeal surface of the cricoid cartilage, and interfered with deglutition.

[From a thorough study of all the cases recorded, the ætiology of these tumours, their histology and laryngoscopic appearances were summarized, together with their symptoms, prognosis and treatment.]

In conclusion I would add the opinion of Virchow on the treatment of these tumours: "We must dispose of the theory that all these enchondromata are benign; they have either in bony parts, or in soft parts infective properties, and should be extirpated when tangible as soon and as completely as possible."

[A large number of drawings, specially prepared to illustrate this paper, were exhibited on the epidiascope. These included laryngoscopic views of the most notable of the cases recorded, also the origin of the tumours as demonstrated after operation.]

Ecchondrosis of the Larynx: Records of Two Cases successfully treated by Laryngo-fissure.

By Sir StClair Thomson, M.D., F.R.C.S.

(ABSTRACT).

Cartilaginous growths are reckoned among the rarest of laryngeal tumours. As late as 1900 the records of only twenty-seven cases could be collected (A. Alexander), and in several of these the nature of the growth was doubtful.

Laryngo-fissure is indicated for most forms of intra-laryngeal cartilaginous growths when the condition is diagnosed early.

More than fifty years ago Virchow indicated the special site of these growths—towards the cavity of the larynx—and pointed out the impossibility of their removal *per vias naturales* owing to their thickness and hardness.

Two cases of ecchondrosis of the endo-larynx are reported; the nature of the growth was confirmed after laryngo-fissure.

Case I.—F., aged 53, first came under author's observation in 1901. Her condition was diagnosed as one of subglottic new growth of uncertain character, probably innocent. Patient advised to see author from time to time. She failed to present herself for several years, returning for consultation in 1909 on account of severe dyspnœa, having only suffered from occasional laryngitis during the eight years' interval. This was relieved by immediate tracheotomy and the growth was removed by the author by laryngo-fissure two days later. At the operation the muco-perichondrium covering the tumour was reflected and afterwards replaced. Tracheal cannula left in neck.

For some days following operation oral respiration impeded by return of subglottic swelling. This gradually subsided and use of cannula discontinued three weeks after operation.

Patient fully relieved by operation and died over eight years later from another cause (intestinal hæmorrhage from duodenal ulcer).

Case II.—Patient, a female, aged 56, shown before Section of Laryngology by Dr. Stanley Green and Mr. Lambert Lack, in 1908, as a probable case of tuberculous tumour of larynx. Later she underwent operation of laryngo-fissure undertaken by a general surgeon. The tumour was incompletely removed and the muco-perichondrium was not raised and afterwards replaced. Patient again shown before Section of

Laryngology (1909), relieved of stridor but still suffering from atrophic laryngitis. Nine years later underwent low tracheotomy for relief of dyspnœa and stridor, but died forty-eight hours later. No post-mortem.

Diagnosis.—Difficult on account of: (1) rarity of tumour; (2) subglottic position; (3) tendency to simulate malignant growths. Reference is made to a case reported by Mr. E. B. Waggett (*Journal of Laryngology and Otology*, 1921, xxxvi, p. 338) previously diagnosed as syphilitic and treated accordingly, which eventually proved to be ecchondroma of larynx.

DISCUSSION.

Mr. E. D. D. DAVIS said he had assisted Mr. Waggett at both operations in the case referred to. It was an enchondroma of the cricoid, and was encapsuled. Mr. Waggett attacked it from outside the neck, by means of a long incision along the anterior border of the sterno-mastoid, exposing the tumour thoroughly. He then enucleated the tumour, taking care to preserve the mucous membrane of the inner surface of the cricoid, in order to prevent stenosis at a later date. Unfortunately, however, the tumour burst, and he scooped out the sago-like granules from the capsule. It was not cystic. The patient did well for two or three years, then the tumour recurred, and the operation was repeated, the tumour with its capsule being removed complete. In these, as in mixed parotid tumours, it was essential to remove the tumour and its capsule, and it was in those cases in which a complete removal with capsule intact had not been effected that a recurrence occurred sooner or later. The patient was in good condition, he had some voice and a fairly good airway.

Dr. P. WATSON-WILLIAMS said he believed the case published by Mr. Waggett was the one he (the speaker) had seen before and had not diagnosed correctly. When first seen by him there was nothing which suggested any kind of growth, there was fixation of one vocal cord, some subglottic swelling, and apparently some perichondritis, possibly septic, possibly syphilitic. On two separate occasions the Wassermann test proved weakly positive, though nothing in the history suggested infection. The improvement following anti-syphilitic treatment was so marked that he allowed the patient to go abroad and resume his duties on the Continent. Some months later the patient had such acute dyspnœa that tracheotomy had to be done. He was subsequently seen by Mr. Waggett, who found this enchondroma. This showed that the onset of enchondroma in the larynx might be very insidious; in this case it took some years to become obvious, for before coming under the care of Dr. Watson-Williams, the patient had been under Sir Charters Symonds for some time, and the laryngeal symptoms had been noticed long previously.

Dr. IRWIN MOORE (in reply) said that Dr. Watson-Williams saw the patient in 1915, whereas Mr. Waggett did not see him until 1921. Sir Charters Symonds had previously seen the case at the end of 1914, and no definite cause of the paresis could be found.

Sir STCLAIR THOMSON (in reply) said that practitioners might oftener endeavour to persuade patients to submit to an exploratory laryngo-fissure.

Methods of estimating the Activity of the Ciliary Epithelium within the Sinuses.

By A. LOWNDES YATES, M.D., F.R.C.S.Ed.

(ABSTRACT.)

THE author gave an account of the investigation of the function of the ciliated epithelium within the nose both in health and in diseased states.

Using lamp black as an indicator, he observed that by the action of the ciliated epithelium the mucus was rolled over and over, thus enveloping all the foreign particles. Using 0·4 per cent. indigo carmine in normal saline solution

at body temperature, he found that the coloured fluid appeared to follow a definite path as follows: It passed along the upper part of the middle meatus, then outwards and downwards, eventually following a definite tract in front of the Eustachian tube. From this situation it passed downwards between the attachment of the soft palate and the posterior wall of the pharynx, and, continuing, passed as a definite stream which lay just behind the posterior pillar of the fauces. But from this part of the stream some of the fluid passed in all cases anterior to the posterior pillar of the fauces, and found its way into the recess between the tonsil and the posterior pillar.

At the level of the dorsum of the tongue, the main stream divided into a larger and a smaller portion. The smaller portion was conveyed across the dorsum of the tongue in the position of the lingual tonsil, and from thence in some cases it passed into the glotto-epiglottidean pouch, and in other cases was swallowed directly. The larger portion continued outwards along the posterior margin of the pillar of the fauces, and thus reached the sinus pyriformis and continued down this to the posterior aspect of the arytenoids, finding its way into the inter-arytenoid space.

In one abnormal case, where the mucus was stained and where its progress was slow, it found its way into the trachea through the inter-arytenoid space. The bulk of it remained in position for a short while behind the arytenoids, and was then swallowed.

When placed in the posterior ethmoidal sinus, the fluid passed in the superior meatus, and escaped from this into the naso-pharynx and was conveyed outwards for a short distance until it occupied a position at the upper part of the naso-pharynx, immediately in front of the uppermost part of the Eustachian tube. Here it divided into two streams, a larger, which passed downwards and joined the position of the stream from the antrum, and a smaller, which passed over the Eustachian tube, and then coursed downwards through the fossa of Rosenmüller, and then curving forwards, joined the antral stream at the level of the lower border of the soft palate. The further course of these streams was as that described for the stream from the antrum. If the fluid was injected into the sphenoidal sinus, it passed from the opening of the sinus in a downward direction until it reached the highest point of the posterior choanæ. Here it turned backwards, and, spreading out like a fan upon the vault of the naso-pharynx, was conveyed in a series of streams, the outer of which made its way into the lower part of the fossa of Rosenmüller. The inner streams passed in an outward direction until they joined together in the position of the antral stream, about half an inch below the lower margin of the soft palate, and the further course of the fluid was as that described for the antrum.

The ciliary action in cases of sinusitis was somewhat different.

When mucus or pus is present in any of the sinuses the rapidity of the flow of the indigo carmine solution may be greatly modified. In cases where the contents of the sinus are mucus only, the rate of flow is little altered. In cases where thick gelatinous pus is present in the sinus, there is some slowing of the stream, but this is slight. In both the above cases the sinus contents are not stained by the fluid which is injected, and on washing out the sinuses the condition generally does not tend to recur.

In cases where the sinus content consists of thin green pus, two noticeable alterations are seen. The pus stains blue, but not as a rule deeply. The fluid is conveyed away along the track from the sinus with considerable sluggishness

and strays from this, and tends to pass over the areas where contrary ciliary streams meet, as for example at the edges of the Eustachian cushion, or at the posterior part of the larynx. In well marked cases the mucus membrane appears to stain, but it is found that this stain can be wiped off, and consists of a thick layer of muco-pus.

In cases of sinus infection of the atrophic type, no pus is found within the sinus, but the fluid contains micro-organisms and débris in some quantity. The fluid is conveyed away very sluggishly, and there may be complete stasis of the fluid within the sinus, and the part of the track nearest to the sinus may appear to be deeply stained, and this stain may remain in position for some hours unless washed away by douching. In two cases in which 15 c.c. of 0·4 per cent. indigo carmine were injected into the antrum, and where this marked stasis was present, indigo carmine appeared in the urine in three-quarters of an hour, and a similar quantity of indigo carmine swallowed at a subsequent visit did not appear in the urine.

Clinical Aspect.

Cases in which the mucus was unstained showed only local symptoms. Cases in which the mucus was stained and ciliary paralysis was partial, showed remote local symptoms, such as laryngitis. Cases in which the ciliæ were apparently totally paralysed, and the secretion of mucus was absent, and micro-organisms present, showed in some cases remote symptoms such as arthritis, but in well-marked cases of this type of sinus infection with atrophy, signs of sinusitis, as revealed by ordinary methods, were scanty.

The ciliary activity may be estimated more simply by using liquid paraffin to wash out a sinus on one occasion, and saline on the next occasion, when the amount of liquid paraffin remaining in a sinus forms some measure of the ciliary activity.

Conclusions.

It would appear, therefore, that the severity of a sinusitis or sinus infection depends upon the power of the mucus to prevent its destruction by the inspired micro-organisms, and if the property that mucus possesses of preventing the passage of water, and hence of water-soluble toxins, is destroyed, that a progressively increasing poisoning by these toxins of the ciliated epithelium occurs, and that, as this ciliary paralysis increases in amount, the secretion of mucus is also paralysed until finally the micro-organisms come to dwell in a symbiotic state in the sinus, and the toxins from them are readily absorbed, with the production of remote symptoms.

DISCUSSION.

Sir StClair Thomson said that it was excellent research work like that of Mr. Yates which raised the reputation of their specialty, and added much to the knowledge of the physiology of the body generally. He (the speaker) referred to his own work on ciliated epithelium undertaken in conjunction with Professor Hewlett thirty years ago when they found that the pharynx of the frog was lined with it. They pithed the frog and having opened up its pharynx, a little piece of charcoal was placed on the ciliated epithelium, and its movement timed. It was moved along at the rate of one inch per minute, so that in two minutes intruders would be expelled from the nose into the vestibule. They also planted *Bacillus prodigiosus* on one another's nasal mucosa, and timed to see how long it took to disappear. Dr. T. S. Kirkland of Sydney, who was a visitor at the Congress, had found the accessory sinuses sterile in animals which were killed. The discovery of pus or microbes in patients dying of ordinary diseases did not necessarily mean that the cavities had been invaded in health. Professor Hewlett and

he had studied the air passages in animals killed in the laboratory and found them sterile. Wurtz and Lermoyez, in Paris, claimed that mucus was bactericidal, but Professor Hewlett and he had only found it was inhibitory.

Dr. P. WATSON-WILLIAMS said that it had been generally conceded that there was, in acute inflammatory conditions of the nasal mucosa, a shedding of ciliated epithelium analogous to that taking place in the acutely inflamed trachea and bronchi. It was the damage done to the mucosa in acute sinusitis which often determined the persistence of the infection. It would be valuable if a more systematic investigation could be made into the condition of the mucosal membrane when a sinus was opened and it was possible to remove a portion of the ciliated epithelium. Another interesting part of the paper was that concerned with tracing the course taken by discharges. He hoped these researches would be continued.

Dr. LOGAN TURNER said that what interested him most in what Mr. Yates had said was the course which the coloured material took in its passage from the antrum; it was similar to that of the course of the lymphatics as described by Grünwald in his experiments. The lymphatics of the antrum passed out through the ostium and along the upper surface of the inferior concha backwards on to the lateral wall of the naso-pharynx. Histological observations on the condition of the mucous membrane in chronic sinus infection showed that large areas became converted from the ciliated type to the stratified squamous type of epithelioma, and possibly that was an explanation of the slower progress of the coloured particles held in the contained fluid.

Mr. SYDNEY SCOTT asked what was the effect of cocaine on the activity of the ciliated epithelium; also was it practicable to use an endoscope for the researches satisfactorily without cocaine?

Mr. YATES (in reply) said that he had made experiments on the point raised by Sir StClair Thomson, using liquid paraffin, and the result was the same. It was a tenable theory that under the use of mucus or paraffin the organism was compelled to live with its own toxins, and therefore it died. He thought the death of the organism was due to auto-intoxication, rather than to any effect of the mucus. He had investigated the ciliary action within the maxillary sinus in two cases only, in both of which a bucco-antral fistula was present. The ciliary action took place in an upward direction on both the outer and inner walls of the sinus. This ciliary action began in the lowest portion of the antrum, namely that part which lay between the roots of the teeth, and the origin of the two streams might be compared to the parting of the hair, and there was a sort of no man's land in between. Certain vessels in the pharynx were coloured by the fluid, but although these were in the position of the lymphatics he was uncertain whether the vessels so coloured were lymphatic vessels or perivascular lymphatics.

With regard to the effect of cocaine on the ciliæ, if the nose was deeply cocainized the ciliæ were quite paralysed, but if the nose was cocainized lightly by spraying, there was generally very little action of the cocaine in the middle meatus itself, and the ciliary movement in that situation went on.

Sometimes it was difficult to be sure whether the paralysis was due to cocaine, or to the toxin, and it was then necessary to repeat the experiment by anæsthetizing a small area of the inferior meatus with solid cocaine, making the injection of the dye through a cannula introduced through the anæsthetized area, and watching the effect upon the ciliæ of the middle meatus which were unaffected if this method was employed.

The Experimental Basis for Theories on Vestibular Function.

By Professor R. MAGNUS (Utrecht).

EVERYONE who is compelled to make use of hypotheses as to the function of the vestibular organ, for further experimental research and for clinical purposes, feels the necessity of reviewing from time to time the facts from which these theories have been deduced, and to see whether this basis is still a solid one.

About a hundred years ago Flourens was the first to show that lesions of the internal ear were followed by motor disturbances. But it was only in 1870, in a short but suggestive paper, that Goltz stated that the vestibular organ is a special sense organ for equilibrium. Shortly afterwards three independent investigators, Mach, Breuer and Crum Brown developed the theory of the function of the semicircular canals which is to the present day the firm basis for all the work done on this part of the labyrinth. Breuer had already at this early date drawn a sharp distinction between the function of the ampullæ and the otolithic maculæ. To the ampullæ he ascribed all reactions and sensations evoked by rotatory movements, to the maculæ the reactions to different positions in space and the sensations of progressive movements. Of the subsequent investigators, two especially must be named: Högyes carefully worked out the mechanism of the rotatory reactions and compensatory positions of the eyes, depending on labyrinthine stimulation; Ewald, at the end of a long series of experiments, came to the conclusion that there exists an influence of the labyrinths on muscle tone. Ewald believed that this labyrinth tone depended on the ampullæ, but, later, in a paper from his laboratory (N. Ach) an otolithic origin of this tone is also discussed. The well-known researches of Bárány on caloric stimulation of the vestibulum also contributed very much to our knowledge.

In the last fifteen years a great number of experiments have been done at Utrecht, with the aid of many collaborators, on the reflexes connected with posture and attitude, with righting function and distribution of muscle tone and on the rôle played in these functions by different sense organs, and especially by the labyrinths. My friend, Dr. de Kleijn, of Utrecht, last year gave an account before the Section of Otology of some results [1] so far reached, and I therefore assume that the chief points as far as they concern the otologist are known to you. The following groups of reflexes, arising from the labyrinths (and absent after labyrinth extirpation) have been investigated:—

TABLE I.

I.—*Reflexes responding to movements*	
1. Rotatory reactions (*a*) On head ... ... (*b*) On eyes ... ... (*c*) On limbs ... ... (*d*) On trunk	*Apparatus of semicircular canals.* (Otolithic apparatus may also be stimulated by movements)
2. Progression reactions	
II.—*Reflexes resulting from position*	*Otolithic apparatus*
1. Tonic labyrinthine reflexes on the body musculature	
(*a*) On limbs	Utricle
(*b*) On neck (and trunk)	Utricle
2. Labyrinthine "righting" reflexes	
(*a*) Asymmetrical	Saccule
(*b*) Symmetrical	Utricle
3. Compensatory eye positions [2]	
(*a*) Vertical	Saccule
(*b*) Rotatory	?

[1] For all details see R. Magnus, "Körperstellung," Berlin, Springer, 1924.

[2] In rabbits and guinea-pigs. In monkey and man (*a*) and (*b*) must be exchanged.

In the first group the reflexes are transient, and are evoked by *movements* (more exactly spoken of as accelerations); in the second they are dependent on *positions* of the head in space; the latter reflexes are tonic and last as long as the head is kept in a definite position.

In the first place we must try to find out whether the different groups of reflexes can be connected with certain structures in the vestibular organ, especially which are reflexes from the canals and which from the otolithic maculæ.

For this purpose we can use a method, first described by Wittmaack. By centrifuging anæsthetized guinea-pigs it is possible to detach the otolithic membranes from the maculæ, and to leave the ampullæ and cristæ of the canals intact. In every single case at the end of the experiment it must be evident by careful microscopic examination, that the epithelium of the maculæ is completely freed from the otoliths, and that the detached membranes are found back in other places of the *endo*lymphatic space, whereas the ampullar apparatus is not damaged. In later experiments it was found by Dr. de Burlet, at Utrecht, that it is possible by means of placing the head in different positions on the centrifuge, to throw the membranes purposely in certain directions, and to find them back at the expected places.

Physiological investigation of successfully centrifuged guinea-pigs has shown that, after detaching the otolithic membranes, all reflexes responding to movements are still present and unchanged, whereas all reflexes resulting from position are absent. From this we must conclude that the reflexes of the first group (Table I) are elicited in the canals, and the second group in the otolithic apparatus. This is in accordance with current views, as far as the rotatory reactions are concerned, but is in contrast to common opinion as to progression reactions. But the experiments leave no doubt that very definite reactions to rectilinear accelerations can be observed after centrifuging all otoliths away, and that therefore these reflexes must be evoked in the canals. These experiments do not by any means exclude the theory that the otolithic apparatus also may be excited by movements, indeed this is very probable. But unfortunately we cannot prove it experimentally, because we know of no method of destroying the canals and leaving the otoliths intact. As far as the function of the otolithic apparatus is concerned, we may safely conclude that all reflexes resulting from position are dependent on them, and that the maculæ therefore are, with different positions of the head in space and with different inclinations against the horizontal plane, in different states of excitation, which remain unchanged as long as the macula keeps its position. The otolithic apparatus is therefore the locality of origin of labyrinthine tonic reflexes.

Before entering into any discussion as to the intrinsic mechanism of the otolithic organ, it is necessary to know in what state of excitation or non-excitation the sensory epithelium of the maculæ is, if *no* otoliths pull or press upon it. This question can be settled in centrifuged guinea-pigs with all four otolithic membranes detached. Directly after the centrifugalization the animals show symptoms of macular stimulation, for instance eye-deviations, turning of the neck, rolling movements, &c. But these soon disappear, usually after one to two hours, and always the next morning. After that time no symptoms can be detected, which point to a remaining stimulation in consequence of the trauma set up by the centrifugalization. But in order to be sure that no such traumatic stimulation was still present, we waited seven to eleven days. If then one labyrinth is paralysed by the injection of a small amount of cocaine into

one middle ear, the other labyrinth with detached otoliths is the only one left in action. There are then two possibilities. If the sensory epithelium without connexion with its otoliths does not produce any excitation, paralysis of one labyrinth cannot give rise to any symptoms arising from the maculæ of the intact side. If, on the other hand, the epithelium still produces stimuli, the symptoms of unilateral macular excitation must appear, because the compensation by stimuli from the other side has been removed. The latter appears to be the case in the described experiments, which always result in turning of the neck and vertical eye deviation. The conclusion is, that the sensory epithelium of the maculæ is able to produce excitations of their own without any connexion with otolithic membranes and that the stimuli, reaching the medulla from both sides, compensate each other intracentrally, so that no asymmetrical symptoms arise. The epithelium of the maculæ resembles in this respect the retina, which also can produce stimuli without any light. The function of the otolithic membrane consists therefore in increasing or decreasing the state of excitation of the sensory cells according to their situation in space, by pulling at or pressing upon the macula. This mechanism renders it conceivable, that the otolithic apparatus is practically not fatiguable, because the production of stimuli perpetually goes on depending on some metabolic changes in the sensory cells and is only changed in intensity by the mechanical action of the membranes.

The following data, for which I am indebted to Dr. de Burlet, give an impression of the extreme minuteness and sensitivity of the whole apparatus.

TABLE II.

		Utricle		Saccule
Size of macula	(macacus)	2·06 mm.²	...	1·8 mm.²
,, ,,	(guinea-pig)	0·48 mm.	...	0·49 mm.²
Weight of	otolithic membrane (rabbit)	0·07-0·05 mgr.	...	0·06 mgr.
$CaCO_3$ crystals in	,, ,, ,,	31·8-38·6 per cent.		30 per cent.
Specific gravity of	,, ,, ,,	1·32-1·39	...	1·27

The maculæ are of the size of $\frac{1}{2}$-2 mm.², each membrane has a total weight of about $\frac{1}{16}$ of a milligram, the arragonit-crystals ($CaCO_3$) embedded in one membrane weigh about $\frac{1}{50}$ of a milligram. From these data it should be possible to calculate the changes of positive or negative pressure, which are sufficient to influence the whole macula or a single epithelium cell: the result would be a very small figure for the threshold of this sense organ.

We have now to discuss the question as to how far we can ascribe the different groups of otolithic reflexes (Table I, Group II, No. 1-3) to the different maculæ, viz., of utricle and saccule respectively. For this purpose we can use the careful measurements of Dr. de Burlet and his collaborators on the position of the maculæ in the skull of guinea-pigs, rabbits, cats and monkeys. We tried to fix experimentally, for each one of the different otolithic reflexes, the positions of the skull in space in which the reflex has its maximum and its minimum, and investigated the position of the maculæ in these maximum and minimum positions of the skull. The results were as follows:—

The *tonic labyrinthine reflexes on the extensor muscles of the limbs*, which can easily be studied in the decerebrate preparation of Sherrington, have their maximum in the back position of the head with the snout a little above the horizontal plane. In this position the maculæ sacculi have no characteristic position, but both maculæ utriculi lie in the horizontal plane. After unilateral labyrinth extirpation the position of the maximum does not change: from this it may be deduced that the maculæ, from which these reflexes arise, are

approximately in the same plane. This also points to the utricles. In the minimum position for these reflexes the maculæ utriculi again lie horizontally. We may safely assume, therefore, that the tonic reflexes on the limb-muscles come from the utricles and that the maximum and minimum of stimulation is reached with horizontal position of the maculæ; that is to say, that the way in which the otoliths influence the epithelium is by pulling or pressing and not by shifting, as the older theories assumed.

If one believes, that the essential effects of these reflexes are on the *static* muscles, the extensors, it follows that the maximum of the stimulation is reached, when the membrane hangs and pulls on the macula. But there are also reflexes—weaker indeed—on the flexors, which have their maximum if the otoliths press on the epithelium. In the case of the tonic reflexes on limb-muscles we cannot therefore decide with certainty in what position the maximum of excitation is reached, we can only surmise that it is the hanging position, a conclusion which should appear to be the right one on investigating other reflexes.

In the same way we come to the conclusion, that the *tonic labyrinthine reflexes on neck and trunk* also depend on the utricles.

Of the *compensatory eye-positions* the site of origin of the *rotatory* reflexes (II, 3*b*) has not yet been made out. Experiments to determine this are now in progress.

The *vertical eye deviations* appear to depend on the sacculi. If one labyrinth is extirpated, the well-known vertical eye deviation follows. This is maximal if the head is in the lateral position with the intact labyrinth below; it is zero or minimal, if the intact labyrinth is above. But with both labyrinths intact, no eye deviation is present with the head in normal position. This change of the minimal position after unilateral labyrinth extirpation proves that the maculæ in question cannot be situated in one plane and that these reflexes therefore arise in the sacculi. With one labyrinth destroyed, the eyes are not deviated at all in lateral position with the *macula sacculi lying horizontally* and the *otolith pressing on the epithelium.* This is the *minimum* position. The maximum of excitation is reached if the membrane pulls at the macula. In this case it can therefore be proved that the maximum of excitation is present if the otoliths *hang.*

The *asymmetric labyrinthine "righting" reflexes* bring the head from lateral positions into the normal standing. After unilateral labyrinth extirpation they tend to bring the head in such a position that the intact macula sacculi lies horizontally with the otolith pressing. This is the minimum position, in which the reflex comes to an end. Here again the same mode of stimulation is found.

The *symmetrical "righting" reflexes* act in such a way that of all possible symmetrical situations the head is always held in such a position that the maculæ utriculi lie horizontally and the otoliths are pressing. The conclusion that these reflexes arise in the maculæ utriculi is also confirmed by another line of experiments to be described presently.

This review, perhaps too short and aphoristic, shows that the utricles are the site of origin of the tonic reflexes in the body musculature and the symmetrical righting reflexes, whereas the sacculi give rise to the asymmetrical righting reflexes and the vertical compensatory eye positions. For the rotatory eye positions the site of origin has not yet been found.

In respect of the reflexes of the sacculi and the symmetrical righting reflexes of the utricles it can be proved, that the minimum of stimulation in the sensory cells is attained if the otolithic membrane presses upon them and

that the excitation becomes maximal with the otoliths hanging. In the case of the tonic reflexes on the body muscles the same is probably true. In this way a first conception has been attained as to the way in which the otolithic apparatus is enabled to fulfil its task.

This distribution of functions between the otolithic maculæ has been deduced only from one line of evidence, viz., the comparison of the experimentally found maximum and minimum positions with the situation of the maculæ in these positions found by anatomical investigation. It would be a great advantage, if a confirmation of these results could be obtained in quite a different way. This happens to be the case.

Injection into one middle ear of a guinea-pig of a small amount of cocaine induces unilateral labyrinth paralysis. But the symptoms do not appear one and all at the same time, they come on in definite stages corresponding to the anatomical situation of the sense organs in the internal ear. In the first stage a vertical eye deviation and a unilateral paralysis of the asymmetrical righting reflexes develop, dependent on a paralysis of the macula sacculi, which is situated just opposite to the foramen ovale and will, therefore, first be reached by the diffusing cocaine. In the second stage the symptoms of paralysis of the utricle are added: (1) turning of the neck in consequence of unilateral absence of the tonic reflexes on neck muscles, and (2) unilateral abolition of the symmetrical righting reflexes.[1] *This sequence is in accordance with the distribution of functions between the maculæ, stated above.* In the second stage the canals are not yet paralysed, they can still be stimulated by rotation and by syringing with cold water. In the third stage the excitability of the canals disappears, the horizontal canal being the first, one of the vertical canals the last. Finally the whole vestibular organ gets out of function.

These experiments yielded also other interesting results, which I cannot describe here. Only one point must be mentioned.

In the second stage, in which both the maculæ sacculi and utriculi are paralysed but the ampullæ of the canals are still in function, the eyes are deviated but no trace of nystagmus can be detected. But at the moment the excitability of the canals begins to disappear a very strong nystagmus manifests itself. This well known nystagmus after unilateral labyrinth extirpation does not depend therefore on the otolithic maculæ, but on the apparatus of the ampullæ. This leads to an interesting sequence. If the ampullar epithelium on one side is brought out of action, the symptoms arising must depend on the ampullar epithelium of the other intact side. The nystagmus is not only present if the head is moved or rotated, but also if is not moved at all. Therefore, the sensory cells of the cristæ ampullares possess the property also of producing excitations if there is no current or pressure change in the endolymph space of the canals. The epithelium of the cristæ exhibits the same physiological behaviour as that of the maculæ. It gives rise to continuous stimuli flowing through the nerves to the central nervous system. The difference of function between the canals and the otolithic maculæ depends on the mechanical arrangement of apparatus, connected with their epithelium. In the canals, currents or pressure changes are evoked by accelerations, last only a short time and pass away. The state of excitation in the epithelium changes, therefore, temporarily and soon returns to the original value, so that only transient reactions can be evoked. The otolithic membranes, on the contrary, preserve their pulling or pressing action so long as the head maintains a definite position and can induce permanent changes in the state of the macular epithelium, which in this way can set up tonic reflex actions.

[1] Only to be seen in animals in which the other labyrinth has been extirpated beforehand.

The experimental work on the lines described, has thus established definite ideas as to the functioning of the different parts in the vestibular organ, which may form the basis for further research and for clinical investigation. But as with all hypotheses, these ideas always need confirmation by new facts and have to be modified, as soon as conflicting observations are made. Possibly they may be of value for diagnosis and treatment in human cases; and clinical examinations will undoubtedly extend and perhaps correct them. In every case we must remember the words of Goethe: "Hypotheses are scaffolds, which are erected before the building, and broken down when the building is ready. They are necessary for the workmen, but one must not take the scaffold for the building."

Labyrinthine Compensatory Eye Positions in Patients.

By A. DE KLEIJN and C. VERSTEEGH (Utrecht).

WE clinical otologists are very interested in the endeavour to find out which of the tonic labyrinthine reflexes are suitable for use in the hospital clinic. The tonic labyrinthine reflexes on the body-musculature have so far only been found in patients with more or less severe lesions of the central nervous system.

For the investigation of the labyrinthine righting-reflexes an animal can be held in the air by its flanks and the trunk can be brought into different positions in space. It is evident that this experiment cannot be used clinically; it would only be possible in very young children; moreover, the voluntary head-movements make it impossible to draw exact conclusions.

As the observation of the vertical compensatory eye-positions is also disturbed by voluntary eye-movements, the only tonic labyrinthine reflexes which enable us to obtain a knowledge of the functions of the otoliths in normal persons are the rotatory compensatory eye-positions, which from analogy with the experiments on animals are thought to arise from the saccular maculæ.[1]

In 1906 Bárány[2] studied the counter-rolling of the eyes in a number of normal persons and patients; he emphasized the fact that this investigation has a great clinical value. The method he used, however, does not enable us to study the labyrinthine compensatory eye-positions separately, as we shall see later on. This year we[3] published a simple method of measuring these reflexes, similar to the one used in previous experiments on animals. A pair of spectacles without glasses but with two thin wires in the place of these is fixed before the eyes. A cross marked upon the cornea is necessary. This can be made by cutting from the dried thin membrane that lies immediately under the hard shell of a hen's egg round pieces as large as the cornea, and making on this membrane a cross with drawing ink. Now, after cocainizing the eye, the prepared membrane may be placed with a moist brush on the cornea, to which it will easily adhere, especially if some small cuts have been made on the outer surface of the membrane. In this way it is possible to fix a cross on the cornea without injuring the eye and without causing the patient any inconvenience during or after the manipulation. If we now photograph the eye and frame together we can determine within one half of a degree the

[1] In rabbits, guinea-pigs, &c., the *vertical* compensatory eye-positions; in men and monkeys the *rotatary*, arise in the saccular maculæ.

[2] Bárány, R. "Ueber die vom Ohrlabyrinth ausgelöste Gegenrollung der Augen bei Normalhörenden, Ohrenkranken und Taubstummen." *Archiv für Ohrenhlk.*, lxviii, 1906, p. 1.

[3] de Kleijn, A., and Versteegh, C., "Method of Determining the Compensatory Positions of the Human Eye," *Acta Ot. Lar.*, vol. vi, fasc. 1-2, p. 170. This journal Dec., 1923; p. 662.

rotatory diplacements of the eye in different positions of the head in space by measuring the angle made by one arm of the cross with one of the wires of the spectacle, especially after enlarging the photographs by means of projection on the wall.

From analogy with the experiments on animals we may expect that the rotatory displacements of the eyes will be maximal in both lateral positions of the patients, the saccular maculæ lying horizontally in these positions. This is the reason why in all cases we took three photographs, one with the head in normal position and two with the head in each lateral position.

As in all investigations on tonic labyrinthine reflexes we must pay attention to the following points;—

(*a*) We must exclude tonic neck-reflexes on the eye-muscles. These reflexes arise when the position of the head is changed in relation to the trunk. Therefore in all positions of the head in space the symmetrical position of the head in relation to the trunk must remain fixed. In the methods of Bárány, v. d. Hoeve,[1] Struycken-Houben[2] and Kompanejetz,[3] the patient has to incline his head alternately to both shoulders, and in this way a combination of tonic neck and tonic labyrinthine reflexes on the eye muscles is studied.

(*b*) It is necessary to exclude reflexes in response to movements (reflexes of the semicircular canals). These reflexes are transitory; the reflexes resulting from position (reflexes of the otoliths) are tonic. Therefore after any change in the position of the head in space, it is necessary to wait a few minutes before examining the tonic labryinthine reflexes.

The following table shows the results of an investigation which we made following this method in a number of normal persons and patients.

LABYRINTHINE COMPENSATORY EYE-POSITIONS.

Normal Persons.

Patient	Right side position	Left side position
A	3°	7°
B	4°	7°
C	7°	6·5°
D	8°	6°
E	8°	12°
F	10°	13°
Both Labyrinths without Function.		
G	0°	0°
Right Labyrinth without Function.		
H	0°	3·5°
Left Labyrinth without Function.		
I	8°	0°
Lesion of the Left Pars Inferior Labyrinthi.		
K	11°	1°
Lesion of both Partes Superiores Labyrinthi.		
L	10°	6°

[1] v. d. Hoeve, J., "Relation between Eye and Ear (including the Vestibular Organ)," *Arch. of Ophth.*, i, 1922, p. 333.

[2] Houben, H. M., "Bydrage tot de Kennis der Compensatorische Raddraaing van het Oog." Diss., Mei, 1924, Utrecht.

[3] Kompanejetz, S. M., "Die Gegenrollung der Augen nach Granatexplosionen, z. f. Hals-Nasen und Ohrenh." Bd. 5, 1923. p. 53.

Normal Persons.

First, you see that the degree of the rotations is very small, and that contrary to what takes place in animals, e.g., rabbits, these reflexes do not play an important physiological part. Secondly, there is a great difference between the rotatory displacements of the eyes in different persons, as well as in the same person with regard to both lateral positions. Observations made on the same persons on different days showed that the degree of rotation was not constant.

Bilateral Loss of Function of the Labyrinths.

As a consequence of our supposition that the rotatory compensatory eye positions are dependent on the labyrinths, we must expect that after bilateral loss of function of the labyrinths these reflexes will disappear.

We had the opportunity of confirming this in one patient, a female, aged 17, with deaf-mutism, after septic otitis media resulting from scarlet fever. There was absolute deafness of both ears. There was no response at all to the various rotation and caloric tests on both sides. According to this no rotatory eye-displacements were found in either lateral positions. However, after inclining the head to the left shoulder and so changing the symmetrical position of the head in relation to the trunk, the patient showed a rotatory counter-rolling of the eyes of 2½°. This proves that it is really necessary to exclude tonic neck-reflexes on the eye muscles when examining the labyrinthine compensatory eye positions.

One Labyrinth without Function.

The table shows the result of an examination in two patients. One patient lost the right labyrinth through labyrinthectomy; the other had a complete loss of function on the left side after fracture of the os petrosum. The results were analogous with the experiments on animals, i.e., in the lateral position with the intact labyrinth underneath, the saccular macula lying horizontally and the otolithic membrane pulling at the sensory epithelium, the rotatory displacements of the eyes were of a normal magnitude. In the opposite lateral position, with the otolithic membrane pressing, no rotations were observed.

These investigations in patients who have lost one or two labyrinths are, however, only of theoretical value. Various other methods (rotation, caloric and hearing tests) enable us to determine whether there is a complete loss of function or not. It is in cases with *partial* loss of function of the labyrinths that the importance of determining the compensatory eye positions becomes clear. Anatomical investigations have shown that the labyrinth is divided into two parts. First the pars inferior which contains the cochlea and the saccule, and secondly the pars superior with utricule and semi-circular canals; these two parts being separated by the so-called "Grenzmembran" (de Burlet). If this anatomical finding has also a clinical value, we may expect two types of partial lesion of the labyrinth: one type with lesion only of the pars superior, and another with lesion only of the pars inferior. If only the pars superior is disturbed, we shall find normal function of the cochlea and saccule with loss of function of the semicircular canals. The other type, in which only the pars inferior is disturbed, shows normal function of the semicircular canals with deafness and the lack of compensatory eye positions (saccule).

That such a localization with a partial lesion of the labyrinth is really possible to be observed clinically, is demonstrated by the following two cases :—

(1) Lesion of the Pars Inferior Labyrinthi.

Male, aged 48, showed after septic otitis media on the left side severe labyrinthine deafness with normal function of the semicircular canals. In accordance with this, there was nearly no function of the left saccule (counter-rolling of the eyes in the left lateral position only 1°). The function of the right labyrinth was quite normal. The counter-rolling in the right lateral position was 11°. Therefore in this case an impairment of the pars inferior of the left labyrinth occurs.

(2) Lesion of the Pars Superior Labyrinthi.

This patient, on the contrary, showed only a disturbance of the pars superior labyrinthi on both sides.

A boy, aged 7, was sent to us because his parents had observed that the equilibrium had been disturbed for some months past, after a severe diphtheria infection, without septic otitis media. The ophthalmological, neurological and internal specialists could not find any disturbance. In this case we find no response whatever to the different rotation and caloric tests. The hearing and the compensatory rotatory displacements of the eyes, however, were quite normal Thus only a lesion of the pars superior labyrinthi on both sides could account for this condition.

The number of patients we could examine is too small for us to draw definite conclusions. We hope that a continued investigation will make it possible in the near future.

Mr. Sydney Scott (Chairman) thanked Professor Magnus and Dr. de Kleijn for their valuable communications.

Vertigo in Relation to the "Otolith" and "Neck" Reflexes.

By Alex. R. Tweedie, F.R.C.S. (Nottingham).

The following notes were originally prepared for purpose of communication to the combined discussion on vertigo at the Royal Society of Medicine last February. As, however, no opportunity then occurred they are now submitted in this form.

All aural surgeons, of course, have had cases of vertigo referred to them, especially since the year 1907, and the development of the knowledge of the physiology of the semicircular canals, for opinion as to whether the cause of such vertigo could be shown to be due to an aural lesion.

With the exception of certain cases in which definite destructive lesions of the middle ear, labyrinth or cerebellum were discovered—testing the semicircular canal responses, as far as my experience went, was, so to say, "disappointing"—that is the responses were all surprisingly normal, or indeed in some cases they suggested a hyposensitive condition.

Since the publication by Professor Magnus and Dr. de Kleijn of their investigations on the otolith and the neck reflexes, and the attention they drew to the clinical application of their discoveries, it has become possible to demonstrate disturbances in certain cases which it seems right to regard as referable to otolithic impulses, or to the effect of neck movements, or to a combination of both.

Some account of twelve samples of such cases are here given. Although all details have not been included, the routine aimed at in each case was conducted as follows:—

(1) The ordinary objective examination of the nose, throat, ears, tubes, &c.

(2) Testing the auditory function.

(3) Noting the presence or absence of spontaneous nystagmus and testing the pointing reaction in the normal sitting position.

(4) Testing the semicircular canals, by either the rotation, caloric or galvanic methods.

(5) Laying the patient in the dorsal position with head extended—(?) "maximum" position for utricular action, and neck reflexes combined.

(6) Laying the patient on either side—(?) "maximum" position for saccular action; and noting in all these positions if nystagmus were induced.

(7) Testing the vertical pointing reactions with the head inclined to either shoulder—for the effect of (?) the combined reaction of the neck reflexes and saccules.

This routine has been adopted as the method of examination, based on the teachings of Professor Magnus and Dr. de Kleijn[1]; some observations of Professor Bárány[2] and methods described by Professor Quix.[3]

That the reports of the method of examination of the various cases show some lack of uniformity is due to the fact that it was inconvenient to submit the patient in all instances to all the tests and also that the routine of these examinations was in process of development. The routine is suggested as the ideal basis of examination.

It is of course important that the "latent" period of at least thirty seconds should be allowed to elapse before making the observations, and it must also of course be remembered that where the relative normal position of the head and trunk is disturbed, the combined influence of "neck reflexes" is a potential factor.

The object of this routine is, by the gradual elimination of other sources of possible stimulation of vertigo, to be able to demonstrate that such phenomena as then occur as the result of the passive positional tests under 5, 6 and 7, must be referable to the otolithic apparatus or neck reflexes.

That vertigo may be due to a simultaneous lesion of both semicircular canals and otoliths, or to the concurrence of other neighbouring disturbances, dependent on variation in blood-pressure, &c., is of course possible—indeed I think this is often the case—but the value of a routine which will help to discriminate is, of course, the basis of all differential diagnosis.

The cases may be summarized as follows:—

(1) Five occurred in men and women between 40-70, with mixed middle-ear and old-age deafness, and cardio-vascular lesions.

(2) Two cases were regarded as toxic in origin, one due to tobacco, one due to alcohol.

(3) One was a case of complete nerve lesion of one auditory nerve, regarded as due to mumps.

(4) One was a case of (?) cerebellar lesion.

(5) One was the result of concussion and (?) fractured base.

In all these cases either a nystagmus was induced, or pre-existing nystagmus affected when the patient was laid in the dorsal or lateral positions, or the previously accurate vertical pointing was rendered inaccurate by altering the position of the head.[4]

[1] *Journ. Laryng. and Otol.*, 1922, p. 213.
[2] *Ibid.*, 1921, p. 229; *Acta Oto-Laryngologica*, vol. ii, fasc. 4.
[3] "Le Rôle des Otolithes dans la Sémiologie de la Maladie de Menière."
[4] A table with details of these cases was included; this is being published in the *Journal of Laryngology and Otology.*

The communication is admittedly incomplete. The suspected diagnosis has yet to be confirmed, the further history of the cases to be obtained, and when occasion and opportunity offer, the subsequent post-mortem histological report is of course necessary before there is really sufficient evidence to warrant these provisional views being accepted as correct. It is only offered as an interim account of clinical conditions noted, with a view to stimulating others to test such cases on similar lines.

Articles in foreign literature are already appearing on this subject, amongst which attention may be directed to reports published since this paper was commenced of accounts of similar reactions, with most interesting commentaries by Dr. de Kleijn and Dr. Versteegh, of Utrecht, and Dr. Nylén, of Stockholm (*Acta Oto-Laryngologica*, vi, No. 1-2).

The latter author has also included accounts of his experiments with the dynamometer, which suggest that we shall have yet to add this method to our routine—but of this I have not yet any personal experience, it would seem however to be well worth adoption.

This communication may also help to direct attention to a most instructive paper by Drs. Fischer and Wodak, of Prague, on the "Physiology of the Human Vestibular Apparatus"; a translation in English of this article, which they have asked me to undertake, I trust will shortly appear in the *Journal of Laryngology and Otology*.

One of the essential points these observers emphasize is the gradually decreasing "pendulum" oscillation in the *feeling* of vertigo after the stimulus has ceased, and the variations which can be induced in these sensations by subsequent alteration of the position of the head.

That nystagmus induced by artificial stimulus can be varied by altering the position of the head is well known of course—but one of the lessons of this paper is that *subjective sensations of vertigo* can also be made to vary with head positions—a point to be borne in mind when trying to reconcile the patient's story with the character and seat of disease.

The subject is most intricate, as we have to do with a degenerating organ from a phylogenetic point of view, and sensations which we habitually ignore, until pathological lesions attract our attention. It seems most important we should collect as many observations as possible on these phenomena due to the research work at Utrecht, so that we may be able to determine their import, assess their true value, and thus perhaps learn ultimately how to alleviate the alarm and distress so often associated with vertigo.

DISCUSSION.

Mr. SYDNEY SCOTT (Chairman) said the thanks of the Sections were also extended to Mr. Tweedie, who confessed himself a disciple of Profesor Magnus and Dr. de Kleijn; he was largely responsible for having drawn the attention of Members to the important work in Utrecht.

Mr. J. S. FRASER said he felt with regard to the contributions of Professor Magnus and Dr. de Kleijn, that as the research proceeded the phenomena and the reasons for them became clearer and clearer. He would be glad to have indicated, by means of the hands, the position of the utricle and saccule in the human being with the head erect. Probably it was the same as in the rabbit. (Professor MAGNUS: Yes.)

In reply to a question which he asked Professor Magnus, he said the rabbit slept with the head in the same position as it occupied when it was running about, namely, forward, resting on its fore-paw. Human beings did not sleep in that posture as a rule, therefore the otolithic apparatus could not be in its "minimal" position when the person was lying on the left or on the right side, or on the back.

He asked whether any work had been done in Utrecht in regard to the effect of explosions on the inner ear. In examining the labyrinths of soldiers who had been blown up in the war, the otolithic membranes were sometimes found to be detached.

With regard to cocainizing the middle ear, it was known how difficult it was to cocainize the inside ear from the operative point of view. He was interested to learn that in animals cocaine could permeate so freely through the oval window, or the bone surrounding it, into the inner ear. It was true that in the microscopical examination of the ears of guinea-pigs one found that celloidin permeated into the inner ear more easily than in the human subject; in the latter, one had to open the superior canal to allow the fluid to enter.

He also asked whether nystagmus was regarded as a symptom depending entirely on the canals or whether it was caused by the otolithic apparatus. It had been said that in sea-sickness, which was supposed to depend on the otolithic apparatus, nystagmus was absent. There would soon appear in the *Journal of Laryngology* an article in which the fistula symptom was attributed to the otolithic apparatus and not to the semicircular canal.

What was the nature of the lesion in the last two cases related by Dr. de Kleijn? The last-but-one might be an instance of congenital lesion of the pars inferior. Surely such a case as the last one, an isolated lesion of the pars superior, must be very unusual.

Dr. ALBERT A. GRAY said that he had himself suggested that all physiological sensation was the result not so much of stimulation, but rather of change of stimulation. If any nerve, or neuro-epithelium, or organism was stimulated it received a change in the degree or quality of stimulation, and if stimulation remained constant it had no effect; if the differential equalled nothing, no sensation was felt at all. In vision, for instance, this was obvious. There was a movement in which the stimulus was constantly changing, a change of intensity which was represented mathematically by the sine of the angle which represented the time. The same law held in the case of hearing and the sensation of heat.

There were no canals at all in the more primitive animals, but only otocysts. Beginning with the lamprey and going upwards, semicircular canals were found which were developed from the otocysts, and the canals could appreciate more minute changes in stimulation. If stimulation in the canals remained constant there was no sensation. A familiar illustration of that was the fact that in a moving railway train many individuals could sleep so long as a uniform speed was maintained, but as soon as there was a slowing of the train the canals responded to the change and the sensation thus produced checked the slumber.

Sir JAMES DUNDAS-GRANT said that the knowledge now possessed concerning the functions of the maculæ, utricles and saccules should result in more attention being paid to them in the future, without an exaggerated view of their value. Such small parts in such close contiguity, must often be affected together; it must be unusual for any of them to be the seat of isolated lesions. Mr. Fraser had pointed out that the nerve which went to the posterior ampulla went also to the saccule, an observation which was very suggestive. The saggitta of the saccules was more displaced by putting the head to one side or the other. The other reflexes must be eliminated before a conclusion could be arrived at.

He (the speaker) wondered how long the effect of unilateral labyrinth extirpation lasted. The disturbance in connexion with an eliminated labyrinth was probably due to the over-action of the normal labyrinth on the other side which was left unbalanced.

Mr. E. WATSON-WILLIAMS said that two years ago he had made one clinical observation the significance of which he did not at the time appreciate. A signalman, between 30 and 40 years of age, complained that when he went up a ladder to attend to lamps, he became giddy; the giddiness came on when he put his head into a particular position, looking up and to the right with the left ear near the left shoulder. When his head was placed in this position, a coarse mixed nystagmus to the right was seen, lasting as long as the position was kept, at least for several minutes. The position of the head in space, and not in relation to the body, was what determined the

giddiness. The caloric and rotatory tests were normal, and the condition was diagnosed as one of otolith disease, and demonstrated on several occasions. He had been hoping to show the case, but the man's trouble cleared up entirely, without treatment.

Mr. F. J. CLEMINSON said that in the case of a small baby, two months old, there was no ear on the right side; the pinna was undeveloped, and there was no meatus. The child always kept its head with the sound side turned upwards when placed on its back, and if laid prone it reversed the head. The mother said she had always noted this when the child was being bathed. Later, as the child became capable of fixing objects with its eyes, this orientation of the head was no longer observed. Probably no labyrinth existed on the side on which the ear was defective.

Mr. W. IBBOTSON said he was to show that day a case of labyrinthectomy. Before doing the operation, he threw a concentrated beam of light into the eyes by means of the ophthalmoscope, and when it was thrown into the eye on the affected side, the patient fell in a vertiginous attack, which lasted for some time. No such effect followed the projection of light into the other eye. Since the operation, the reaction on both sides was normal. He asked what was the anatomical path of that reflex, whether it passed round the commissure of Gudden and into the red nucleus. He believed there was a connexion between the red nucleus and the internal geniculate body.

Had Dr. de Kleijn considered that there might be good hearing even though there was a lesion of the superior portion of the labyrinth, the vestibular labyrinth in its inferior part being unaffected? He did not see how it came about, because if the endolymph were continuous between the vestibular and the cochlear labyrinths he could not understand how there could be a lesion affecting only one part of the vestibular labyrinth, the inferior portion being healthy, with normal hearing.

Professor MAGNUS (in reply) said that Mr. Fraser raised the question of the position of the otoliths in man. The measurements of Dr. de Burlet were not yet published, but as animals always kept their head in such a position that the otoliths of the utricle lay almost horizontally, the same could perhaps be expected in man. In this case the situation of the macula sacculi would also be about the same in man as in animals. The observations of tonic labyrinthine reflexes on the limbs and of compensatory eye positions in man were in accordance with this suggestion.

Mr. Fraser's second question was very interesting, but also very difficult, namely, the position taken during sleep. During sleep the righting function was not operative; usually people lay on their side and remained so. During sleep the centres of the midbrain must be out of action. Because an animal took up a certain position in sleep it was not to be assumed that this depended on righting reflexes.

With regard to the cocainization of the labyrinth in man and in different animals, these experiments on cocainizing the internal ear by injecting the middle ear only succeeded in the guinea-pig; in the rabbit and the cat they were not a success. He had no experience of cocainizing man.

He knew that in the guinea-pig unilateral paralysis of the maculæ did not cause nystagmus and that paralysis of the canals on one side caused nystagmus. Whether *stimulation* of the maculæ on one side with the maculæ of the other side intact could give rise to nystagmus was not yet settled; therefore he could not give any definite opinion as to that.

He did not think it had been decided that the symptoms of sea-sickness only arose in the otolithic apparatus; he did not see why the canals should not coöperate in this.

In answer to Dr. Gray, the investigations into muscular changes in animals had been made after complete extirpation of the cortex cerebri; therefore there were no reactions to subjective feelings in the experiment.

There was no doubt that the otolithic apparatus could give rise to tonic reflexes which remained constant for months, even for years. The same held good with the tonic reflexes arising in the proprioceptors of the neck and, according to the lately

published experiments of Sherrington, also with the static reflexes of the body muscles arising from proprioceptive stimulus (myotatic reflexes). He agreed with Dr. Gray in regard to the canals, but believed that the otolithic apparatus could maintain reflexes by a constant level of excitation. A possible but improbable alternative was that the otolithic stimulus was transient and that the maintenance of the reflex was of central origin.

In answer to Sir James Dundas-Grant, the direct symptoms of unilateral labyrinth extirpation could by careful examination be demonstrated as long as the animal lived. But after a few weeks or months they were more or less compensated, partly intracentrally, partly by other reflexes connected with posture, such as neck reflexes, righting reflexes, reflexes arising from the body and the eyes, &c. The amount that was contributed to the static functions by the labyrinths decreased as the animal scale was ascended, whereas the influence of other receptors increased. Therefore, after unilateral labyrinth extirpation the other sense organs would be able to compensate much more in the monkey and in man than in the rabbit and guinea-pig, so that in the former the symptoms soon became difficult of detection.

The questions asked by Mr. Jenkins, Mr. E. Watson-Williams and Mr. Cleminson he would prefer Dr. de Kleijn to answer. He had not any experience in connexion with the influence of the eyes upon vertigo.

Dr. DE KLEIJN (in reply) said, in regard to the effect of explosions, that in a military man suffering from complete deafness on one side after shooting, the labyrinthine compensatory eye-positions were quite normal. He thought it only possible to settle the question in course of time after examining a great number of patients.

He agreed with Mr. Fraser that an isolated lesion of the pars superior labyrinthi was very unusual.

The division of the labyrinth into two parts could only be applied to the peripheral labyrinth. The combination, saccule-posterior vertical canal, mentioned by Mr. Fraser we could expect in cases with lesions of the vestibular nerve. With regard to the opinion of Mr. Ibbotson that the endolymph between the vestibular and cochlear labyrinths was continuous, and a lesion only of the pars superior labyrinthi was incomprehensible; he (Dr. de Kleijn) said that this was only possible because the canalis utriculo-saccularis in man was very narrow.

After unilateral labyrinth extirpation in animals; the sound side was always carried uppermost as in the case of the patient mentioned by Mr. Cleminson.

With regard to the attacks of vertigo, if the head was brought into a special position in space, he said that it was very difficult to explain these cases, because the attacks might be due to different influences: labyrinthine reflexes, neck reflexes, &c. Changes of pressure in the cerebro-spinal fluid and in the labyrinth, caused by changing the position of the head in relation to the trunk, might also exercise an influence.

Mr. A. R. TWEEDIE (in reply to Mr. Fraser's question) suggested that the reason we did not keep our utricles in what was presumably the normal position was, that men were not, so to speak, guinea-pigs; we had learned to disregard our otolithic functions in healthy conditions, and to rely more upon our tactile and other sensations. He further suggested that during sleep we relied mostly on these tactile sensations and our "higher" development. His own most comfortable sleeping posture was lying on his abdomen.

DISCUSSION ON NASO-PHARYNGEAL GROWTHS.

Sir WILLIAM MILLIGAN,

who opened the discussion, showed a number of lantern slides. He remarked that growths in the post-nasal space were not uncommon, but that the cause of clinical symptoms for which patients applied for treatment was often overlooked and the growth frequently attained a considerable size before it was discovered. The varying malignancy, the comparative inaccessibility and the practical

difficulties encountered in the complete eradication of the growths rendered the prognosis inevitably grave.

The principal varieties of growths were naso-pharyngeal fibromata, angiomata, sarcomata, endotheliomata, carcinomata and chondro-sarcomata. The classification of these growths, however, as a result of microscopical examination of a small portion, was frequently misleading.

Although nasopharyngeal fibromata were histologically benign, their clinical course could be described as almost malignant. It was not easy to determine the exact site of origin of the growth, but whether it was pedunculated or sessile was important from a therapeutic and prognostic point of view. Attention should be paid to the vault of the nasopharynx, the lateral wall, in front of or behind the Eustachian cushion, the Eustachian cushion itself and the orifice of the tube, the posterior surface of the soft palate, the various sutural lines, the adjacent accessory sinuses, the pterygo-maxillary recess.

His experience was that growths springing from the vault were most amenable to surgical interference, especially when possessing a pedicle. Growths springing from the posterior surface of the soft palate formed a group by themselves and were amenable to surgical removal, unless very extensive.

The gradual enlargement of nasopharyngeal fibromata was accompanied by considerable distortion of surrounding structures and at times by hæmorrhage and ulceration. Preliminary irradiation of the growth materially lessened the risks of severe hæmorrhage during attempt at subsequent removal, and also caused a marked shrinkage in size. Unscreened emanation "spicules" should be inserted with special trochar needles, encircling the growth and allowing of "cross-firing." For the surgical removal of the growth a long and highly tempered chisel to detach the growth and a strong forceps to pull it away, were preferable, in his experience, to the snare; and routes of approach were through the nasal passages or through the mouth, but he was opposed to splitting the palate. Diathermy presented certain advantages over many existing surgical procedures, and could be employed before or after the use of "emanation-spicule" treatment, or independently of it.

In angioma radium treatment was most effective; tubes should be implanted directly into the growth and external radiation given from plates.

Naso-pharyngeal sarcomata were met with occasionally; the growth might be discrete or diffuse, and, owing to the late appearance of glandular involvement, might appear to be amenable to surgical interference, but such was rarely the case. Diffuse sarcomata were marked by frequent attacks of hæmorrhage, progressive wasting and ultimately by glandular involvement and severe blood changes. Operative treatment of sessile non-pedunculated tumours was practically futile. Radium in sufficiently strong doses had an immediate, although evanescent effect. He emphasized the necessity of endeavouring to combat blood changes by intravenous or intramuscular injections of some arsenical preparation.

Endotheliomatous growths were comparatively benign and were best treated by radium, the application of the diathermy needle and occasional removal by the curette.

Three symptoms pathognomonic of pharyngeal carcinoma were: (1) Recurring spontaneous hæmorrhage, (2) recurring attacks of unilateral sero-mucous catarrh of the middle ear, (3) persistent otalgia without objective signs of imflammatory reaction. In many cases of nasopharyngeal malignancy "otitic neuralgia" was the earliest symptom. The columnar and basal cell type of growth were more favourable from the irradiation point of view than the

squamous-cell epithelioma, but unfortunately the epithelioma was the more common type of growth.

He believed that surgery offered less hope in these cases than irradiation or diathermy treatment.

Many years would have to be spent in elaborating radium technique and in raising dosage from the somewhat empirical position it at present occupied to a sound and scientific status.

Mr. Sydney Scott (Chairman)

said that most of these cases were met with too late, because physicians who first saw the cases treated the symptoms, instead of investigating the possible cause.

Dr. Dan McKenzie read a paper on "Diathermy in the Removal and Treatment of Pharyngeal Cancer."

Mr. Herbert Tilley

spoke of the route of approach to naso-pharyngeal fibroma. At a previous meeting he showed a very large specimen which arose from the basi-sphenoid, the lateral mass of the ethmoid and from the posterior wall of the left maxillary antrum. That was twenty years ago, and the patient was now a man in the best of health. He did not recommend lateral rhinotomy because it did not give such sufficiently good access, but preferred an incision under the cheeks from one malar ridge to that of the other side. After retraction upwards of the soft parts the ascending process of the maxillary bone was removed and a portion of the anterior wall of the antrum. Another important detail was to cut through the floor of the nasal septum in its anterior portion, because it facilitated the exposure of the "field of approach."

A further important point was rapidity of removal of the tumour. Once there was a clear field and one got at the base of the growth, its removal should be done as quickly as possible. He agreed that one must get as near the base of the growth as possible; this, and quick working, enabled the surgeon to control the bleeding which was severe for the time being. He favoured laryngotomy and plugging of the lower pharynx before beginning the operation. This preliminary step gave plenty of "slack" in the cheeks because the mouth could be closed. A further advantage of a laryngotomy was that the anæsthetist could keep out of the way of the surgeon.

Mr. Musgrave Woodman

said that carcinoma of the pharynx was a very important condition.

In regard to the upper part of the pharynx more attention should be paid to the diagnosis and life history of growths in this situation.

In January last he saw a case which struck him very forcibly. A patient came up complaining of slight blockage of the left side of the nose, with a little bleeding into his handkerchief, and some deafness in the ear. On examination a small reddish raised area was seen around the region of the Eustachian tube. A section was taken which proved it to be a squamous-celled epithelioma. Radium was inserted. A little later the same patient came with a slight bulging through the drum of the left ear and paralysis of the sixth nerve. A section from the ear showed an epitheliomatous formation, and during the month which had elapsed between the two visits the growth had spread down the Eustachian tube. Two months later paralysis of the right external rectus began and pressure on the sympathetic, resulting in exophthalmos. A few weeks later the patient died in great agony from pressure on the fifth nerve.

The lesson of this case appeared to be that a growth in the upper part of the pharynx ran through the soft spongy bone and through the foramen of the base of the skull with great facility, and easily affected the nerves at the base of the brain. These growths, therefore, unless seen very early, must remain inoperable.

With regard to growths lower down in the pharynx, he directed attention to a feature in the life history. These growths tend to spread beneath the muco-periosteum of the pharyngeal wall and to present themselves as a round smooth swelling covered with mucous membrane.

He agreed with Dr. McKenzie that diathermy had revolutionized the surgery of the naso-pharynx. He considered that diathermy should be combined with surgery in this region. In most cases it would be wiser to approach the pharynx from the side, utilizing the operative procedure suggested by Mr. Trotter of tying down the sterno-mastoid to the pre-vertebral muscles, at which operation the glands could be excised and the external carotid artery tied. He did not advise puncture of the glands with the diathermy needle, but for the inside of the pharynx this treatment was invaluable.

He did not consider a fistula leading from the pharynx important; such a fistula often formed when diathermy had not been done, but it always healed up.

Mr. Norman Patterson

said he had had many cases of naso-pharyngeal fibromata, and for four or five years he had ceased operating on them, as treatment by radium alone, or combined with X-rays, effected a cure. Dr. Logan Turner would remember a case of a naso-pharyngeal growth extending into the zygomatic fossa and passing beneath the zygoma into the temporal fossa. The man came to London, and he (Mr. Patterson) removed the growth, but the patient died an hour later from shock. He treated a similar case with radium at the London Hospital and the recovery was perfect; and he had had other cases cured in this way.

He did not advocate the use of diathermy in the neck. In carrying out dissections for malignant glands he nearly always removed the sterno-mastoid. There was no fascia or muscle left to cover in the vessels. A complete clearance of both anterior and posterior triangles was necessary. In only one case had he found it advisable to occlude the lateral sinus before embarking on the neck dissection.

Dr. J. Aldington Gibb

said in such cases he went round the fauces with a diathermy knife, and afterwards charred with a button. A lady, aged 65, had a malignant growth on the posterior wall of the pharynx and in the tonsillar region. The posterior wall growth was movable, and he stripped it off with the diathermy knife.

There was a case he had shown here, of extensive cancer of the tongue and fauces, and the patient was returned as inoperable by Mr. Howell Evans. The speaker diathermized it, and there was now no scar tissue remaining in the tongue. The floor was involved to the alveolus, and he charred it rapidly with the diathermy button. The man was shown at the last meeting of the Section, and it would scarcely be realized that there had been any growth. Shrinkage of glands in the neck after diathermy was very noticeable, and now they had almost disappeared.

He asked whether the experience of operators had been the same that there was a blocking of lymphatics, or was it the development of something which was

let loose as a result of the extreme heat of the diathermy? In the case of the man shown here, the glands still remained free, though it was now six months since the operation. On analysis, lecethin lipoid was found to the extent of four grains of the pure substance, and there was a large amount of fat which did not give the reaction of animal fat. He was wondering whether lecethin lipoid might have some effect on cancer growth.

Dr. W. S. Syme

said he was interested in hearing the results of treatment of these cases by radium; his own view agreed with that of those who said the radium treatment of these growths was not hopeful.

Another important point was that of diagnosis, especially from the side of the ear. The purple appearance of the membrane, associated with pain on that side, made one suspicious of malignant disease.

He did not think a hard-and-fast line of treatment could be laid down for fibromata of the nasopharynx: each case must be judged by itself. He had had a case of a large fibroma in a child aged 6, and he removed it completely behind the palate without doing anything to the palate or a preliminary rhinotomy. Another case he approached through the antrum—a very direct route; it gave a direct view of the side of the nose and of the side of the nasopharynx.

He would have been glad to hear more as to the after-history of the cases reported by Dr. McKenzie, who had been using diathermy a long time. For malignant disease of the pharynx, he (the speaker) agreed that diathermy was the most beneficent method; even in manifestly inoperable cases, much could be done to secure comfort for the patient. He did not agree with removal of the glands before attacking the primary growth, because frequently after removing the primary growth the glandular trouble diminished, owing to the removal of sepsis, and thickening of the cellular tissue disappeared. It was possible to remove the glands some time afterwards more easily.

There was room for an extension of the method of diathermy farther down the pharynx, and even in the œsophagus. He had applied the method through the œsophagoscope to the œsophagus, and though one did not look for cure, one patient was decidedly benefited. Naturally very great caution was necessary.

Mr. Norman Patterson

said he had found the best route of approach was through the palate, but not entirely by splitting the soft palate, as that was not sufficient. The soft tissues of the hard palate were incised in the middle line, the flaps were elevated, and a large part of the hard palate and the posterior part of the septum were removed. Little or no deformity resulted. It was important not to split through the centre of the uvula, but to one side of it, as the incision was shortened and made through more vascular tissues.

Mr. Harmer was the first to use diathermy in this country, and, after him, Dr. Lambert Lack, in 1913. The speaker assisted Dr. Lack with his first case.

Mr. A. D. Sharp

said it was very advantageous to use radium in a case of nasopharyngeal fibroma before operation. In the case of a young adult, who had all the features of nasopharyngeal fibroma, he had used radium on two occasions, with

the result that the tumour was reduced in size by half; and the patient stood examination with the probe without the occurrence of serious bleeding, which had not been the case before. He was able to remove the tumour by means of forceps and snare, and though at the operation bleeding was severe, it was controllable. He had great difficulty, on account of the rubber-like consistency of the tumour, in getting small radium tubes to remain in the tumour, the tumour seemed to push them out again. He had therefore to put them in through the nose and pack the nose with gauze.

Mr. F. Sydenham

asked how long the radium tubes were allowed to remain in position, and whether there was any danger of their falling out. Was it likely that there was any risk to the air passages?

Dr. W. Hill

said that at the Manchester Radium Institute they often employed the American method of embedding, merely glass-screened, radium emanation "seeds" in malignant growths of the buccal cavity and throat, and when these got loose and were swallowed apparently no harm resulted. He had had a personal experience of a string-moored and metal-screened emanation tube containing the equivalent of nearly 50 millicuries of radio-activity breaking away from its moorings in the throat; it took two days to pass through the alimentary canal; its regular progress was watched at intervals by aid of the X-ray screen and the patient was none the worse for the mishap. A laparotomy however would have been necessary if its progress had been long delayed at any point, e.g., at the ileo-cæcal valve. It had been suggested that radium rays might possibly cause destruction of the hempen or silk or catgut ligatures used for mooring radium tubes.

Mr. E. D. D. Davis

asked whether Dr. McKenzie or Mr. Patterson had had cases of severe scarring after diathermy. He had seen three patients who had a large growth at the base of the tongue and pillars of the fauces removed by diathermy, and the scars were very painful and extensive, making swallowing difficult; trismus occurred, and food collected in the cavity left by removal of the growth. The pain and disability were so great that the patients were truly miserable.

Sir William Milligan (in reply)

said that his object in bringing the subject forward was to ascertain what it was best to do in the surgery of these tumours, or whether any surgical interference should be attempted. He was not very particular as to what route of approach was chosen, so long as the approach was adequate. Much of the surgery of the back of the nose had been done somewhat in the dark; the region had not been approached sufficiently fully and clearly. Free access was very important, not only if one intended to remove the tumour, but even if one was to manipulate it with radium.

He agreed with Mr. Sharp as to preliminary irradiation; for these tumours, whether innocent or malignant, were very vascular, and arrest of hæmorrhage by irradiation was very valuable; it also diminished the size of the growth, rendering the choice of operation easier.

When radium was used these tumours were implanted with a barrage of spicules, i.e., tubes containing radium emanation, not the actual salt. Exposure of the tumour was necessary so as to get the spicules deeply embedded and arranged symmetrically, in that way ensuring cross-firing in all directions. The spicules were retained from twelve to twenty-four hours, the time and number varying with the size and character of the tumour.

With regard to what became of the spicules, they either become encysted in the fibroid mass which resulted from the irradiation, or they came out and were either swallowed or coughed up. He had never seen any trouble follow after using these spicules in laryngeal growths. For laryngeal growths it was unquestionably better to do a laryngo-fissure, so as to secure free access to the growth and a clear route for the tubes.

Dr. Hill had referred to another method of using radium in the naso-pharynx, and he (Sir William) would refer to still another. Passing the tube through the nose he did not think was a good plan. There was a method named after Burwood. In this, the radium tube was encased in a little almond-shaped piece of wax, to which a silk thread was attached, and in that way it was drawn up into the naso-pharynx and tied. The irradiation used was from the beta- and gamma-rays. The spicules were made in the laboratory by a boy whose duty it was to do this. They were glass tubes, which were pulled out until their diameter was 0·03 of a millimetre. Each tube was then filled and cut into segments with a blowpipe, and twisted off.

He thought the secret of success in regard to these growths was the way in which they were exposed, whether doing a surgical operation, or trying to get rid of them by radium or by diathermy.

Dr. Dan McKenzie (in reply)

said that for the pain after operation he usually gave a little morphia; in some cases there was very little pain. Swallowing movements were quickly accommodated to the new conditions, and no special method of feeding was called for. He always ordered a throat spray, but more as a placebo than anything else, as the wound was sealed aseptically.

With regard to scarring, he had had a case in which there was a suspicious growth on the inner side of the cheek, and he removed the inside of the cheek without damaging the exterior, and the scar contracted so much that the patient could not open her mouth, but Dr. Hernaman-Johnson, who had sent him the case, applied X-rays to the scar, and in two or three months one would scarcely have thought there had been a scar there at all.

The interval between the two operations had varied in his cases from a few days to two or three weeks. He preferred to do the outer one first; in order to be able to remove the primary growth of the pharynx as thoroughly and deliberately as possible.

Mr. H. J. Banks-Davis (Chairman)

said that a number of years ago he sent some cases to Dr. Walsham of chronic adhesive otitis media, which had post-suppurative scarring of the tympanic membrane, to see whether he could improve the hearing, but he believed the X-rays made no difference. He did not see why the rays should not have an effect on the tympanic membrane.

Mr. E. D. D. DAVIS

said that in one case in which part of the tongue and fauces were removed the scar was treated by X-rays for three months and then the patient threatened to commit suicide. The rays had no effect on the scarring nor on the pain.

Mr. A. L. YATES

said the effect of X-rays was enhanced when the scar was a recent one, but the rays had very little effect on old scars.

A Preliminary Note on the Results obtained in Some Experiments in which the Facial and Recurrent Laryngeal Nerves were Anastomosed with other Nerves.

By Sir CHARLES BALLANCE, K.C.M.G., C.B., M.V.O.

(ABSTRACT.)

[This paper is published *in extenso* (with illustrations) in the *British Medical Journal*, August 30, 1924, p. 349.]

THE results of the experiments I am about to describe have been controlled not only by myself but by several friends. Mr. Colledge has reported on the condition of the vocal cords; Dr. Lionel Bailey has been responsible for the reports on the electrical conditions of muscles, and Dr. Eidinow has made radiographic observations on the condition of the diaphragm, after interference with the phrenic nerve. Sir David Ferrier has been present at many of the experiments and his advice has been of great help to me. Before the late Professor Shattock became too ill to attend the experiments he was always present. Dr. Dale, of the National Research Institute, has also been of the greatest assistance to me.

The results presented to-day must not be looked upon as final. Many experiments are in progress, and every experiment seems to open out the need for further experiment to elucidate some point which is not quite clear.

(1) *The Recurrent Laryngeal Nerve.*—When this has been injured or pressed upon by growths, the vocal cord on the same side becomes paralysed. Before the war, at St. Thomas's Hospital, a case was referred to me with paralysis of one vocal cord. I attached the recurrent laryngeal nerve to the side of the vagus nerve, thus making an end-to-side anastomosis. The operation was done in hospital, the patient disappeared, and I do not know the result; but from the experiments I have since done, I can guess the result in this case.

Two of the experiments—illustrated by the diagrams shown—are end-to-side anastomosis of the recurrent laryngeal with the vagus; and end-to-end anastomosis between the descendens noni and the recurrent laryngeal. When I speak of a result of an experiment it is really the result of several.

With end-to-side anastomosis of the recurrent laryngeal with the vagus, it was found there was no movement of the vocal cord at all in tranquil respiration. What it is desired to obtain is physiological movement during tranquil respiration. At the end of six months partial asphyxiation caused a movement of the vocal cord, that is, the cord slightly abducted on inspiration. At the end of fifteen months, when the wound was reopened and the vagus was stimulated above the point of anastomosis, the vocal cord abducted, but at the end of eighteen months there was no movement on tranquil respiration and no movement on partial asphyxiation. This shows there was a definite degeneration of function, from the period three months previously, when partial asphyxiation

caused abduction of the cord. At the present time the wound in this monkey has not been reopened and the vagus has not been stimulated again above the place where the recurrent laryngeal was anastomosed to it. So I cannot answer the question as to whether with such stimulation there would still be abduction. The laryngeal muscles are probably becoming shortened and atrophied from non-use and the operation has been a failure.

I may mention another point about the vagus nerve. It consists of many bundles of nerves, and each bundle probably has a definite function to fulfil. The little bundle of fibres which forms the recurrent laryngeal is quite distinct from the rest of the vagus nerve. In an anastomosis between the vagus and recurrent laryngeal nerves it is a matter of chance whether the recurrent laryngeal nerve is attached to that part of the vagus where the recurrent laryngeal nerve bundles are situated. In several instances I have attached the recurrent laryngeal to the tracheal side of the vagus, but I have never yet succeeded in obtaining movement of the vocal cord in tranquil respiration as a sequence to the anastomosis. I think the main reason is that I do not hit upon the little group of nerve bundles which are a continuation of the recurrent laryngeal in the vagus nerve.

A similar result takes place when end-to-end anastomosis of the descendens noni with the recurrent laryngeal is performed. After about three months the appearance of the cord becomes normal, it is no longer slack, it is straight, and its tone is good. After five or six months, if the animal is partially asphyxiated an abduction movement of the vocal cord is seen. At the same time if the wound is opened and the hypoglossal, the descendens noni and the point of anastomosis are stimulated there is an abduction of the cord. This experiment does not, therefore, seem the way of getting physiological movement of the vocal cord.

As the vagus and descendens noni nerves failed to produce the desired result other nerves which might be used had to be considered. The nerve to the serratus magnus was excluded, because the muscle is only used, if at all, in forced respiration. The second intercostal nerve might possibly be brought up through the upper opening of the thorax and joined to the divided recurrent laryngeal; but that seemed to be rather a complicated operation.

Then it occurred to Mr. Colledge and myself that, besides the recurrent laryngeal nerve, the phrenic nerve functions in tranquil respiration. The diaphragm moves rhythmically with the vocal cord and hence it was determined to use the phrenic nerve. The phrenic was divided, and the recurrent laryngeal was united end-to-end with the phrenic. Then it was desirable to arrange for the recovery of the diaphragm; and so the distal end of the phrenic nerve was united end-to-end with the proximal cut end of the descendens noni nerve. As the latter was cut, we had to think as to how we could prevent atrophy of the depressor muscles of the larynx and hyoid bone. So the distal cut end of the descendens noni was anastomosed end-to-side to the hypoglossal. I may tell you at once that these depressor muscles of the hyoid bone are functioning, and the electrical reactions are normal; and Dr. Eidinow reports that the diaphragm is working normally through the descendens noni. How it does so I do not know, perhaps Sir Frederick Mott can tell us. And as to the important anastomosis between the recurrent laryngeal and the phrenic, on examining the vocal cords at the end of a few months we could see that both were acting harmoniously. A short time later we found that the vocal cord which was being energized through the phrenic nerve was working much too vigorously, though synchronously with the other cord. I suppose it is natural

that it should be so, because the nerve current flowing along the phrenic to the diaphragm must be very much larger than that passing along the recurrent laryngeal. This phrenic-recurrent laryngeal anastomosis is a fundamental experiment; and various experiments are now in course to determine the best way of anastomosing the recurrent laryngeal to the phrenic in order to recover normal movement of the vocal cord. It is obvious that the whole phrenic is not required.

There is still much to do. All the anastomoses are subjected to microscopical examination. Monkeys are difficult to keep alive; sometimes a lethal epidemic arises, and then the experiments have to be begun over again.

(2) *The Facial Nerve.*—In 1895 the first anastomosis was done for the relief of facial palsy. That operation was an end-to-side anastomosis of the facial into the spinal accessory. The result of these spinalis anastomosis cases was practically and universally this: that recovery of the face was obtained after many months, but movement of the face was associated with movement of the shoulder: thus, if the shoulder was moved, the whole side of the face twitched. This was very marked and disfiguring, especially in a patient who would laugh and talk. It is important to remember that in doing a nerve anastomosis we must try to stick to one objective. I had tried to cure the face and not interfere with the sterno-mastoid and trapezius; and I failed. Whatever form of facial-spinalis operation is done, there always follows some weakness of the sterno-mastoid and trapezius; and in women who wear low dresses the deformity of the neck is very noticeable.

Under these circumstances it was natural that surgeons should look out for some other nerve to anastomose to the facial. The next nerve which was employed was the hypoglossal. I read a paper on facial anastomosis at the German Surgical Congress in 1902, and Professor Körte read a paper on the same subject. Each of us described one case of facial-hypoglossal anastomosis, and the result in each case was good. End-to-side anastomosis of facial and hypoglossal was at first done; and then afterwards, when the result was not perfect, the surgeon turned back the whole of the hypoglossal, making an end-to-end anastomosis. In order to save the tongue from atrophy, a slip was taken from the spinal accessory and brought across the neck and united end-to-end to the distal segment of the hypoglossal. Another operation, of which I show a diagram, was descendens noni-facial anastomosis end-to-end.

Eighteen months ago I was able to invite some friends who are present to-day, to an operation on a patient in whom the whole hypoglossal was turned backwards so as to be united end-to-end with the facial; and the divided descendens noni was united end-to-end with the distal part of the hypoglossal. In this case there has been a remarkable, and nearly a complete, recovery of the face, but there is still an associated movement of tongue and face, but this is becoming less and less. At dinner (for some months) this patient, as she ate and swallowed, suffered considerable movement of the side of the face, but this is now gradually disappearing.

The question was considered, what other nerve could be used for anastomosis with the facial? Facial-glosso-pharyngeal and facial-lingual anastomoses were carried out experimentally. Here I show a diagram showing the glosso-pharyngeal nerve to its termination beneath the hyoglossus muscle, where it divides into branches of distribution to the back of the tongue. It was turned back and united end-to-end with the facial nerve. No branches of the glosso-pharyngeal are interfered with except the sensory branches going to the circumvallate papillæ and the side of the tongue. The result of facial-glosso-pharyngeal anastomosis is early recovery of the paralysed face muscles.

The glosso-pharyngeal is connected with the facial nerve in various ways. I think these communications must have something to do with the good results which have been obtained from the anastomosis between the glosso-pharyngeal and the facial nerves.

There is a great deal more to be said, but the two points needing special emphasis are the following: (1) I think the glosso-pharyngeal is the nerve which should be used by surgeons for the recovery of motion in the face in cases of paralysis. (2) I think that some portion of the phrenic is the proper nerve to use in order to obtain recovery of movement in paralysis of the vocal cord.

DISCUSSION.

Sir DAVID FERRIER, F.R.S., said he had admired the technique of Sir Charles Ballance's operations. He had also been able to study the functional results, and he could verify all that Sir Charles had said. The last operation—anastomosis of the glosso-pharyngeal with the facial—seemed to be the most interesting of all, and the results were very satisfactory.

In operating on the small nerves of the monkey and lower animals, he thought it would be well if some method could be discovered of keeping the cut ends in accurate apposition without the use of sutures, as it seemed to him the latter must damage both the proximal and the distal ends, and so at least delay recovery. On his (the speaker's) suggestion, Sir Charles did use some glutinous material for the purpose, composed of finely pounded muscle.

Sir FREDERICK MOTT, F.R.S., said he examined the animals, and was unable to see any movement of the vocal cord. He was impressed with the success of the anastomosis of the phrenic. He thought he could understand it. One must think that the abductors of the vocal cords had association cells connecting the nucleus of the phrenic with the abductor nucleus. But what had to be considered was, why did Sir Charles carry out the operation? A person with a paralysed recurrent laryngeal could breathe satisfactorily, after anastomosis of the recurrent laryngeal with the vagus, but he could not speak, and the question was as to whether vocalization could be restored by anastomosing the phrenic. This monkey could make a noise, but a gibbon was a better animal for the purpose, as it had a remarkable larynx. Its thyro-arytenoid muscles had fibres inserted into the vocal cord (vocal muscle), and this might explain the remarkable vocal powers of this animal, which could imitate the bark of a dog or the shrill notes of the guinea-pig. He suggested that Sir Charles should make experiments on the gibbon.

He once did an experiment on a monkey, removing all the area of the cortex on both sides, which by excitation yielded movements of the vocal cords, and he had scarcely finished the operation when he was told that the animal was now barking quite as well as it did before. Barking might be represented at a lower level.

He had seen, that day, a lady who had had a considerable amount of movement restored by anastomosis of facial and hypoglossal, but he was disappointed because he did not think the tongue had recovered to the extent one could have wished. Unless there was much more return, it was a question whether it would be wise to do the operation.

One experiment Sir Charles showed was very satisfactory, namely, anastomosis of the facial with the glosso-pharyngeal, because the muscles on what was the paralysed side responded very well indeed, and he thought that in a short time the response on the two sides would be equal.

Dr. W. S. SYME referred to the case of a man whose vagus nerve was destroyed by a war injury. When he saw the man, there was paralysis of the left nerve, and Professor Kennedy removed the cicatricial tissue round the vagus, and sutured the two ends. Two years later there was no return of function. Professor Kennedy had also done for him (the speaker) a successful anastomosis of the facial with the hypoglossal, but it took a long time for function to return. The ultimate result was however most satisfactory.

Sir WILLIAM MILLIGAN said that three years ago he had a case in which he anastomosed the facial and hypoglossal nerves, and the difficulty mentioned by Sir David Ferrier of uniting the ends of the nerves without a suture if possible, made him (the speaker) think of the following method. He wrapped the two nerve ends in gold leaf, and then tied a very thin silk thread at the upper end of one cut portion, and at the lower end of the other cut portion, so that there was a little traction on the two ends in the direction of keeping them together. He saw the patient nine months later, and there was a return of function, though not very good. The patient then went abroad. He asked Sir Charles Ballance's opinion of the method.

Sir CHARLES BALLANCE (in reply) said that in certain cases success had attended anastomosis without suture. A piece of muscle was removed from the animal being operated upon, cut very small, placed in a small sterilized mortar with the addition of three or four drops of sterilized salt solution, and it was pounded up so as to form a sticky mass. This sticky mass held the nerves in contact.

He had not been asked if he had done a glosso-pharyngeal anastomosis in man. He performed one on March 12 of this year (1924). This woman was operated upon sixteen years ago by a member of the Section of Otology, and since then she had had total facial paralysis on the right side. In June i.e., a month ago, Dr. Bailey reported, that the muscles on the paralysed side of the face reacted to a galvanic current, of the same strength of current as on the opposite side. On June 3, he (the speaker) was sure there was slight voluntary movement at the angle of the mouth, and of the orbicularis oris. That had seemed to be a hopeless case, but he thought the patient would recover as there was already great improvement in the tone of the face, and there was not now any running from the angle of the eye; there was almost the same extent of palpebral fissure on one side as on the other.

Referring to Dr. Syme's case of destruction of the vagus nerve, he, (Sir Charles), said that he had cut the vagus on one side in a large he-goat, and that he had failed to discover that the animal was afterwards inconvenienced in any way.

In one of his baboons he did an end-to-side anastomosis of the recurrent laryngeal with the vagus, and on the twentieth day afterwards it died of acute pneumonia. Though influenza was rife at the time, he could not help thinking that the operation had something to do with the pneumonia. He therefore thought that if the laryngeal nerve was to be anastomosed, the vagus nerve should not be used, because of the danger of some such complication. Chevalier Jackson had recently recommended the use of the vagus, but he (the speaker) disapproved of this, because of the experimental results.

In answer to Sir Frederick Mott, he would be pleased to receive a present of a gibbon if anyone would kindly send him one, and it seemed to him that if these two Sections would provide two or three chimpanzees and a gibbon, in a couple of years there might be agreement as to some definite course of action to adopt when it was desired to operate for facial palsy or for paralysis of a vocal cord.

The Clinical Importance of Nasal Sinus Infection in Mastoiditis.

By PATRICK WATSON-WILLIAMS, M.D.

(ABSTRACT.)

TWO cases are cited, one a boy of 11 with acute mastoiditis requiring a Schwartze operation, and in whom exploration of the maxillary antra at the time of the operation revealed an unsuspected antral suppuration on the side corresponding to the mastoiditis. The second case was a subacute mastoiditis in which operation was postponed only on account of a co-existing frontal and maxillary antral sinusitis. After opening and draining the infected nasal sinuses the mastoiditis cleared up and no antral operation was required.

Such experience induced me to examine cases of mastoiditis for intranasal sepsis with greater regularity, and I have tabulated the results of nasal sinus exploration in a continuous series of forty-two of my hospital cases in which nasal sinus explorations, with cultures of the contents extracted from the sinuses, were made.

We may note that, combining the forty-two Schwartze and radical mastoid operations, nasal sinus infection was found in thirteen on both sides, in twelve on the side of the ear operated on, in three on the opposite side of the ear operated on; in twenty-eight the sinuses showed infection. In other words the sinuses were found infected twice as often as they were found not to be infected.

Of the twenty-eight cases with sinus infection, in twenty-five the side of the nose corresponding to the operated ear was infected, and in three only was the infection limited to the opposite nasal sinuses.

These observations would be misleading if it were not that the cases are not a continuous series of mastoid operations, but are only those in the series of patients in whom the sinuses were explored, and the contents submitted to the bacteriologist. The sinuses of other mastoid cases, in which previous examination did not lead one to suspect nasal sepsis, were not explored. Moreover, these explored cases are mainly those in which careful examination of the nasal conditions before operation was difficult, viz., young children. Thus of the series of forty-two patients whose sinuses were explored no less than twenty-one were under 15 years of age; of these twenty-one children eight were not shown to have infected maxillary antra, while thirteen yielded pyogenic organisms.

Of the thirteen children, eleven were proved to have nasal sinus infection on the side corresponding to the operated ear; and of these no less than eight had the sinus infection on that side only—the nasal sinuses on the other side yielded no pyogenic infection.

It seems fair to conclude that a nasal sinus infection is a frequent determining cause of mastoiditis, and that the mastoid infection tends to correspond with the most infected side of the nasal sinuses.

It is not suggested that mildly infected nasal sinuses should always be opened and drained; indeed of the twenty-two children under 15 years of age, only once was a sinus opened and drained at the mastoid operation, and that was a left antrum in a child of 6, full of pus yielding streptococci and pneumobacilli.

It is my usual practice when exploring a sinus to follow up the washing out of each sinus explored by injecting a little disinfectant, such as collosol argentum or $\frac{1}{10,000}$ biniodide of mercury aqueous solution. Frequently this alone seems to be sufficient unless the infection is of long standing or virulent, and clinical experience apparently shows that if the sinuses are infected they may undergo spontaneous recovery when the major operation on the mastoid is performed. But when a definite and virulent septic nasal sinus is found it is desirable to open and drain it, if possible before or at the same time as the mastoid operation is done.

To investigate the nose and throat and examine for evidence of sepsis of tonsils and adenoids, or of the various nasal sinuses in recurring catarrhal or chronic purulent otitis, is, I believe, very usual, but in mastoiditis involving operation of a more or less severe character, the importance of careful regard to nasal sinus sepsis is less obvious, though at times so valuable.

Exploration of the antra and sphenoidal sinuses with a patient under general anæsthesia need not occupy more than two minutes, while if it seems necessary to open and drain, a few minutes more will be enough, and this may

go far in guarding the mastoid operative field from re-infection, apart from the lessened risk of a pulmonary septic infection following a prolonged general anæsthetic. And when, after a mastoid operation, recurrent purulent discharge is observed in the region of the Eustachian tube orifice, careful investigation for missed nasal sinus infection is called for, and in such cases exploration of the maxillary antra, sphenoidal sinuses and ethmoidal cells may reveal a source of infection otherwise difficult to locate.

It is noteworthy that the cases with unilateral nasal sinus infection occurred four times as frequently on the side corresponding to the mastoid disease as on the opposite side. And whether the Schwartze and radical operations be taken separately or added together the proportion is the same, e.g. :—

Schwartze	4 cases infected same side	
	1 case ,, opposite side only	
Radical operation ...	8 cases infected same side	
	2 ,, ,, opposite side only	
Total	12 cases infected same side	
	3 ,, ,, opposite side	
Further	28 ,, showed nasal sinus infection	
,,	14 ,, showed no sinus infection	

Many of the forty-two cases were in young children, and while the maxillary antra were explored in all the cases, the sphenoidal sinuses were explored in fourteen only ; in two cases the sphenoidal sinuses alone showed infection, in six the sphenoidal sinus infection occurred in association with infection of one or both maxillary antra.

The organisms recovered from the infected sinuses were pneumococcus, streptococcus and staphylococcus.

Results of Exploration of Nasal Sinuses in Forty-two Cases Submitted to Mastoid Operations.

	Analysis. Schwartze operation	Radical operation	Total
(1) Both sides of nose infected			
A A	2	5	7
A A and S S S	1	5	6
Total	3	10	13
(2) Side of nose corresponding to mastoid only infected			
A A	4	4	8
A A and S S S	0	4	4
Total	4	8	12
Total of (1) and (2)	7	18	25
(3) Side of nose opposite to mastoid only infected			
A A	0	2	2
A A and S S S	1	0	1
Total	1	2	3
Total of (1) (2) and (3)	8	20	28
(4) Both sides of nose sterile			
A A	8	3	11
A A and S S S S	2	1	3
Total	10	4	14

A A, Maxillary antra, right and left.
S S S, Sphenoidal sinuses, right and left.

Mr. H. S. SOUTTAR, C.B.E., F.R.C.S., showed a new form of Œsophagoscope, which was demonstrated by the Chairman (Mr. H. J. Banks-Davis) in the absence of the exhibitor.

Dr. W. S. SYME said he had had a man wearing one of these tubes three weeks on account of malignant disease of the lower end of the œsophagus, and he stated that he was able to swallow better in consequence.

Laryngo-fissure in a Case of Tuberculosis of the Larynx.

By Sir STCLAIR THOMSON, M.D., F.R.C.S.

THIS is the first time, in an experience of sixty-four cases treated by laryngo-fissure, in which a clinical diagnosis of epithelioma was not confirmed after operation.

The patient, J. U., aged 49, had been twenty-six years in the police before taking up work as a messenger. In May, 1921, his voice became husky and remained so until he consulted me on July 24, 1922—fourteen months later. He then weighed 12 st. 10 lb.—his usual weight. There was no cough, no sputum, and no dysphagia. He spoke easily in a rough voice. The air-passages were all normal except the left vocal cord. This appeared somewhat narrowed and was occupied in its whole extent, except at the posterior extremity of the vocal process, by a pink, knobby, irregular infiltration. There was also a slight dimpling on its inner surface and a decided sluggishness in movement when compared with the anterior half of its opposite cord.

It was noticed that no suitable portion could be removed for examination, and the condition was looked upon as typically malignant and suitable for laryngo-fissure if the anterior commissure was not invaded.

Laryngo-fissure was performed on August 8, 1922. The larynx was divided a trifle to the sound side of the anterior commissure; the growth occupied chiefly the inner surface of the left cord and stopped short of the vocal process. Before removal I felt it with my finger, and remarked to those present upon its softness, but I confess it did not even then strike me that it could be anything else but a malignant neoplasm.

The tracheotomy tube was left in for five hours.

Instead of an almost normal progress, his temperature was 100·5° and 101° the next day; his pulse full and slightly intermittent, and his pupils, although they reacted, were very dilated. Still, on the fifth day his temperature was normal and he went out for a walk.

I was that evening surprised to receive the following report from Dr. Eastes' Laboratory on the removed tissue:—

"The growth proves to be a chronic tuberculous lesion with typical histological features; it has ulcerated on the surface. The anterior and posterior edges of the tissue removed are free from tuberculous deposit. There is no sign of malignancy."

The sputum was examined several times, but no tubercle bacilli were found.

He was sent to the Sanatorium, at Midhurst, on August 26. Infiltration was then found at the apices of both lungs, but the temperature was always steady and tubercle bacilli were only found on one occasion. The fistula in the neck left by the tracheotomy tube was slow in closing. The endo-larynx on the left side remained indolent and covered with flat, moist granulations, and did not completely heal until the following March.

When examined last in October, 1923, patient weighed 13 st. 2 lb., felt in perfect health, and worked hard in his garden. The larynx is soundly healed; the new left cord does not move, although the left arytenoid is not quite fixed.

This case illustrates some of the difficulties of diagnosis, particularly in a middle-aged man, where there has been no loss of weight, no temperature, no cough and no sputum to examine.

The case is not shown for the purpose of recommending laryngo-fissure in laryngeal tuberculosis. Had it been possible to diagnose the case correctly in the beginning, the condition would have been healed more promptly and safely, and with a better voice, with the help of the galvano-cautery.

DISCUSSION.

Mr. TILLEY said that a case had been sent to him (the speaker) by a colleague, who said that the patient's chest was normal, but there was hoarseness. There was a localized lesion of the left vocal cord, which had been diagnosed as malignant. During the operation of laryngo-fissure he (Mr. Tilley) noticed it was a soft type of tumour. On the ninth day the incision in the neck broke down, granulations appeared, with suppurating spots which suggested tuberculous follicles. On the application of iodoform ointment to the wound it slowly healed. After two years the patient was again admitted to hospital and this time with a pleuritic effusion. He was tapped, and died after being in hospital two months. The post-mortem examination showed extensive tubercle of the lungs, and the vocal cord showed the typical tuberculous ulceration. The fixation of the cord was mostly due to infiltration of the corresponding crico-arytenoid joint. Microscopic examination of the "growth" removed at the time of the operation showed the typical giant cells of the tubercle.

Dr. JOBSON HORNE said a larger number of similar cases had occurred than was commonly known. Some years ago, when he had more time to give to research work, there passed through his hands portions of laryngeal tissue, and on one occasion an entire larynx, which had been removed under an erroneous diagnosis of malignant disease, and which had occasioned surprise when they demonstrated nothing but tuberculosis. Those contributions helped to confirm the conclusions he had formulated namely:[1]

> (1) That malignant disease was diagnosed more often than it existed. (2) That tuberculosis was the disease more commonly overlooked. (3) That the eye trained to recognize the changes in the larynx caused by tuberculosis would make very few errors in the diagnosis of laryngeal diseases. (4) That tuberculosis must be eliminated before arriving at a diagnosis of malignant disease.

One of the cases he had referred to was very instructive; not only in illustrating the errors the most experienced were liable to make, but more particularly in throwing light upon the conditions under which the adjacent cervical lymphatic glands might become infected in tuberculosis of the larynx.

The patient, a man, had a condition of the larynx typical of malignant disease, and suitable for removal by laryngo-fissure. That was done. Under the microscope the tissue removed showed histological features typical of an old lesion of tuberculosis.

Now lymphatic glands adjacent to the larynx did not (Dr. Horne said) in his experience become enlarged when the larynx was infected in the course of pulmonary tuberculosis.

In this case, subsequent to the laryngo-fissure, a gland adjacent to the lesion became enlarged. That naturally suggested an error in the pathological finding. The gland was removed; and a section showed typical histological structure of tuberculosis.

From closer inquiries it was then ascertained that some years previously the man had had lung disease and had recovered.

The conclusions Dr. Jobson Horne had arrived at were these; that, given a case of pulmonary and laryngeal tuberculosis in which the disease in the lungs and in the larynx had been completely arrested, if the old lesion in the larynx were aroused then the re-infection did not occur in the lung but in the adjacent lymphatic gland.

[1] "Introduction to a Discussion in the Section of Laryngology and Otology of the British Medical Association, July, 1907," *Brit. Med. Journ.*, October 12, 1907.

The case did not alter, but rather confirmed, his views (1) that tuberculosis of the larynx was always secondary to tuberculosis of the lung; (2) that the lung disease was never secondary to that in the larynx; (3) that when the disease in the larynx had become arrested the disease in the lung was already arrested; and (4) that primary tuberculosis of the larynx was a negligible quantity, excepting the conditions under which the larynx was the site of infection as had been described by him many years ago.[1]

Dr. P. WATSON-WILLIAMS said that it was sometimes impossible to obtain a satisfactory fragment to submit to the pathologist, whilst it was often possible to corroborate or controvert the clinical diagnosis by histological investigation before operating. It was well to note that the pathologist's finding might at times be misleading, as in a case in which he (Dr. Watson-Williams) had operated on the strength of the pathologist's report.

The patient, a man aged 50, had been hoarse for some months and there had been a nodular mass on the right vocal cord. A portion had been removed and the pathologist had reported that it was epithelioma, a diagnosis which had justified laryngo-fissure. The growth had involved the whole front part of the cord and the arytenoid, but had not been so hard as might have been expected. Microscopical examination had shown a chronic tuberculous ulcer, and three weeks afterwards the patient had died from acute pulmonary tuberculosis. Signs of long-standing tuberculosis had been found at the post-mortem examination.

Sir JAMES DUNDAS-GRANT said the case showed that laryngo-fissure in tuberculosis was not necessarily fatal. Before the larynx was opened, even in a case of "obvious" epithelioma, a small portion should be removed for microscopic examination, but to be of value this must represent the main mass. Sometimes there were other means of diagnosis; he himself had recently had a case in which glands were concerned. It had been referred to him as tuberculous fixation of the left vocal cord, but with considerable infiltration in the region of the arytenoid, bulging both into the larynx and outside it, not ulcerating through. He had diagnosed carcinoma. But *on the other side* there was a large softened gland of typically tuberculous appearance. The gland, however, was removed, and in its walls there was found squamous-celled epithelioma.

Sir STCLAIR THOMSON (in reply) agreed with Dr. Jobson Horne that these cases occurred more often than they were recorded. Cancer was more frequently expected than was tuberculosis. Laryngeal tubercle without pulmonary symptoms was more apt to occur in the elderly than in the young. His (the speaker's) statistics showed that tubercle in the elderly occurred more often in males than in females without pulmonary symptoms. He further agreed with Dr. Horne that in tubercle of the larynx it was quite unusual for neck glands to be enlarged.

His (Sir StClair's) case, and Mr. Tilley's case also, showed a general point of much interest with regard to shock in tuberculous patients. A man aged 80 was still hunting a year and a half after laryngo-fissure for cancer, and he had very little shock at the operation. But tuberculous patients, even when there was nothing to be detected in the lung, did not stand operation well. In tuberculous subjects all operations which were not absolutely necessary should be avoided. He (the speaker) always removed a small piece for examination when he felt satisfied that this would be sufficient to justify a conclusion. The example quoted by Dr. Watson-Williams had shown how a pathologist might lead one astray.

He had not kept this patient long enough under observation. There was no need of hurry to operate in these cases, and it would have been wiser if he had deferred operation until, at a later date, the symptoms had been further investigated and fresh ones had declared themselves.

[1] *Journ. of Laryng, Rhinol. and Otol.*, December, 1901.

Enchondroma of the Larynx.

By Irwin Moore, M.Ch.

Patient, a medical man, aged 55, was referred to me in August, 1923, during the absence of Sir StClair Thomson, complaining of loss of voice for four months, accompanied by a feeling of fullness on the left side of the neck —at the level of the thyroid cartilage. Three years previously he had experienced a sensation of a lump in the throat, and had consulted Sir William Milligan, who had been unable to find anything the matter, and had attributed the symptoms to a neurosis.

After an extraction of teeth under gas in July, the voice had returned for half-an-hour, and he could speak perfectly plainly.

On palpation of the neck, a hard, immobile swelling, the size of a pigeon's egg, was felt deeply situated on the left side, above the level of the cricoid cartilage. Laryngoscopic examination showed the left vocal cord fixed in the "cadaveric" position, and there was no movement of the corresponding arytenoid.

A recent X-ray examination with a bismuth meal, had shown no œsophageal obstruction, nor any indication of pressure on the recurrent laryngeal nerve. There was no history of syphilis.

In consultation with Mr. James Berry, on September 20, the question of malignancy of a portion of the left thyroid lobe was discussed. The possibility of an enchondroma was also considered.

On September 21 the tumour was exposed by Mr. Berry, by a lateral incision in the neck, with reflection of the left thyroid lobe, and it was found to consist of a hard, oval, encysted mass, about 1 in. in its longest diameter, springing from the left thyroid ala, and surrounded by a thickened investing membrane, consisting apparently of the external perichondrium of the thyroid ala. A diagnosis of chondroma was made.

On opening into the tumour, it was found to be of soft consistence, with the appearance, so well described by Waggett, of "sago pudding." Convalescence was somewhat delayed by deep-seated sepsis, otherwise the patient made an excellent recovery.

When seen on February 15, 1924, he stated that his voice had returned three weeks after operation—a result which he attributed to the lesson received from Mr. Cortland MacMahon, on October 30, consisting of perfected breathing and low speaking, i.e., producing the voice low down, by lowering the larynx by means of the sterno-mastoid muscles; he had obtained most improvement by singing long sustained notes at the piano. Following this treatment, his voice became practically normal in three or four days.

Examination of the larynx at this date, showed the left vocal cord in the same position of abduction, whilst the right cord had developed compensatory over-action, which accounted for the improvement in the voice.

Microscopical examination of the tumour by Dr. Ernest Shaw showed that it consisted of hyaline cartilage with large and small round cells, in parts free from cells and myxomatous in character—a simple chondroma.

DISCUSSION.

Mr. James Berry said that the main point of interest lay in the diagnosis. The age of the patient, together with the complete paralysis of one vocal cord, suggested malignant disease of the pharynx extending forwards into the larynx. On the other hand, the long duration (three years), the complete absence of dysphagia and of any

impairment of general health or nutrition, was against this supposition, as was also the fact that no disease could be seen or felt within the larynx or pharynx.

The swelling itself was ill-defined and deep-seated, and obviously connected with the side and back of the larynx rather than with the thyroid gland. A perichondritis of the thyroid ala could not be definitely excluded.

The operation consisted primarily in an exploration of the pharynx and back of the larynx from the outside. When it was clear that there was no malignant disease in this region, the diffuse swelling of the thyroid cartilage, which was greatly expanded and presented "egg-shell crackling," was exposed and the tumour removed by enucleation.

Sir StClair Thomson said Sir Felix Semon had regarded a fixation of the cord with a growth of the larynx as characteristic of malignant disease. This case showed the error of that generalization.

Recurrent Laryngeal Nerve Paralysis in a Case of Tuberculosis of the Lungs, with Calcareous Degeneration of Tubercular Tracheo-bronchial Glands.

By Irwin Moore, M.Ch.

Patient, a male, aged 52, was first seen by Mr. Dawson, in September, 1921, complaining of an irritating cough, attributed to septic tonsils. The tonsils were removed in February, 1922. The following May, patient complained of dysphagia, and could only swallow liquids. When seen again a month later, he complained of hoarseness, and this was found to be due to paresis of the left vocal cord, which had not been present at the first examination. In July, 1922, this cord was seen to be fixed in the "cadaveric position." The Wassermann reaction was negative. In July, 1922, X-ray examination of the chest was said to show no abnormality. Œsophageal examination showed swelling with marked pulsation on the left side of the œsophagus in the cardiac region.

Following this examination the dysphagia improved somewhat, for a short time. In October, 1922, the lungs and great vessels were reported to be normal, and the explanation of the pulsation was attributed to the subclavian artery being near the surface.

When again seen in January, 1923, patient had become very thin, suffered from cough at night, and complained of weakness.

When I first saw the patient (May 19, 1924), in view of the unsettled diagnosis and the importance of accounting for the recurrent nerve paralysis, I sought the assistance of Dr. Halls-Dally, and the co-operation of other members of the Mount Vernon Hospital staff with the following result:—

On physical examination of the chest, signs of long-standing tuberculosis of the hilar glands were found. These were confirmed by X-ray examination; the glands were found to be calcified. In the apices of the lungs there was evidence of *slight* tubercular activity, especially on the right side, also not of recent origin. The heart was normal in size, shape, position and sounds, and the great vessels at the base showed no broadening, or evidence of aneurysm. The opinion was expressed that the paresis of the left recurrent nerve was probably inflammatory in origin. A second X-ray examination, following three days' treatment with atropine, showed some obstruction to the passage of a barium meal at the level of the cricoid cartilage, only small quantities passing, and after considerable delay. [X-ray pictures were shown.]

It was suggested that a small ulcer—possibly tubercular—causing pain and consequent spasm, produced the difficulty in swallowing, without any active

obstruction. Œsophagoscopic examination confirmed the swelling in the œsophagus, but beyond some congestion confined to a small area, no ulcer could be found.

I consider that the recurrent laryngeal nerve has become involved in a mass of calcified hilar glands, which press against the œsophageal wall and are represented by the swelling seen on œsophageal examination, and that the marked pulsation is transmitted from the great vessels through this mass.

Note.—This case was previously shown at a meeting of this Section by Mr. Dawson (publication being deferred for further clinical investigation).

I am indebted to Mr. Dawson for the early notes of the case, and I have to thank Dr. Halls-Dally and others for much help in forming the diagnosis.

Sir StClair Thomson said that tuberculous paralysis of the recurrent laryngeal, while not common, was commoner than was generally supposed. At the Sanatorium he had seen eleven cases of it in ten years. They occurred on the right side as well as on the left. When on the right side it was not so likely to be due to enlargement of tracheo-bronchial glands, and then the cause was likely to be thickening of the pleura.

Recurring Nasal Polypi in an Infant, aged 4 months.

By Irwin Moore, M.Ch.

Patient, a male infant, aged 4 months, was admitted to hospital on December 10, 1923, suffering from obstruction of the right naris, which prevented proper feeding.

The previous history was that a polypus had been removed from the right nostril by Dr. Scatliff of Brighton two months before, followed by a recurrence three weeks later. A section examined by Dr. Rainy Newth had shown a polypoid growth, with lining membrane transition from squamous to columnar cells. Although there was some epithelium in the deeper part, there was no definite evidence of malignant tendency.

When first seen by me on December 14, 1923, the right naris was occluded by a large polypus, which presented at the orifice.

Skiagrams by Dr. Allchin gave no definite information as to the condition of the maxillary antra, which were quite rudimentary, and nothing abnormal could be seen in the detail.

When the polypus was seized with forceps, it ruptured, and a quantity of serous fluid escaped; the sac and pedicle were afterwards removed; the nasal cavity was then perfectly clear.

Microscopical section showed that the growth was composed of œdematous tissue containing a few mucous glands and patches of small, round-celled infiltration. It was a simple polypus without any evidence of malignancy.

On January 31, 1924, the child was again admitted to hospital with a recurring polypus of similar consistence in the same nostril, which was removed, the patient being discharged on February 4. It was later reported by the family physician that five days after returning home the child developed broncho-pneumonia, from which it completely recovered. Two months later (March 4) convulsive attacks began, affecting chiefly the left side of the body, and lasting three days, the child dying in one of these attacks. At this time the right naris was completely occluded by another polypus. The cause of death was stated to be, primarily, meningitis, following on otitis media sup-

purativa, and secondarily, nasal polypi. Unfortunately no autopsy was held. It was later ascertained that while the infant was in hospital at Brighton there was slight discharge from the right ear, but no indication of ear trouble was present on December 14, or early in February.

This case is recorded on account of the great rarity of nasal polypi in children under 10 years of age, and especially at such an early age as four months.

A Direct Laryngoscopic Spatula.

Shown by IRWIN MOORE, M.Ch.

A DIRECT laryngoscopic spatula with interchangeable proximal and distal lighting, designed by the exhibitor from the autoscope of Kirstein. With this instrument a complete and direct view of the larynx and deep pharynx may be obtained, as with the suspension apparatus. Owing to the absence of any surrounding tube, the removal of growths of the larynx is greatly facilitated—as compared with the ordinary tube spatula.

Oesophageal Pouch in a Man aged 39.

By F. J. CLEMINSON, M.Ch.

PATIENT, aged 39, first seen on February 25, 1924.

History:—In June, 1922, while motor cycling, he noticed that his voice was suddenly altered, so that he found it "a strain" to make his passenger hear. This alteration was transient but has recurred many times since. There is no association of these attacks with any acute infection in the nose or throat. He next noticed, particularly at night, that curious uncontrollable noises occurred in his throat, and he was occasionally conscious of a sensation of constriction below the larynx. Later still, he found that particles of food regurgitated into the back of the throat. By an effort he was able to bring these up into his mouth. (Food so returned was never of a stale character, but had always been recently taken.)

On examination: Nothing could be seen by the mirror, but noises could be heard through a stethoscope placed just above the inner end of the right clavicle. Water was given to the patient to drink and as it was swallowed a laryngeal mirror was held in position to show the hypopharynx. Pressure on the neck above the clavicles was then made and water was seen to regurgitate from behind the arytenoids.

[Radigrams taken by Dr. Coldwell are exhibited showing the size and situation of the œsophageal pouch.]

DISCUSSION.

Dr. IRWIN MOORE advised leaving the case alone. He (the speaker) had been consulted many years ago in two early cases, both in middle-aged women, and it was hoped that no operation would be recommended. He had advised slow eating and frequent small meals, and as a result he understood they had passed through life comfortably without any operation.

Sir STCLAIR THOMSON said he thought this must be the youngest case of the kind on record. It was unusual in people under 45 years of age. A man aged 39 ought to be operated on, and the sooner the better. He had watched one lady with the condition for fifteen years—55 to 70—and she wasted to a shadow. The operation was now much safer than fifteen years ago.

Dr. BROWN KELLY said he had had at least twelve of these cases. He thought that this man should be watched to see whether the pouch enlarged quickly; if it did it should be operated upon. In the meantime the best treatment was the washing out of the pouch. Of his (the speaker's) own patients only one had had the pouch removed. Of the others only one had died from inanition due to the pouch; she was about 85 years of age. Several others had died, but from causes unconnected with the pouch. Most attained an advanced age, which he attributed to the slow eating and very careful mastication necessary in this affection.

Dr. IRWIN MOORE reminded Members of the fatal case which he had previously recorded before the Section (specimen now in the Museum of the Royal College of Surgeons) in which a pouch, at its lower part consisting of only a mucosal layer, was perforated by a bougie. A bougie, if passed in such cases, almost invariably entered the pouch and not the œsophagus.

Mr. CLEMINSON replied that the X-rays showed only a moderate-sized pouch. His idea had been to wait and watch the pouch. If it increased in size he would do the operation recently described by Mr. Lionel Colledge in the *Lancet*.[1] At the present time it was not causing much inconvenience; the patient was chiefly concerned about the change in his voice. When the pouch was filled with bismuth, the impression obtained, owing to the weight of the bismuth, was that it was larger than was actually the case.

A Case of Bilateral Abductor Paralysis in a Child 1 year and 8 months old.

By MICHEL VLASTO, F.R.C.S.

THE child was taken to a London hospital when a year old with the diagnosis of diphtheria. A tracheotomy was performed. Stridulous breathing began three months ago, and has persisted ever since. At present there is a loud inspiratory stridor. The voice is clear.

Direct examination of the larynx showed a bilateral abductor palsy. (Drawings shown.)

The child's Wassermann is strongly positive, and so is that of both father and mother. Members of the Section are asked for suggestions as to treatment.

DISCUSSION.

Dr. BROWN KELLY suggested that this was a case of spasmodic adduction.

Dr. IRWIN MOORE suggested that this case might be a suitable one for intubation, in preference to tracheotomy.

Sir JAMES DUNDAS-GRANT said that he had cured a little child by means of intubation, using a tube suitable for a child several years older.

Mr. E. D. D. DAVIS said that intubation was a very troublesome procedure; the tube had to remain in for a long time, causing ulceration which would be followed by cicatricial stenosis, and the patient was then much worse off. He advised Mr. Vlasto not to use an intubation tube.

Mr. VLASTO (in reply) said that he did not feel inclined to practise intubation, partly because he had had very little experience of this form of treatment, and partly because of the danger of leaving the child unattended for an indefinite period. The stridor did not disappear or improve under the anæsthetic; he had satisfied himself that the stridor was present before the tracheotomy. Dr. Sidney Owen, who had examined the child from the neurological point of view, had thought this might be one of those rare cases of abductor paralysis following diphtheria.

[1] *Lancet*, 1923, ii, p. 1237.

Larynx removed for Basal-celled Epithelioma, Patient still under Treatment. (Microscopical Section shown April 4, 1924.)

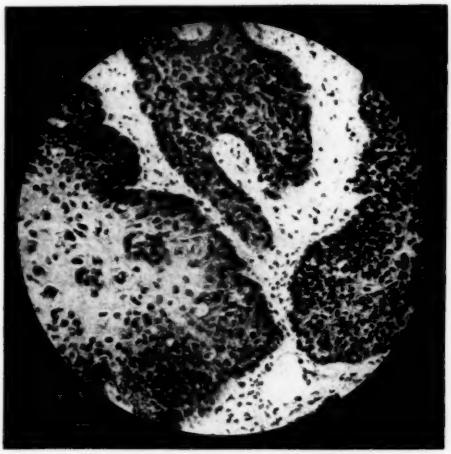
By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

DESCRIPTION OF LARYNX.

THE specimen consists of the entire larynx including the epiglottis and the surrounding tissue. It is cut close to the posterior margin of the hyoid bone.

Front.—A rounded mass, the size of a hazel-nut, projects through the notch of the thyroid cartilage. The left ala projects outwards and the upper margin is fused with the dense morbid tissue extending to the hyoid bone.





Section from the epiglottis (showing basal-celled epithelioma).

FIG. 1.—Low power. FIG. 2.—High power.

Above.—Projecting from the dense growth upwards and backwards is a fungating mass connected with the right half of the epiglottis from which the portions necessary for microscopic examination have been removed (*see* figs 1, 2). The left two-thirds of the epiglottis is much swollen, the superficial part presenting an œdematous appearance. The left attached portion of the epiglottis is distorted inwards by a mass of new growth between it and the thyroid cartilage.

Behind, the left ary-epiglottic fold and its surroundings, as also the ventricular band, are enormously enlarged and infiltrated with new growth, the superficial parts of which are pale. The right ary-epiglottic fold is only moderately enlarged. The vocal cords are well preserved and there is no growth below the cords.

Below. — The cricoid cartilage, the trachea having been completely removed.

Branchial Cyst in Fauces.

Shown by FREDERICK SPICER, M.D.

This cyst was removed from the posterior pillar of left fauces of a man aged 41. It was as big as a fairly large grape, had a thin wall, and contained a thick yellowish fluid. There were no symptoms. It was discovered by chance by his doctor.

Pathologist's Report.—"A section of a cyst wall lined with stratified squamous epithelium, probably a developmental cyst from a branchial cleft."

Case of Distortion of the Larynx producing Tracheal Obstruction due to Cicatricial Contraction following an Abscess.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

PATIENT, a male, aged 35, shown on April 4, 1924.

The case was operated upon by Mr. Roberts on May 9; he excised the sunken cicatrix behind the right sternomastoid; a median incision was then made and the thyroid cartilage exposed; the left sternohyoid muscle was found dislocated to the right, covering the notch of the thyroid cartilage. On the right side there was a firm dense fibrous band attached to the hyoid and thyroid, pulling these down to the right; this was divided on a grooved director and the cut ends separated about half an inch. Two other fibrous bands were found below the one first mentioned and they were also divided. When this was done the tilting of the larynx was rectified and the left sternohyoid muscle sutured in its normal position. These bands corresponded to the omohyoid and other subhyoid muscles which had evidently undergone inflammation and fibrous cicatricial change. While they were being cut there was a cracking sound like that accompanying tenotomy of the tendo Achillis.

The anæsthetic was administered by the intratracheal method, the distortion of the larynx and the immobility of the right vocal cord making it extremely difficult to introduce the tube.

The next day there was considerable breathlessness and depression, but the larynx was seen to be symmetrical in position and both vocal cords were visible, the left one moving very slightly, both, however, being much congested and swollen. Tracheotomy was performed and the tube was kept in for a few days, when it was coughed out and the patient breathed sufficiently well to render its re-introduction unnecessary.

Case of Chronic Circumscribed Labyrinthitis, with Healed Fistula of the Superior Semicircular Canal.

By ERIC WATSON-WILLIAMS, M.C.

F. J. G., MALE, aged 31, bootmaker.

History.—Old-standing double otorrhœa. Left radical mastoid operation, 1920; no complications, healing good; this is now the "good" ear. Right radical mastoid operation, 1922: the ear healed, but after two months discharge recurred, and vertigo was experienced. These troubles were very

severe in October, 1923, but during this year there has been marked improvement.

Description.—Three phases are observed :—

(1) Latency : there is no vertigo, nor spontaneous nystagmus.

(2) Activity : there is a sense of unsteadiness, and spontaneous nystagmus ; frequent mild attacks of vertigo occur, in which the patient may have to lie down.

(3) Severe attacks of vertigo, in which the patient falls to the ground. There is a sense of falling backwards, and falling is always backwards. Objects appear to move up towards the ceiling ; looking down may help to steady him. Never any sense of lateral rotation. This phase has not been observed since the early part of April ; shortly after then the fistula test failed.

Examination.—Right ear : a mastoid cavity, the fundus filled with granulations which obscure details ; purulent discharge. Bone conduction of sound diminished. Left ear ; a well-healed mastoid cavity. Air conduction fair ; bone conduction normal : Weber to left.

Labyrinthine tests : Caloric ; both ears respond.

Rotation. Head.		Duration of nystagmus on stopping after 10 turns in twenty seconds.	
		To right.	To left.
30 degrees forward	... ...	0′ 25″	1′ 7″
60 ,, back and 45 ,, to right	... ...	0′ 20″	1′ 10″
60 ,, back and 45 ,, to left	... ...	0′ 40″	0′ 36″

i.e. assuming right labyrinth only to be abnormal, the external canal is abnormally sensitive, the superior shows diminished function.

Fistula tests : Bárány. (Right ear.)

Pressure + vertigo, backward deviation of head, upward movement of eyes, and falling back (response within one second).

Pressure −, no clear response.

Pressure on granulations, same response as pressure +.

Mygind's test—no response.

Left ear : typical "pseudo-fistula" test after about five seconds.

During the active phase (2) there was spontaneous nystagmus vertically upwards, on turning the eyes up. This was most marked when the head was held forwards, and became gradually almost imperceptible as the head was carried up and back. At times, there was nystagmus to the right on looking to the right, and objects appear to move to the right.[1]

Rectilinear movements of the head, active and passive, indicate altered function of the saccule ; there is probably some altered function of the utricle, but I have not been able to devise a test for this.

It is probable that the exposure of the superior semicircular canal has been in some degree repaired, perhaps by scar tissue.

Postscript.—The patient has now (October) been free from symptoms since early in August.—E. W.-W.

[1] *Postscript.*—This phase was present when the patient was shown at the Meeting ; it had then been present for eight days, and lasted three days more.

Case for Diagnosis—Chronic Traumatic Recurrent Laryngeal Dislocation.

By William Ibbotson, F.R.C.S.

B. C., aged 35, ex-service soldier.

History.—June 25, 1921. Throat "bad" for some months. Dysphagia with phlegm; has been unable to swallow any solid food for the last two or three days. Experiences a "full" feeling after taking a deep breath, also "wind" in the throat after eating or drinking. Lately he has been unable to lie down flat in bed, for then he suffers from great pain in the region of the larynx, chiefly on the left side, and his throat feels as if "it were being pushed out from inside." Complains of a hard swelling in the neck on the left side. No cough. Not losing weight. Attacks of aphonia whilst in the Army in Egypt, during the war; no previous attacks, and at that time, viz., January, 1916, he was exposed to sand-storms. Later on, he was severely wounded in France, losing his left arm, and incurring extensive flesh wounds in the right thigh. Was not wounded in the neck. No syphilis. Has been a great smoker of cigarettes, but has lately exercised much more moderation.

Examination.—Nose: greatly deflected septum. Nasopharynx: chronic œdema of posterior septal border. Mouth: no gross lesion. Ears: no gross lesion, hearing healthy. Larynx: on complete adduction, the cords approximate well, but the apices of the arytenoid cartilages overlap, viz., the left slips well behind the right. There is no evidence of any inflammation. There is definite "clicking" on forcible adduction, as also on passive movement of the larynx *en masse*. The voice is resonant and does not appear to have lost any power. Neck: ventral aspect: The whole larynx appears to be somewhat twisted, so that its left surface tends to look more anteriorly than its right; and thus the left superior cornu of the thyroid cartilage is definitely more prominent and easily palpated than the right. Pressure over the left superior cornu causes tenderness, and here is the point where patient often notices a swelling and feels pain. He also states that he often experiences a sensation of "something" being out of its right place, and that he can push this back into its natural position. The pomum Adami is distinctly to the right of the middle line. There is no external swelling. Dorsal aspect: there is no scoliosis. Trunk: the left upper limb has been amputated at the shoulder-joint. When standing quite straight there is seen to be a marked elevation of the left shoulder, and patient seems to have lost the power of drawing the left scapula backwards.

At my request, on May 24, 1924, Dr. F. G. Crookshank, F.R.C.P., saw the patient, and found that he was suffering from flatulent dyspepsia, with some spasm of the pharyngeal sphincter muscles.

Diagnosis.—The condition appears to be one of recurrent laryngeal subluxation, associated with dyspepsia.

In my opinion the laryngeal lesion is entirely due to the upset of equilibrium between the muscles of the head and neck of the two sides; thus, the right upper limb, having no opposition from the left side, is exerting an abnormal dragging force on the right side of the neck, and this overaction appears to be increasing. In this disability the larynx suffers.

Patient has, so far, felt unable to wear an artificial upper limb on account of the tenderness at the site of amputation.

Case of Chronic Epi-tympanic Suppuration treated by Trans-mastoid Atticotomy.

By WILLIAM IBBOTSON, F.R.C.S.

FEMALE, aged 32, married. First seen on January 10, 1922.

History of deafness: pain and discharge in left ear with pain in mastoid and cervical region. Tinnitus in both ears. Slight vertigo on right side. Duration twenty-five years. Onset followed "brain-fever." No operation on the ear, but has undergone two operations on the throat.

Examination.—Nose: nasopharynx healthy. Mouth: tonsils diseased. Ears: right, chronic tympanic catarrh, previous otitis media suppurativa, scarred membrana; left, chronic otitis media suppurativa, attic disease, no mastoid tenderness or swelling. Neck: no cervical swelling, but there is tenderness immediately below the left mastoid tip.

Treatment.—Non-operative at first, with lavage, &c., but there was no improvement.

February 15, 1922.—Operation: trans-mastoid atticotomy on left.

July 3, 1923.—Patient is very well and has no complaints. She underwent tonsillectomy in June, 1922.

Hearing Tests for Left Ear.—(*a*) Before operation: conversational voice heard at 6 in.; loud voice, 2 ft.; no cochlear or vestibular labyrinthitis, the deafness being entirely tympanic. (*b*) After operation: whisper heard at 25 ft.; very slight cochlear labyrinthitis; no tinnitus; no vertigo.

DISCUSSION.

Mr. J. F. O'MALLEY said, in reference to the second case, that a French surgeon had recommended the procedure during the war, and he (the speaker) had adopted it in one or two cases, with satisfactory results. The first difficulty seemed to be to keep the membrane intact, in order to give sufficient support to the ossicles. The cases were specially chosen for this because the disease was localized in the attic. In one of the cases Nature had done an almost similar operation on the other side, the attic having been already cleared out by the destructive inflammatory process.

Mr. E. D. D. DAVIS asked whether what had been done in this case was what Dr. Logan Turner called the modified radical mastoid operation—leaving the middle ear intact but removing the bridge and attic disease. He had so treated a few cases, and was very well satisfied with the result, because in some mastoid cases, whatever one did to the Eustachian tube, there was a persistent tubal discharge and the middle ear was always moist. If some part of the drum was left it scarred over the Eustachian tube, and helped to cure the persistent tubal discharge. Ionization for these cases had been a failure in his hands.

Dr. W. H. KELSON said, with regard to the first case, that he did not agree with Mr. Ibbotson's idea that the man's larynx was pulled round by the action of the right arm. He (the speaker) thought there might be something behind that larynx, on the left side, which was pushing it round. The cord appeared to move well, and there seemed to be no obstruction in the larynx itself. The patient had such a full neck that it was difficult to feel the deeper parts.

Mr. IBBOTSON (in reply) said there had been considerable eructations, and at first he could not discover what the condition of the larynx was in the first case. In 1917 the man was wounded, and had amputation of the left shoulder-joint, and in 1920 an alteration of the muscles had occurred. The pull of the muscles on the right side was not antagonized and the muscles had probably drawn the larynx to the right side.

He would try to bring the patient on another occasion, as he was anxious to know how to treat him.

With regard to the operation in the second case, he had tried to avoid touching the membrane, and his procedure had therefore differed from the ordinary modified mastoid operation. The antrum was first opened, and the external attic wall removed, the attack being made from above, so as to come down on top of the ossicles and not touch the membrane or the ossicles. When there was perforation of Shrapnell's membrane with the ossicles apparently intact and acting well, he did not think one was justified in doing more than atticotomy. He had had another case, which had done very well. Perhaps, as had been suggested, if one operated in a good number of cases there were bound to be some failures among them, but he (the speaker) did not think it was justifiable to do a radical mastoid in such cases, and this patient had good hearing.

Case of Chronic Traumatic Non-suppurative Labyrinthitis, treated by Labyrinthectomy.

By WILLIAM IBBOTSON, F.R.C.S.

MALE, aged 35, ex-service officer.

History.—Patient was badly wounded in the Great War, the right ear and right side of head being affected. From 1917 until 1920 he underwent eight operations on the right ear by various surgeons, who attempted, and finally successfully, to cure the suppuration in the tympanic and mastoid regions.

No operation on the labyrinth had been performed before December 13, 1920; I first saw the patient on this date. He then complained of attacks of very severe vertigo that had been existent since October, 1917, and of loss of memory and mental confusion. He could not drive a motor-car without great risk of a vertiginous attack occurring when passing another vehicle. The vertigo was to the right and forwards.

Examination.—After a number of tests had been applied it was found that patient was suffering from right cochlear labyrinthitis, with almost complete deafness and right vestibular labyrinthitis, irritative in character.

Treatment.—Drugs were tried for some months, but gave no relief.

Operation.—November 2, 1922: Right mastoid region re-opened, and a skin-graft removed. (No stapes was found, even after a long and careful search. Foramina ovale and rotundum were joined into one cavity.) Outer surface of promontory chipped off. Lateral wall of external semicircular canal freely excised. Superior semicircular canal laid open. Roof of vestibule largely resected.

September 22, 1923.—Patient's condition is very satisfactory: he has entirely lost the vertigo, and can carry out all natural functions as in complete health. After a series of tests it was found that the right labyrinth (cochlear and vestibular) had completely ceased to function, and gave no response to any of the tests. His mentality has very greatly improved, there being no confusion of thought.

Comments on the Case.—June 1, 1924: It is extremely interesting to note that mental inco-ordination existed concomitantly with physical, the former disappearing when the latter was cured. He is now holding an arduous and responsible post in London. This patient demonstrated the possible value of what I should like to call the "optic radiation test": vertigo was produced, with nystagmus, when a concentrated beam of light was thrown on to the retina of the affected side, a healthy response being given with the opposite retina; after the operation both retinæ reacted in a healthy manner. I should much value suggestions as to the anatomical paths of this reflex.

An Unusual Case of Chronic Frontal Sinus Suppuration.

By WILLIAM IBBOTSON, F.R.C.S.

MAN, aged 29, first seen by me on February 5, 1924.

History.—Swelling over left area of forehead for the past three years, becoming larger; another, smaller, one in the centre of the forehead, noticed for about two months. Attacks of pain about once a fortnight, situated over these swellings, sometimes very severe in character and of a "heavy pressing" type. During these attacks, or immediately after their cessation, the swellings become larger. Nasal discharge, bilateral, greenish-yellow, offensive. Difficulty in seeing with the left eye on waking. Definite left ocular pain for the past week. Several blows on the head during war. No V.D. Has been under treatment for some months by more than one doctor, one of whom considered the swellings to be of the nature of exostoses until several months later he obtained evidence of a nasal discharge.

Examination.—Nose and nasopharynx: Chronic ethmoidal suppuration left. Mouth: Infected teeth and tonsils. Face: There was a swelling 1½ in. in diameter, situated about an inch above the centre of the left orbit. There was another swelling, 1 in. in diameter, situated in the middle line of the forehead, 2 in. above the nasion. Both these swellings were of bony hardness, definitely tender, but exhibiting no fluctuation. Skin appeared to be healthy over them. No proptosis. Radiographic report: "The frontal sinuses are very large, and the portion of the left side, just above the orbit, is particularly large and clear. Maxillary sinuses are rather 'cloudy.'"

Treatment.—A few days later, under local anæsthesia, left middle turbinectomy was performed, and polypoid masses were seen to be blocking the left fronto-nasal duct, whilst lavage of the left frontal sinus gave exit to pus.

Diagnosis.—The case appeared to be one of very chronic frontal sinusitis, in which blocking of the left fronto-nasal duct had resulted in gradual accumulation in the sinus, and dilatation of its bony walls, and the two swellings were not exostoses, but frontal herniæ, the bony walls of which were thinned and atrophic from pressure. These two swellings, apparently, were separated by an incomplete bony septum.

March 5, 1924.—*Operation:* It was found that the anterior bony wall was composed of bluish bone, so thin and atrophic that it could be pulled off with forceps with the greatest ease, after the original trephine opening had been made; the bone forming the left supra-orbital margin and posterior wall of orbit was in a similar condition. The left frontal sinus was extremely extensive, passing outwards for some considerable distance over the orbit, and downwards behind the posterior orbital wall for about one inch; also over to the right, extending above the right frontal sinus, that was, itself, relatively, a very small cavity. An incomplete bony septum separated the two bony swellings. The frontal sinus was completely full of polypi, and there was a collection of pus, under pressure, in the extreme left corner behind the orbit; the right sinus was opened from the left and found to be similarly, though less extensively, diseased.

March 6, 1924.—Bacteriological report on the pus from the frontal sinus: "Culture grew pure streptococci. Film showed a few Gram-positive diplococci."

June 3, 1924.—No nasal discharge; no headaches. The condition appeared to be cured.

Comments.—The posterior wall of the left frontal sinus appeared to be free from atrophy, nor were there any fistulæ discovered. During the post-operative

treatment patient was given regular intramuscular injections of collosol manganese. These appear to my mind to exercise a beneficial influence in many cases of chronic suppuration. Wassermann reaction was negative. The bony frontal swellings, in several respects, resembled exostoses.

Case of Laryngeal Cancer: Operation.

By CYRIL HORSFORD, F.R.C.S.

J. P., AGED 56, married. Occupation, hairdresser.

History.—Hoarseness noticed about a year and a half ago, and in six months loss of voice became almost complete. Patient was seen first by me on December 1, and shown at the Section of Laryngology on December 7. At that time his larynx showed an extensive growth involving the right vocal cord and subglottic region. It was irregular in outline, forming two distinct prominences. The right vocal cord was distinctly fixed.

At the meeting the opinions expressed were unanimous in favour of a diagnosis of epithelioma, and demanding its removal by thyro-fissure. One or two Members expressed the opinion that the disease was so extensive that total laryngectomy should be done.

I subsequently removed a portion of the growth, which was cut and reported upon by Dr. Benjafield as epithelioma, and on December 17 I operated and re-removed the whole growth and ventricular band with the adjacent portion of the thyroid cartilage in one mass.

I had the privilege of the co-operation of Mr. Edward Davis at the operation, and the details of the operation were those practised by Sir StClair Thomson, who had kindly given me the opportunity of being present at a similar operation performed by him a few days before. In his case, however, Sir StClair stripped the growth away from the cartilage and then removed each separately. In my case I found it better to remove them both without separation.

The tracheotomy tube was removed immediately after the operation, and the patient left the nursing home within a week, without a rise of temperature, and with scarcely a cough.

Present Condition.—The cavity of the larynx shows no sign of recurrence, and the voice is improving daily and is surprisingly strong.

[Microscopic slide was shown; the growth and surrounding parts, suspended in a bottle, were also exhibited.]

Mr. E. D. D. DAVIS said he had assisted at the operation. There was nothing unusual about it, except that he had suggested taking the growth and the thyroid ala together, and not separating the growth from the ala. The result was good, except that there was a web-like scar in the anterior commissure, and it somewhat curtailed the lumen of the larynx.

Epithelioma of Nose.

By CYRIL HORSFORD, F.R.C.S.

PATIENT, a male, aged 49. Symptoms of nasal obstruction commenced eleven months ago. Patient has been losing weight but his general condition is good. No history of cancer in his family. Pathological report on portion of growth removed: "Squamous epithelioma appears to be growing from the middle meatus and the inferior turbinal, left side."

Operation: July 7, 1924. Moure's incision. After the anterior surface of the superior maxilla was removed the growth was found to be most extensive. The whole maxilla—except the palatal floor—was removed, also the malar bone, and all visible growth. On the fourth day symptoms of general septicæmia developed, and the patient died on the seventh day after operation.

Tortuosity of the Carotid.

By J. F. O'MALLEY, F.R.C.S.

MRS. M., aged 55, came on account of deafness, and said: "I have a swelling in the neck." She had seen Mr. Choyce the day before, and he came to the conclusion it was not aneurysm, but tortuosity of the artery. A definite loop can be felt, and Dr. Brown Kelly tells me he thinks it is rarer than the type he spoke of, (see p. 1) because it is in the common carotid, not the internal. I will try to bring the lady again.

DISCUSSION.

H. J. BANKS-DAVIS (Chairman) said he had had a case of obvious aneurysm of the carotid, which had been regarded as an enlarged cervical gland. He had intended to show the patient, but a zealous practitioner, thinking the swelling was an abscess, had opened it, and after that, nothing more had been seen of the patient.

Sir CHARLES BALLANCE said that, long ago, one of the surgeons at St. Thomas's Hospital had had a patient with a pulsating lump in the neck. Under the idea that it was an abscess, with transmitted pulsation, he had cut straight into it, and the blood had hit him in the face. The tumour was an aneurysm. In this case also nothing further was heard of the patient.

Case of Sixth Nerve Paralysis.

By W. H. KELSON, M.D.

(Shown at the Section of Otology, March, 1924.)[1]

WHEN this patient (male, aged 28) was first brought up, various routes were suggested for getting at the trouble, including the sphenoidal sinus route. But I selected the post-aural operation, and found two very large sequestra, which of course were removed. One involved the facial canal, the other the middle fossa, so that one could get below the brain for some distance. Healing took place slowly, but the patient lost his squint, and yesterday when I showed him he appeared to have quite recovered.

Paralysis of the sixth nerve in cases of acute otitis is not very uncommon, but in chronic ear disease its occurrence is most rare.

Sir WILLIAM MILLIGAN said he thought that in such chronic conditions there was a localized pachymeningitis in Dorello's space. It was petro-mastoid trouble, and the infection invaded through the petrous process. The localized œdema produced the paralysis. If the mastoid was opened and drained, the infection thus being relieved, this localized pachymeningitis disappeared.

Dr. WILLIAM HILL showed a Specimen of a large Calculus of the Tonsil.

[1] See *Proceedings*, 1924, xvii (Sect. Otol.), p. 82.

Mr. H. J. BANKS-DAVIS (Chairman) referred to the case of a lady who was always having tonsillitis, and had been treated for rheumatism and gout. On one occasion she consulted him, and on examining her tonsil he found a hard white mass, which obviously was a calcareous deposit. When this was removed, the symptoms cleared up. The deposit must have been present for years.

Dr. DAN MCKENZIE showed Skiagrams illustrating Osteitis Deformans with Severe Nerve Deafness.

Mr. DOUGLAS GUTHRIE showed (1) a Specimen of an Unusually Large Aural Polypus in a man aged 50. (2) Two Specimens of Exostoses of External Meatus.

DISCUSSION ON OTOLOGICAL AND RHINOLOGICAL PROBLEMS IN SCARLET FEVER AND MEASLES.

Mr. T. B. LAYTON.

I shall epitomize the problems in connexion with scarlet fever and measles, dealing first with the otological ones, and then with the rhinological. My first remarks will deal with scarlet fever, the later with measles.

In scarlet fever the otological problems are those connected with otitis media, and those connected with mastoiditis.

In the majority of cases of otitis media seen in ordinary practice the first symptom which draws attention to the disease is pain, but in most cases in scarlet fever pain is wanting. Therefore I think the first problem is how to find your case of otitis media and recognize the disease before there is otorrhœa. Any suggestions in regard to this will be of the utmost value to those working in a fever hospital. The next problem is that of treatment. This will depend on the condition of the ear and on the state of the patient. If you do get a drumhead which has not yet burst but is ripe for incision, the proper treatment is to do a primary paracentesis. It is difficult to persuade medical officers in charge of cases that a patient is more likely to do well if that is done than if it is allowed to burst. From the otological point of view, I would like to hear remarks on the value of secondary paracentesis, i.e., incision of a drumhead which has already ruptured but in which there is otological evidence that the discharge from the middle ear is not quite free. The other subject connected with the treatment of otitis media in scarlet fever is that of operation on the throat and naso-pharynx. Should operations on tonsils and adenoids be performed? If they should, at what stage? And should there be removal of tonsils and adenoids, or of adenoids only?

The third problem is to study the natural termination of otitis media in scarlet fever. In a large number of the cases of children suffering from chronic otitis media this is attributed by the parents to scarlet fever. Some of these cases, however, come on after the patient has recovered from the scarlet fever. I had a good example of the latter. I was told there was a child in the hospital with an attack of scarlet fever who had had an attack also three and a half years before, and that I had done a mastoid operation on the child when it was previously in hospital. I asked how the mastoid was going on, and the doctor said it had been suppurating ever since. I saw the child, and recognized I had not done the operation. The operation had been done six months after the child left the fever hospital, and there had been

no suggestion—the notes had been kept—of any otitis media during the attack of scarlet fever. When doing county school work there seems to be a considerable amount of ear disease which originated from scarlet fever, leaving destruction of the middle ear and considerable deafness. In the hospital the majority of cases heal up without perforation, or if there is perforation left it is but a small one. It is difficult to find in fever hospitals the cases described in text-books as typical otitis as a result of scarlet fever, with almost complete destruction of drumhead, or with very large perforations.

Supposing you cannot get the ear dry, what is the risk to the patient? Secondly, how long should such a patient be kept in hospital? If you never discharge a patient with a running ear you are faced with the alternative either of keeping the child in hospital for a considerable time, or of doing a mastoid operation, possibly a radical one. From the point of view of other people, there arises the question of the degree of infectivity of a child with a running ear. I have taken up the standpoint that the question as to how far these cases are carriers of scarlet fever should be left to the hospital authorities; that, whether the child can be allowed to go home with safety to other children should be for the medical officers in the fever service to decide: but that it is my responsibility to say whether it is better in the interests of the child to be discharged with active disease still present or whether further treatment should be attempted in the hospital.

The carrier problem is, however, one that we must consider in the near future and this brings us on to the question of the cases in which the Klebs-Loeffler bacillus is present in the nose of children on whom you want to operate for otitis media.

Passing from otitis media to mastoiditis, two problems have to be considered; first, that of diagnosis, then that of treatment. It is the aim of the otologist to diagnose mastoiditis before there is any retro-auricular swelling. I ask if any members have suggestions as to how you can make that diagnosis in children with scarlet fever. They do not often complain of pain, though, not infrequently, they have tenderness, and this may occur during the apyrexial stage of the disease. I do not remember having diagnosed mastoiditis until a swelling has appeared. And it is the more difficult of diagnosis because one of the great complications of scarlet fever is the presence of enlarged glands in the neck; you may not be able to tell whether the swelling is due to mastoiditis, or to enlarged glands in the neck, or to the two combined. One day I saw two cases at the fever hospital in which mastoiditis was thought to be present. The first case I thought was due to gland enlargement and in the other case I did not think there was any doubt that it was Bezold's mastoiditis. I was wrong in both cases: when I said it was glands, it was mastoid, when I said it was mastoid, it was glands only. Cases must be judged individually, according to the position of the swelling and your observations from day to day.

Turning to the treatment of mastoiditis in scarlet fever, all the authorities on scarlet fever that I know say there is not any great anxiety with regard to the extension of the disease from the bone to the meninges, and I incline to agree with them. You can wait to make your diagnosis in scarlet fever with greater safety and for a longer time than in the ordinary mastoiditis of hospital or private practice. This will influence the way in which you approach the treatment. We agree that some form of operative treatment is necessary. What should it be? Should it be the simple Wilde's incision, or the simple mastoid operation? Should it be the Schwartze, with the turning out of

diseased bone? Should it be the conservative operation with meatal flap? Or should it be the radical operation? Which of these should be done is a matter for discussion in varying conditions and cases.

When we go from scarlet fever to measles, the problems are probably much the same, but they are more difficult to tackle. Moreover, there are fewer opportunities of studying them, as there is not much measles taken into fever hospitals. Another point is that sufferers from measles are much younger children, and they are so seriously ill when admitted that examination of them is difficult. You may kill the child in attempts to get the otitis media well.

Turning to the nose and throat, the problems before us there are those connected with rhinitis and pharyngitis and sinusitis. The medical officers in charge of cases deal chiefly with the rhinitis and pharyngitis and the question is chiefly one of syringing *versus* not syringing. Most of the people I know in this work have given up syringing the throat in infectious disease, and their results since are as good as, or better, than before.

I do not think sinusitis is very common during infectious diseases. I do not not know how you will diagnose it until a swelling is seen in the face. In three years I have seen only three such cases of sinus disease.

Mr. Sydney Scott (Chairman)

expressed the gratitude of members to Mr. Layton for having opened the discussion on a subject the importance of which was not yet realized by the outside public, not even by those in the profession who were not specialists, nor perhaps even by all medical superintendents of fever hospitals, though a change was now taking place.

Dr. Kerr Love

said he considered that, as an aid to treatment, tonsils and adenoids should be removed as soon as practicable, i.e., after the fever had subsided. The risk of aural complication would thus be diminished; if such complication had occurred its duration would be shortened.

No authority was entitled to dismiss from a fever hospital a patient who had had ear discharge, until that discharge had ceased. If in two or three months it had not ceased, the mastoid process must be opened. Under the present imperfect arrangements patients were being dismissed before the cessation of the ear discharge. He (the speaker) thought that certainly the school authorities should not re-admit such a patient to school, whether the discharge resulted from scarlet fever or from measles. That point was raised at a meeting of the Oto-Laryngological Society at Edinburgh, and he (Dr. Kerr Love) had already asked the school authorities for the County of Dumbarton and those for the City of Glasgow to take that view, which he believed would prevail.

With regard to the occurrence of more serious complications in connexion with these two diseases, a few months ago he (the speaker) had operated on the right maxillary antrum of a girl who, some months before, had had scarlet fever, and he believed the antral condition had been due to the scarlet fever. More recently he had operated in a school clinic upon a child in whose case complete sinus thrombosis was due to an attack of measles months previously.

He (Dr. Kerr Love) thought it would be the duty of this Society to make a a move in the direction of getting the consciences of medical officers in charge of infectious diseases hospitals aroused on this important subject and getting them to co-operate more thoroughly with otologists, too few of whom had been appointed in connexion with such hospitals.

Sir WILLIAM MILLIGAN

said that when he was President of the Otological Section of the British Medical Association at its Manchester meeting in 1902, he had brought forward a resolution asking that the authorities would take this question into consideration, and make appointments accordingly. That was carried unanimously. Yet now, in 1924, few civic authorities had given effect to the request. He agreed that it was incumbent upon such Sections as theirs to lead in semi-political matters as well as in scientific ones; every social and political means open should be used to bring home to the authorities the need for more to be done because of the danger to the rising generation. Otologists knew how destructive these processes were, and how they handicapped children in their struggle through life. One solution was the appointment of men competent to investigate these cases, which should be investigated daily, for the greatest success rested with those who caught the cases early. It was a policy which he was at the moment discussing with the Manchester Corporation; he had recommended that at least two otologists should be appointed to inspect the scarlet fever cases in the fever hospital. Objection might be taken on the score of expense, but in the first year such an appointment was made in Edinburgh, the stay of patients in hospital was much shorter than formerly, therefore even financially it was well worth while.

What was the difference, in regard to the effect on the ear, between scarlet fever and measles? In scarlet fever the tendency was for the infection to invade bone, in measles the tendency was to involve mucous membrane. Some years ago he had said that in scarlet fever cases the whole middle-ear cleft was invaded, whereas in measles there were segments of the middle-ear cleft which could be invaded independently. In measles there could be a pure tubal infection, whereas in scarlet fever the tendency was for the disease to be generalized in the cleft and to invade bone secondarily.

Another important matter was the part played by the surgeon in the conduct of a case in which it was known that tonsils and adenoids were present. Adenoids should be removed during the infectious stage the moment the temperature came down; it was not prudent to wait for this until convalescence, for early removal of the material expedited recovery and saved the ears.

There was a great difference between cases seen in private and those seen in hospital. He did not recall an early case seen in private which needed after-treatment of an urgent character, for he did not wait until the membrane bulged; as soon as it showed congestion, he incised it. One should anticipate pathological events, and, from the first, facilities for free drainage should be provided. If the perforation were a pin-point one, it should be freely enlarged to permit of free drainage.

With regard to what the authorities should do in the case of a child with running ears, was an authority entitled to discharge a patient in such a condition? The individual was a possible carrier, and therefore a public danger, and the authorities should insist on his being detained in hospital until he had entirely recovered. It might be a number of months, but it would not be so long if it were known that this would be the system, for some type of operation would be done at a much earlier stage than was the custom now. If careful treatment for two months did not stop the discharge, he thought the patient should be submitted to a mastoid operation. At present these children went out, they disseminated infection, and so a vicious circle was formed.

Mr. J. Adam

said he had had the impression that a chronic suppurative otitis after scarlet fever always needed a mastoid operation. At the Child Welfare Hospital at Motherwell he saw many children and he asked the medical officer of health to give him some statistics. In five years he had 1,370 cases of scarlet fever, and among these were sixty-two cases of otitis. Twenty per cent. of the sixty-two were not dry on dismissal, 80 per cent. had dried, none had paracentesis done. Only five of them required a mastoid operation. That supported what Mr. Layton said: that cases of scarlet fever otitis mostly got well, even if no operation were done, and that the bone was not involved so often as one was led to expect.

Dr. Dan McKenzie

said a child who had been under his care at the Central Throat and Ear Hospital for otorrhœa developed scarlet fever and was removed to the isolation hospital. She was taken in again seven or eight weeks later and, within a week of her arrival, the children in the cots beside her developed scarlet fever. He did not see how it was possible to avoid an accident of that kind. He raised a question he brought forward some years ago, namely, whether these cases should not be more frequently operated upon by way of the mastoid. The problem of secondary paracentesis should never arise. If primary paracentesis failed to cure the case, why not drain the antrum? Posterior drainage of the middle ear only took a few minutes to perform, and the route of discharge was thereby transferred from the meatus to the mastoid, so that healing of the membrane was facilitated. One sign to which he found himself attaching more and more weight as an early indication for posterior drainage was deep meatal œdema close to the membrane. If after paracentesis the discharge continued profuse longer than for three or four days, some mastoiditis was occurring. If posterior drainage failed to cure the disease, it was a case for the radical mastoid operation.

The position he had now arrived at was actually as follows: In a case of acute suppuration of the middle ear, which was not getting well and in which there were enlarged tonsils and adenoids present, it was safer and better to do posterior drainage through the mastoid first, before trying the effect of removing the tonsils and adenoids.

Dr. Neil Maclay

said that for three and a half years he had acted as otologist at the City Fever Hospital, Newcastle-on-Tyne, where the scarlet fever admissions were 500 to 600 per annum. He was familiar with otorrhœa or otitis media showing themselves during the second week of the disease; he had never seen them in connexion with the large devastating perforations which were described in the text-books. Usually the onset of the discharge from the ear was unaccompanied by pain; generally the first knowledge of it came from the report of the nurse that discharge was seen on the pillow in the morning. It was most difficult to anticipate this by paracentesis, even if the ears were examined every day. Soon after admission, an effort was made to cleanse the mouth, nose and fauces, as well as the external meatus. Many cases had hard wax plugs and epithelial débris in the ears, and varying degrees of external otitis. If a membrane ruptured into a fairly clean external canal, there would be a better chance of natural healing. Many of the children had septic teeth, gingivitis,

even pyorrhœa, and those conditions should be treated promptly. The nasal douche was not used. If tonsils and adenoids were present, they were removed, and at an early period. By treating cases in this way, the stay in hospital of patients had been reduced from fifty-eight or sixty days to forty days, representing a considerable saving to the civic authority. There had been a singular freedom from mastoid implication: he had only done one or two mastoid operations in three and a half years in that hospital. But he had seen, fairly frequently, post-auricular cellulitis, which responded to post-auricular incision and drainage. In two such cases he had investigated the mastoid cell area and not found osteitis. It was associated with a type of external otitis, which was believed to be peculiar to scarlet fever. It was not furunculosis, but a sub-epithelial inflammation due to the streptococcus.

Mr. J. S. Fraser

said he was glad to hear the references to false economy; the authorities seemed to have acknowledged their mistake. Dr. W. T. Gardner held the view that it would not make much difference if early paracentesis were performed in these cases, and he had had a large experience as otologist in a fever hospital. Difficulty in getting permission to perform the operation was experienced; parents should be asked to sign a form of permit on admission to a fever hospital, as in the voluntary hospitals. Dr. Gardner had laid stress on the different mentality of the parents of a patient entering the voluntary hospital as compared with those of a patient entering a fever hospital. Removal to the latter was compulsory, and parents were not equally willing to have an operation performed.

He agreed with Dr. McKenzie as to secondary paracentesis. He (Mr. Fraser) had never seen that a secondary paracentesis did any good at all. If the spontaneous rupture of the membrane did not result in the clearing up of the acute otitis media within three or four weeks, the mastoid should be opened.

Too little stress had been laid in this discussion on the rhinitis of measles. He felt that cure of the rhinitis resulting from scarlet fever and measles would obviate ozœna in the future, because the main cause of ozœna was the rhinitis following those two diseases. Ozœna really corresponded to chronic middle-ear suppuration and cholesteatoma in the middle ear.

Mr. Hugh Jones

reminded members that just before the war a full discussion took place on this subject at the International Medical Congress, and a Superintendent of the Metropolitan Asylums Board at that time said that chronic suppurative otitis media had never resulted from scarlet fever. Every otologist present was much surprised at this statement. Often the connexion was absolutely certain, but it must be admitted that misleading histories were sometimes given. He remembered two cases which were sent from a fever hospital, in which the discharge was supposed to have entirely stopped, but three days later the patients turned up at the eye and ear infirmary with renewed discharge. Both patients had perisinus abscesses. He supposed that in many of the cases there was a temporary cessation of the discharge, or the discharge was so inspissated that there was no external evidence of it, and the non-expert, assuming it to be cured, sent the patient out. At the date of which he was speaking the speaker arranged a sort of liaison between the Superintendent of the Liverpool fever hospitals and the ear hospitals, the idea being to have the ear hospitals case-sheets started in the

fever hospitals, and continued afterwards in the ear department to which the patient was referred. Each patient with otitis on discharge from the fever hospital was to be sent direct to an ear hospital or department. This plan worked for three months, then the war intervened. During that three months period the speaker, rather to his surprise, saw only one case of scarlet fever, and seven of diphtheria, and he concluded that there were similar problems in connexion with diphtheria, to those in connection with scarlet fever and measles. Once patients had been discharged from the fever hospital it was difficult to get parents to bring them for treatment, as they thought nothing further was required. The necessity for following up these cases should be strongly represented to the civic authorities.

He remembered an epidemic of measles in a large preparatory school, in which he was asked to see the cases of otitis which occurred. There were forty cases of measles, and about twelve of acute otitis media. Seven of the latter required paracentesis, two needed the mastoid operation. The medical officer said he believed the whole trouble was due to the fact that all the boys had been using a douche, and the speaker thought he was probably right. This led him to deprecate the use of douches, while he still thought that sprays were harmless.

Mr. H. J. Banks-Davis

asked whether it had been definitely ascertained that a child with scarlet fever and discharging ears could induce scarlet fever in another child.

With regard to keeping a child in hospital until the discharge had ceased, it might not cease at all. If a mastoid operation had been performed, and the ear was still discharging, should such a patient be taken into hospital? In the Civil Service, a person with a discharging ear would not be accepted, but if he had had a mastoid done and the ear was discharging, he was regarded as partly cured. But he thought such a patient was in the same position as the other.

Mr. Sydney Scott (Chairman)

said that the carrier question was a very important one, and asked if it could be shown that a child with a discharging ear was necessarily a carrier of scarlet fever or measles?

Mr. J. S. Fraser.

said that Dr. Carr, of Edinburgh, stated that he got returned cases of scarlet fever from houses to which a child with running ears had returned.

Mr. F. B. Gilhespy

said that at Birmingham fever hospital cases were not allowed to go out with running ears. In the last year four patients with discharge had been allowed to go out, but they were cases from which tonsils and adenoids had been removed and the mastoid operation done. Those cases were removed from the hospital at the wish of the parents. Otherwise cases with running ears were kept in as they were regarded as infective. At the Birmingham Ear Hospital three or four years ago there were epidemics of scarlet fever, and it was thought they could be traced to children who had been brought into hospital for either mastoid operation or for treatment of running ears. The Medical Committee had passed

a resolution that cases with running ears caused by scarlet fever should not be received for treatment at the ordinary hospital.

He was interested in ozœna and he was watching cases, but they would have to be followed up after they had left the hospital.

Dr. Hartog

agreed that it was very difficult to diagnose mastoiditis in scarlet fever before the tissues behind the ear were swollen. He was afraid of scarlet fever complications in the ear, as he had had a bad experience of them. He had seen several cases in which the mastoiditis could not be diagnosed and in which he operated and found softening of the whole bone to such an extent that after clearing up sequestra, dead bone, &c., there remained nothing more than egg-shell bone at the mastoid, and every complication was to be feared. Therefore an early diagnosis was very important. To this end he recommended the taking of stereoscopic radiograms of the mastoid, which plainly showed the sequestra lying in the bone.

Mr. A. D. Sharp (Leeds)

said he found douching of the nose excellent in practice, though some might have objection to it on theoretical grounds. The risks of inducing middle-ear infection by using the douche he characterized as a bogey. Eighteen months ago he was asked to visit a preparatory school in which there was an epidemic of measles and influenza, with ear complications. One boy had died from septic meningitis, there were two patients with swelling behind the ear, several with bulging membranes, and ten or fifteen with red membranes. In two the usual mastoid operation was done, those with bulging membranes had them incised, and all were put on to nasal douching, with reasonable precautions. He had found that to be of the greatest benefit. There were no further ear complications, and within forty-eight hours there was no further cause for anxiety. Until the condition of the nasal mucous membranes had been improved there would be no improvement in that of the Eustachian tube. After douching in these cases there was a decided improvement in forty-eight hours. Many had enlarged tonsils, probably adenoids. Nasal douching gave rapid relief, so that the tonsil and adenoid operation could be safely postponed until the patient was convalescent. He had never had a case of middle-ear infection as a result of douching. The spray was quite useless. During the douching the patient sat up, with the head bent forward; few children were too ill to be propped up for a few minutes.

Mr. F. Sydenham

said he did not think there was any evidence of scarlatinal infection from a chronic otorrhœa; and if an appeal were to be made to the public for the appointment of otologists to fever hospitals there must be some solid ground for the request. One had been asked for proof for this infectivity, and one had to confess there was none. But otologists did know, and could prove, the damage done to the hearing apparatus, and the Government could be impressed with the importance of making such appointments so as to conserve the hearing.

Mr. J. F. O'Malley

said that for some time he had filled the position of consulting otologist to a community in North-west London, and a rather advanced experiment was made there. In previous years the establishment had been used as an isolation

hospital, and later was partly used as such and partly for the treatment of ear, nose aad throat troubles in connexion with the exanthemata. Owing to economical and political reasons, a change was made, and he ceased his connexion. But as a result of his experience there he concluded that scarlet fever and measles were a serious cause of ear troubles. He was not called upon to treat many cases of mastoiditis, but that was chiefly because the medical officer did a simple incision, and many of the cases healed up. Several hnndreds of cases of tonsils and adenoids passed through his hands, but he had only six to eight mastoid cases in a year.

Mr. Hugh Jones, himself and others had been trying to bring this matter before the authorities, and one of the results was the appointment of Mr. Layton and others to such posts: he hoped it would be extended.

Mr. T. B. Layton (in reply)

referred to a paper by Dr. Goodall giving statistics, but these were based on reports by nurses as to discharge from the external ear, and he thought Mr. Adam's statistics of 80 per cent. dry were based on the same kind of observation. He (Mr. Layton) refused to consider such statistics; none should be accepted unless they were the result of examination by the otologist with the forehead mirror. Only thus could the number who went out with the ears healed be ascertained. Even thus in some cases it was difficult to say that there was no active disease present; and if none could be detected, there was a weakened mucous membrane, which might be infected shortly afterwards. Some might first be attacked with ear disease as soon as they left the hospital.

The important point had been raised as to whether an operation on a child with running ears ought to be undertaken not for the benefit of the child, but for the benefit of somebody else. Yet no one could say all those cases might cause scarlet fever, and in any individual case it might be the nose rather than the ear that caused the return case. Until more was known of the bacteriology of scarlet fever, the problem would not be solved. A number of parents would refuse the suggested operation; what ought to be done then? He was not prepared to support Dr. Kerr Love and Sir William Milligan in their contention that no authority was entitled to dismiss a child unless the ear was dry.

He quite agreed with Dr. Kerr Love that the school authorities might refuse to have those cases back at school. Even if it meant the loss of a year's schooling, he did not think it would matter very much. It was for the parents to decide whether they would have an operation done, or whether the child should remain from school until the ear was dry. He thought a good number would heal up as a result of general healthy surroundings. A certain number of the cases healed up after Wilde's incision. His difficulty had been to decide at what stage, healing having failed, a mastoid operation should be done. Mr. Cheatle had told him that if hot fomentations were put on the ears, as much good was done by that as by Wilde's incision, but he did not think this was so.

He was indebted to Dr. Hartog for disagreeing with him, as it would make him carefully reconsider his views as to the relation of scarlet fever mastoiditis to meningitis. After what Dr. Hartog had said about stereoscopic skiagrams, he would also consider the question of advising the Metropolitan Asylums Board to instal a stereoscopic X-ray apparatus at one or more of their hospitals.

Dr. J. Eastman Sheehan (introduced by Mr. Bedford Russell): "Treatment of Maxillary Antrum Empyema by Scraping and Thiersch Grafting."

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Section of Medicine.

President—Dr. Robert Hutchison.

PRESIDENT'S ADDRESS:

By Robert Hutchison, M.D.

My first duty is to express my thanks to the members of the Medical Section for the honour they have done me in electing me their President; I assure you it is a distinction which I value very highly. No one can say that the duties of the office-bearers of this Section are of the nature of a sinecure. Indeed I am rather tempted to ask my colleagues—as Mr. H. G. Wells asked the leaders of the Russian Bolshevists—whether we may not have "boarded a derelict rather than taken over a going concern." For there can be no doubt that in recent years the business of the Section has become more and more attenuated and the difficulty of running it proportionately greater. This is not due to any lack of energy or enthusiasm on the part of the Fellows as a whole but to the constant splitting up of the Society into special Sections which has been going on for some time and which does not seem even yet to have reached a terminus. It looks in fact as if the Sections of Medicine and Surgery, which are really the parent Sections of the Society, are being gradually devoured of their children, and that soon the territory of general medicine at least will be reduced to a small area lying around the umbilicus; and it only needs the establishment of Sections on Respiratory Diseases, Gastro-enterology, Endocrinology, Arthrology and Diseases of Metabolism—and there is no reason why these should not come—for medicine proper to fade away like the "Cheshire cat," till there is nothing left but the grin.

Now there is no use in our grumbling at this process of constant partition. It is, after all, only the reflection in the work of this Society of the constantly increasing tendency to specialism in practice which has been going on now for some years. Nor do I propose to weary you with any discussion of the well worn theme of the pros and cons of specialism, but I should like to consider for a moment its effect upon our meetings and work here. Like most processes of evolution the fissiparous tendency in the Society has its good and its bad side. To mention first its advantages: there can be no doubt, I think, that the specializing of Sections makes for better discussion and keener criticism. A member of such a Section is pretty sure to hear at every meeting something that interests him, of which he has first-hand knowledge and which nearly touches his daily work in practice. In a more generalized Section this is not so, or not to the same extent, and many of the papers read and discussions that take place are only of a remote and academic interest to a large proportion

of the audience. On the other hand it is one of the chief disadvantages of the specialization of Sections that it interferes with what one may speak of as the "cross-fertilization" of minds, which is, after all, one of the most important intellectual functions of such a society as ours. For all specialism, though favourable to the observation and collection of facts, is bad for philosophy—it tends to interfere with the genesis of ideas. And I think a study of specialist literature will bear this out. Nature, after all, knows no barriers and no such artificial divisions between the diseases affecting different parts of the body as we try to make here. The organs are "members one of another," and knowledge gained by the study of disease in one may well throw light on the diseases of others and start fresh trains of thought and investigation.

How, then, are we of the Section of Medicine, whilst recognizing the inevitable uprising of special Sections, to justify our continued existence and to contribute our share to the life and activity of the Society as a whole? I think we may do so, in the first place, by standing up for the solidarity and catholicity of Medicine and by asserting our right to entertain papers and discussions on any subject which falls within the wide sphere of the physician as opposed to that of the surgeon. Further, we must aim at securing the help of the younger men before they have specialized. I think, also, that we can do useful work by acting as a kind of liaison officer between the different sections in the promotion of joint discussions at which subjects can be approached from different angles by the various specialists, so promoting that cross-fertilization of minds of which I spoke a moment ago. I am glad to say that the necessity and advantages of such joint discussions are being increasingly recognized throughout the Society and several have been arranged for the forthcoming session in which this Section will take part.

But if these discussions are to be really vital and to lead to any useful end certain conditions must be fulfilled. They must not be of too "cut and dried" an order, and the leading protagonists should not be men whose views on the subject are already fixed and well known. Plenty of time should also be left for the "back bench" members to contribute their opinions and criticisms, which are often just as much worth hearing as those of the openers. I have heard it hinted also that the discussions of our Society suffer from too much 'politeness," by which, I take it, is meant that they are not enough of a "cut and thrust" character and that extreme and fanciful views meet with too tender a criticism. It will probably be admitted that there is something in this view, and that an infusion of outspoken frankness would not be amiss in our debates even if it might sometimes lead to embarrassments for the Chair.

Another source of knowledge that we might tap with great advantage is the general practitioner. It is much to be regretted that the experienced general practitioner contributes so little to our common stock of knowledge. After all, he has unique opportunities for studying many aspects of disease which are denied to the specialist or to the pure physician. He can follow a case from its earliest beginning to its final outcome; he can watch the influence upon the disease of family and constitutional proclivity and of surroundings and habits. He is able to judge of the success or failure of different methods of treatment and to acquire that foresight of the course of events which is the basis of prognosis. And yet how often do we see the tragedy of all this store-house of knowledge and experience acquired by a lifetime of practice being lost to the world, and the possessor of it dying "with all the music in him." In part, the silence of the man in general practice is no doubt due to modesty and a regrettable inability to believe that he possesses anything worth con-

tributing to give to the common stock. Allowance must also be made for the exacting character of his work and for the demands which it makes on those who are successful in it, demands which leave little time for attendance at societies and little energy for the preparation of papers. I believe, however, that there is still another reason for this infructuosity—a reason which affects others besides those in general practice. An unfortunate notion seems to have got abroad that medicine is now only to be advanced by the work of the laboratory, that "research" can only be carried on with instruments of precision, and that patient observation at the bedside by the unaided hand and eye and ear is not to be dignified by that name nor indeed to be regarded as strictly "scientific," it being forgotten that accurate clinical observation and deduction has been the characteristically English contribution to scientific medicine from the time of Sydenham onwards, and that it is the spirit in which it is done that makes work scientific and not the mere technique employed. It should be our part in this Section to make a stand against this pernicious doctrine – for which the established and official "scientific" bodies in medicine are not wholly free from responsibility—and to welcome here papers founded on work of this class, papers which the general practitioner is well able to contribute. I can recall, indeed, in past years some contributions of this nature by such men—though it might be invidious to mention them by name—which were of the highest interest and value.

It is on these lines, I conceive, and with the enthusiastic support of all our Members, that we must proceed if we are to make the work of our Section a success, and to justify our continued existence as a part of this Society.

Hæmatemesis "Without Lesions."

By GUSTAVE MONOD, M.R.C.P.Lond., M.D.Paris.

(Physician to Vichy Thermal Hospital.)

I AM representing here the Association for the Furtherance of Medical Relations of the Paris Faculty of Medicine. It is our great desire to keep in touch with our British confrères. We shall never forget the years we lived shoulder to shoulder.

International medicine has its organizations: the Congresses. But, as we say: *Les temps sont durs.* And we have been wondering if it would not prove useful and easy to discuss simultaneously a given subject in the medical societies of various countries.

I am, therefore, bringing before you a question which is actually under discussion at the Gastro-enterological Society of Paris, and which is going to be brought before the Cercle Médical of Antwerp towards the beginning of next month.

The best proof I can give you of the opportunity of a working liaison between our countries is the fact that when I went through the literature referring to my subject I found that it had been very thoroughly investigated and discussed in your country, and therefore it is our British colleagues who ought to introduce this discussion in France. It is from you we have to learn.

I come here seeking your criticisms and advice with regard to the following questions: Is there such a thing as hæmatemesis without lesions? If so, how is it to be explained? and how should it be dealt with?

If we refer to Latour's work [1], published at Orléans, in 1815, "Histoire philosophique et médicale des hémorragies," it will be seen that he admits the occurrence of gastric bleeding *sine materia*, and quotes cases of Portal, Franck, Hoffman, Van Swieten, Baldinger, Warton. The name of Dalché [2] (1803) may also be mentioned, and then let us pass on to the present time, when presumptions have given place to facts. One of your leading clinicians, Sir William Hale-White [3], in a lecture published in the *British Medical Journal*, (November, 1906), relies only on proved facts. I mean cases in which diagnosis is verified either by operation or by autopsy. He collected twenty-nine cases derived from this part of the world. He points out how very rare is the occurrence of such an accident in males, and after reminding us that, conversely, deaths from hæmorrhage due to ulcers are far more frequent in males than in females, he concludes: "Either the outlook of gastric ulcer in women is much more favourable than it is in men, or many cases occurring in women, and classified as gastric ulcers at the bedside, are really instances of some other undescribed malady."

To such a malady Sir William Hale-White gives a name: gastrostaxis.

To his twenty-nine cases let me add a few recent ones which have been observed in Paris hospitals. Here are three which have been reported only last month to the Society of Gastro-enterology, by Professor Pierre Duval and Dr. J. Ch. Roux [4], and have not yet been published. The patients were three women.

Case I.—Patient, aged 24, complained of painful dyspepsia of nine years' duration. For a year she had attacks of hæmatemesis, bringing up about 2 to 3 oz. of blood at a time. She was operated upon at the age of 16 and had her appendix removed (which often happens with this type of patients). Blood count 4,900,000. Bleeding time: Three minutes. Coagulation time: Twenty minutes.

The screen shows an elongated stomach (the fact should be noted) and stagnation in the second portion of the duodenum.

As medical treatment yielded little result, operation was performed. Slight adhesions are found around the gall bladder. Normal recovery.

Three weeks later there was a fresh crisis; severe hæmorrhage per anum. Blood corpuscles 2,500,000. The surgeon thought he must have overlooked a duodenal ulcer. Operation: The duodenum was opened from end to end. No lesion. Gastro-enterostomy. Normal recovery.

Three months later, a fresh hæmorrhage, slight in amount. Four months after this the patient was admitted again for severe pain. She was operated upon for the fourth time. The surgeon opened the jejunum. No ulcer. An unabsorbed thread was removed. Since then the patient seems to have recovered.

Case II.—Patient gave a history of late onset of pains after taking food. She had an acute attack with hæmatemesis. Professor Duval opened the stomach from cardia to pylorus. The duodenum was carefully inspected: no lesion whatever found.

Case III.—Patient, age 32, had her appendix removed seven years ago. Admitted for hæmatemesis which appears at the period of menstruation. An X-ray plate showed stenosis of the third portion of the duodenum. Operation: A meso-colic band pressed on the duodenum. No ulcer. Duodeno-jejunostomy. Recovery.

So here are three recent operations, in which ulcer was diagnosed, but was found not to be present. In two cases there was oozing, and perhaps microscopically visible erosions. In the third case the bleeding was from congestion due to duodenal stenosis.

M. Basset, a well-known Paris surgeon, recorded three instances very similar to these cases, under surgical control. Professor Duval has just had two cases in which he thought he would find ulcers. He operated, found nothing, and both his patients recovered permanently.

M. Robineau [5], surgeon to the Necker Hospital, Paris, records the history of a patient, aged 39, who had so profuse a hæmorrhage that the blood count numbered

950,000, with 25 per cent. hemoglobin. There was no definite history of syphilis, though the patient stated that she had had a hemiplegia which abated under intravenous injections. After a long and difficult examination under X-rays a diverticulum of the fourth portion of the duodenum was diagnosed. M. Robineau dissected out the small pouch, the operation being prolonged on account of excessive bleeding of the gut. Neither macroscopically nor microscopically was there any lesion to be found. No sub-mucous angioma. Patient made a normal recovery and has never since been troubled by hæmorrhage.

The result in these cases supports the conclusions at which Sir William Hale-White and Lord Dawson have arrived. And now the question arises: What are the causes of such hæmorrhages? I think we may eliminate chlorosis, the belief in which as an independent ailment is for various reasons losing ground every day. We hesitate, too, to call the hæmorrhages vicarious. They do not follow the rhythm of normal periods, and we do not find that our patients report suppressed hæmorrhoidal bleeding. Hind relates the case of a woman who suffered from hæmatemesis once a week during nine consecutive years.

I have to admit that the cases collected and reported in France are lacking in accuracy, the excuse for this being that in all instances they were urgent cases, and that we do not admit a diagnosis of gastrostaxis if direct proof of it is not forthcoming. The non-operated cases are diagnosed as ulcers.

Is a possible cause to be found in some nervous disorder? That well-known French gastrologist, Mathieu, used to describe two varieties of nervous gastric hæmorrhage. The first he called hysterical, the second emotional, and he had a case of a man who had an attack of profuse gastric hæmorrhage after he had been startled by the report of a gun. But in the absence of direct control we cannot exclude the possible existence of an ulcer.

The research work of Professor Latarjet [6], of Lyons, in which he is now engaged, may possibly throw some light on this question of ætiology. We are indebted to this anatomist for the most accurate study of the gastric nerve supply. He has performed many resections of the nerves of the stomach in cases of gastric crisis due to tabes, ulcus, and various gastralgias. In the course of his experiments on dogs he saw that resection of the nerves proper to the stomach is followed by very marked vaso-dilatation of the stomach wall, of the omentum and of the peritoneum (even to the extent of the appearance of ecchymoses) *but the mucous lining does not partake in the phenomenon*. Vaso-dilatation of the mucous lining is perhaps under the control of local centres, and we do not know at present what may be the track followed by the afferent nerves. These findings lead on to the question of the pathogeny of hæmatemesis—a vast field yet to be explored. We may admit that gastrostaxis is due to some disturbance of the normal reaction of the general nervous system to the visceral system—and we may also assume that there are local circulatory circuits regulated and, it may be, disturbed, by the neighbouring organs. In this way the occurrence of hæmatemesis in cases of appendicitis and cholecystitis may be explained. It is to be hoped that physiologists will supply us with the key to the meaning of some of these cases at any rate.

We owe to Müller [7] knowledge of the location of sympathetic vaso-motor centres in the tractus intermedio-lateralis of the medulla. A short time ago Mariano Castex [8], at the Paris Academy of Medicine, showed that the same centres are also vaso-trophic (as has been proved in cases of purpura).

We may admit the fact of disorder of these nuclei, or of the sympathetic nerve ganglions, or of the nerve-endings, as a cause of hæmatemesis. In the present condition of our knowledge we can but mention the problem.

As I am a follower of the late Franz Glenard, I must therefore be expected to mention hepatism as a possible cause of gastrostaxis. You are aware that this great Vichy clinician—a worthy follower of Murchison—taught us the part played in pathology by small defects of liver-cell adequacy. Hepatism accounts for many forms of hæmorrhage due to slight changes in the vessels.

And, further, if we admit that the immense majority of patients suffering from Glenard's disease are really what he called himself, "des hépatiques," how can we be otherwise than seriously impressed by the figures given by Mr. Walton [9] in his recent paper on chronic dyspepsia in women? Mr. Walton writes: "Hæmatemesis is a frequent symptom in the gastric type, especially in women. In my own experience some 50 per cent. of the cases have had a sharp loss of blood at some time or other of the disease. The loss of blood is manifestly due to the supervention of small gastric erosions in a dropped stomach." How pleased my old master would have been to read these lines. He would have explained the fact not from erosions due to a dropped stomach, but would have said that in patients suffering from liver insufficiency both gastroptosis and hæmorrhage are likely to occur. Mr. Walton adds: "Are such patients suffering from an organic lesion as well as their chronic dyspepsia? and if it be certain that they have no organic lesion, should they be treated by operation?"

These small erosions recognized by Mr. Walton in ptosis have been described by Professor Dieulafoy. But Dieulafoy, unlike the London surgeon, used to teach that these minute ulcerations follow a rapid and acute course. The hæmorrhage is the first symptom.

In one of our cases we have mentioned the possibility of syphilis. Syphilis is often a cause of bands and kinks, and consecutive stenosis might quite likely be a cause of gastrostaxis. This is a fact to be kept in mind.

Prognosis is good. Out of 7,500 post-mortem records at Guy's Hospital, Sir William Hale-White finds four deaths due to gastrostaxis. I have not come across any French statistical data, but the best proof of the benignity of the symptom is that none of the old authors speaking of gastric hæmorrhage ever considered it to be a very serious accident.

As regards treatment, we must distinguish between the treatment of the condition and the treatment of the symptom. The treatment is surgical or medical. Neither Sir Berkeley Moynihan nor Lord Dawson recognize any marked improvement due to operation. On the other hand, Professor Duval found his patients very much better for being operated upon. A curious explanation was offered. It was said that his incision happened to cut across a small ulcer which was afterwards stitched up without being recognized. It is difficult to admit this explanation in one case, but in two cases it is impossible to do so. It has also been suggested that a long incision through the stomach wall produces enervation of the stomach—the operation of Latarjet. This might be admitted, and as a matter of fact in one of Latarjet's cases, the patient who suffered from hæmatemesis was cured after gastric enervation.

The practical rule that we might adopt is that if X-rays do not give any definite information as to the origin of the bleeding no operation is justified, at any rate in our present condition of knowledge. Besides, a patient who has had severe hæmorrhage is in poor condition to stand the shock.

Medical treatment, as has been demonstrated by Sir William Willcox,

consists primarily in complete rest. Dr. Hurst gives a striking statistical table demonstrating the low mortality of the condition. As the question has been exhaustively discussed at this Society this very month, it seems unnecessary to say more. I am only sorry that the hydrologists have not had a word to say about treatment of the condition which leads to the hæmorrhage. This side of the question might well be of considerable interest.

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The Clinical Interpretation of Fever.

By OLIVER HEATH, M.D.

ONE cannot doubt that fever has been a common experience of man for countless generations: yet I think I may say that its meaning remains a mystery.

The discovery of bacteria has shown us the causal agents, and we now know that immunity is in some way linked up with fever, but the main question is still unanswered, "Is fever an event of ill omen and to be combated as such?" or, "Is it a natural method of defence against bacteria?"

The literature, with which I have dealt more fully in a previous paper [1], reveals few theories; these refer only to the cause and each puts forward some form of split protein the toxic properties of which are said to excite fever [2] and [3]. Acceptance of such ideas would lead one to consider fever as a symptom of poisoning.

The only theory I know of which embraces all the events of fever is the one I am now bringing to your notice, the experimental aspect of which is embodied in the paper referred to above [1]. Briefly, I hold that the bacteria in the lesion are continuously neutralizing antibacterial substances; that the resulting diminution of the antibacterial power of the circulating blood is the cause of fever; and that fever is a defensive response intended to get rid of the bacteria and restore the blood.

The present paper is partly founded on and partly in support of the above; the two are complementary.

The description of fever leads one in a circle, the reason being that the bacteria are (1) the agents which cause the fever, and (2) the objective at which the reaction is aimed.

Diagram I.—I have endeavoured to put this in diagrammatic form. Starting in the lesion, the bacteria neutralize the antibacterial property of the lymph which is returned in a continuous flow to the circulation and causes gradual impoverishment of the circulating blood. This affects some nerve centre from which three, or perhaps four, impulses proceed, (1) dilates the vessels in the lesion and starts inflammation; (2) inhibits intestinal activity and causes malaise and anorexia; (3) (if it acts) stimulates some unknown organ to

produce antibacterial substances, and (4) constricts the vessels of the skin and starts processes which raise body temperature. These events are common to all fevers, but in those due to bacilli of the typhoid, dysentery, and *Bacillus coli* groups there is an additional change of function by which specific antibacterial substances are produced after about the ninth day. The description will follow this schema, and antibacterial substances will be referred to as antibodies.

The Lesion.—It is debatable whether inflammation is caused through a local or central reflex: some authorities think that the bacteria or their hypothetical toxins are tissue irritants and start inflammation through a local reflex.

Personally, I find that after injection of typhoid vaccine into my subcutaneous tissue, several hours elapse without local sensation; bacteria are not therefore irritant in the ordinary sense and dead bacteria cannot evolve toxin—yet inflammation follows. Apart from the prick of the needle, the first sensations I experience are synchronous with appearance of signs of inflammation and are, as we know, due to tension in the vessels. This probably means that when contact of the bacteria and lymph has led to slight deterioration of the blood, inflammation is excited through a central reflex; and when the blood becomes seriously affected the stronger stimulus brings fever to the aid of inflammation; in a clinical analysis of the common cold [4] I found that this is what actually happens—when inflammation fails to cure, fever develops.

The Systemic Symptoms.—Inflammation having failed to cure, the second impulse inhibits intestinal activity. The patient feels ill (malaise), and loses desire for food (anorexia): the observer notes cessation of bodily activity, and cessation of ingestion of food.

I interpret this to mean that the patient has given up all normal forms of activity which use energy, but are not immediately essential to life. The excretory functions, which are essential, are not inhibited.

This makes available a supply of energy for defence. So that the primary change in fever is a redistribution of energy, with concentration on defence. This change is really preparatory, and may be considered as of a negative character.

Fever Reaction Proper.—The only two positive, or active, processes underlying fever are (1) production of antibody, and (2) production of heat, which is auxiliary to the former.

The two are in many ways comparable. Both function in a small way in health to keep body temperature and the antibacterial power of the blood within a certain close range, to which we refer as the normal of health.

At the onset of an acute fever the activity of each process is increased, but later, in the bacillary fevers I am especially considering and possibly in all fevers, the normal is replaced by a special process which greatly increases the output and adjusts this to raise body temperature and the antibacterial power of the blood to higher levels, which I propose to call the normals of fever.

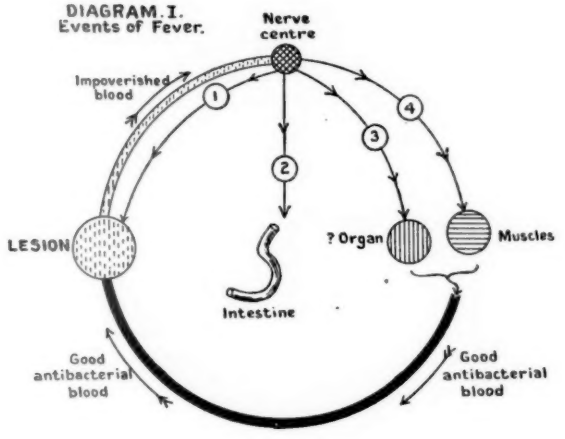
Antibody.—We know nothing of the method or site of production.

When the change to special function has secured the increased output of specific antibody, the blood-content of these substances does not reach a high fever normal equivalent to this output owing to continuous and rapid neutralization by the bacteria; this continuous neutralization, by keeping the blood below the fever normal which the patient is working to attain, is the cause of the continuance of fever. The actual antibacterial power of the blood is the

resultant of two forces—production by the patient and neutralization by the bacteria.

Three advantages accrue to the patient by this change: (1) change to special function in some way enables him to produce (specific) antibody with less expenditure of energy (taking equivalents, rise of temperature is less when specific antibody is formed) [1]; (2) each cubic centimetre of blood is made a more effective destroyer of bacteria owing to increased concentration of antibody, and this allows a corresponding decrease of inflammatory circulation; (3) if the patient react strongly and the blood is raised above the normal of health there is a proportionate abatement of symptoms and, if food can be taken, the final result is the restoration as well as economy of energy.

So far, then, the initial diminution of antibacterial power of the blood below the normal of health caused severe symptoms; but when the special fever process has raised the blood above the normal of health the symptoms are abated. This means that the transference of energy from normal to defence



functions (which results directly from the symptoms) occurs in strict harmony with the needs of the blood.

Temperature.—The fourth impulse constricts the vessels of the skin and produces a sensation of cold.

In health the sensation of cold excites one to take muscular exercise, but (and here I am thinking of the effect sea-bathing has on me personally) if after exposure one avoid voluntary muscular exercise, one passes through a stage of shivering, followed by action of the pectoral muscles causing a shrugging movement of the shoulders and finally general muscular contraction and chattering of the teeth, i.e., the autonomic nervous system has assumed control and caused rigor.

That rigor is directly due to the sensation of cold has been shown by experiment: if an animal be put into a state of rigor and the surrounding temperature is then raised, rigor ceases. Clinical experience also tells us that sufficient warmth will quieten rigor [5].

Rigor in fever occurs only in response to a sudden neutralization of antibody.

The sensation of cold is probably also the stimulus which excites the special fever process. This produces heat from increased metabolic activity, again chiefly in the muscles, but turns all available energy into heat and wastes none on movement; this means a further saving of energy.

The resulting rise of temperature has the effect of accelerating antibody production. Clinically, we know that the pulse and respiration-rates increase as the temperature rises; this means that metabolism increases as the temperature rises (the blood supplying raw material, while the inspired air supplies oxygen and the expired air carries off waste products).

The clinical evidence, therefore, shows that when the temperature has been raised there is a great increase of metabolic activity, and since less energy will will be required to sustain than to raise the temperature, it seems clear that the additional energy thus released will go to the production of antibodies which, as I have said, constitutes the only other active process underlying fever. The clinical evidence seems to afford very clear and strong support to the experimental, to the effect that the production of antibodies is accelerated by the rise of temperature.

One would expect, then, the rise of temperature to occur in relation to the need for production, and in the experiments referred to, I found that the height to which the temperature is raised does in fact depend on what fraction of the antibody capacity or output is neutralized, e.g., neutralization of half the output will cause a higher temperature than neutralization of a quarter of the output, and this holds good whether the output is large or small. In other words, increased output meaning quicker production, the patient increases the pace of production in harmony with the pace at which the bacteria are neutralizing antibody.

This brings all the clinical features of fever into relation with the state of the blood, and the circle is completed by bringing the increased output of antibody, through the circulating blood, back to the lesion from which the depletion which caused fever originated.

Control of Fever.—I have suggested a common cause for inflammation and fever, and in experiment, and I think also in disease, one finds the two processes increase in severity together.

The increase of inflammation still further lowers the blood power, and so excites more severe inflammation and fever, and there seems at first sight to be a vicious circle. But when the reduction of blood power becomes inconsistent with the safety of the organism, an automatic regulating mechanism comes into action as follows: (1) In the lesion the increased tension of the vessels causes a sensation varying from discomfort to pain, which in its turn causes a decrease of activity of the part or finally complete inaction, and this again, by slowing the inflammatory circulation, tends to lessen the causal stimulus; (2) at the same time in the system the more severe fever reaction means larger output of antibodies, so that the two processes work together in harmony to prevent a dangerous reduction of the antibacterial power of the blood.

Summary.—Diagram II: Events of Fever.—In the lesion, bacteria coming into contact with the lymph neutralize antibody, and the return of lymph to the circulation leads to a gradual lowering of the antibacterial power of the circulating blood.

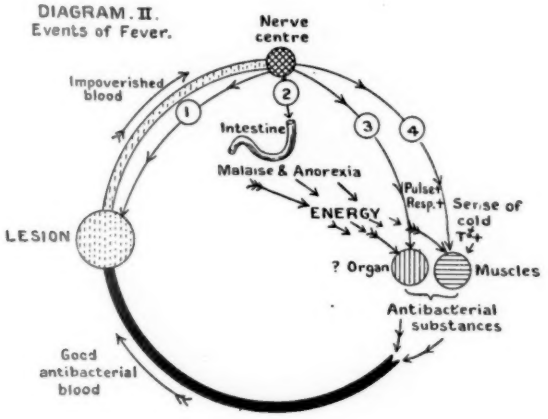
When the blood is slightly affected a central stimulus excites inflammation.

When the blood becomes seriously affected, i.e., when inflammation has failed to cure, a second central stimulus inhibits intestinal activity and causes malaise and anorexia and a transference of energy from normal functions to those of the defence.

A third stimulus, by causing constriction of the vessels of the skin, produces a sensation of cold which excites production of heat, first by muscular contraction, later by a special fever process which economizes energy; this results in raising body temperature to a higher mean level.

As the temperature rises, increased pulse and respiration-rates show that metabolic activity is increased, and the increase is expended on antibody production which is in this way accelerated by the rise of temperature.

In certain bacillary fevers (and perhaps others) a special process of antibody production replaces the normal, specific antibodies appear and output is greatly increased: if the patient react well, the antibacterial power of the blood is now raised above the normal of health, and reduction of both inflammation and fever is associated with a further saving of energy.



Finally, if the blood power is reduced too far for safety, tension in the lesion causes pain, which tends to slow inflammation at the same time as the systemic process has increased production of antibody: inflammation and fever are controlled as one combined defence process.

Cases.—There are certain clinical points in relation to the rise of temperature which can be best explained from an actual experiment.

Chart I.—This represents the effects of an intravenous injection of 500 million typhoid vaccine.

Reaction was extreme. Timing events from injection, in five minutes I was pale and cold; malaise and anorexia increased in severity and in less than twenty minutes I was in bed fully dressed and in the throes of a full rigor.

Severe symptoms covered thirty-eight hours, rest in bed forty-four hours, and the temperature was above normal fifty hours.

The two points to which I want to call attention are:—

(1) On another occasion, subcutaneous injection of the same dose of 500 million typhoid vaccine caused only one rise to 102° F., covering about fifteen hours, with mild symptoms. The comparison shows increased severity and prolongation of all the evidences of fever in response to increased intensity of attack, i.e., to the more sudden neutralization of antibody following intravenous injection.

(2) Rigor took the temperature up to 100° F., i.e., 1½° F. in two hours; while the metabolic process caused a further rise to 102·5° F., i.e., 2½° F. in one hour. This shows well, rigor coming to my aid in a sudden emergency, and that the metabolic process raised the temperature about two and a half times as quickly as rigor.

Interpretation of Clinical Evidence.—Method (Chart I, continued).—In this extremely acute reaction the clinical features are clearly differentiated,

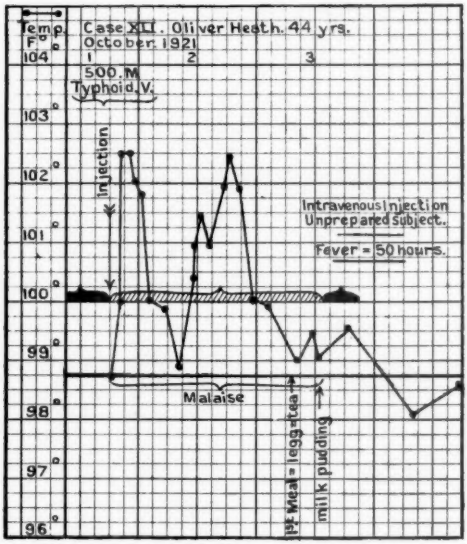


CHART I.

and in the absence of blood tests (which I was too ill to do) I will endeavour to indicate my idea of how we should interpret the clinical findings.

(1) Extreme rigor means very sudden neutralization of antibody. It does not mean low blood power, because I have experienced a rigor during which my blood was at no time less than twenty times normal strength.

(2) The severe malaise and anorexia mean that the blood power was reduced far below the normal of health.

(3) The very rapid initial rise of temperature is further evidence of sudden attack. The rise to 102·5° F. means that a large fraction of my total antibody was neutralized (judging from experience of successful fever reactions my optimum seems to be about 103° F.). The equally sharp fall indicates necessary rest after an acute effort, and is the analogue of the nightly fall which occurs in health.

On the first day, there is, then, clinical evidence of a very sudden neutralization of antibody which reduced the antibacterial power of my blood approximately to zero; and from the experiments referred to I have made calculations from which I conclude that this dose of 500 million typhoid bacilli was capable of neutralizing one and a quarter times the total quantity of antibody in me at the time.

(4) On the second day, absence of rigor and less acute rise of temperature show that the emergency had passed. The continued severe symptoms and the rise of temperature to the same point, 102·5° F., show that the blood power was still very low.

(5) On the third day, symptoms abated so much that I took light food for breakfast and a short walk after lunch, which, with the lower temperature, show that blood power was rapidly recovering towards normal.

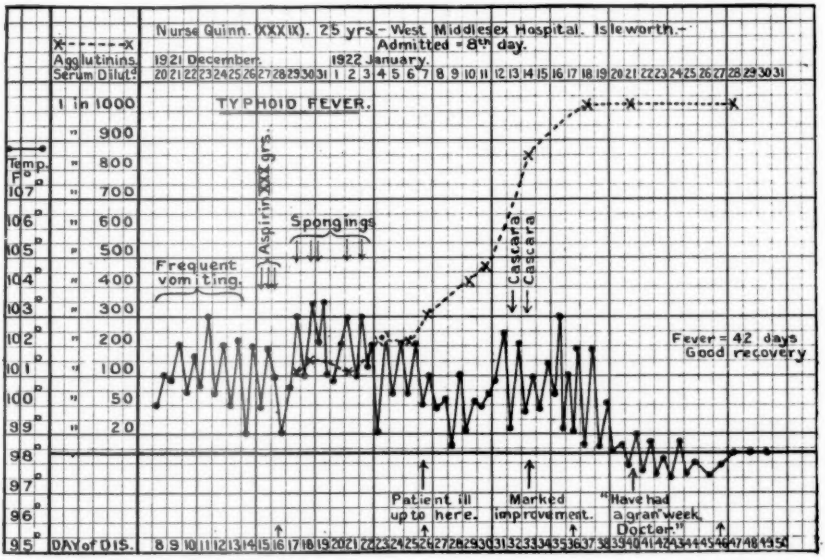


CHART II.

I have not mentioned the pulse or respiration-rates as they do not refer to the state of the blood; but when taken in conjunction with the rise of temperature they have, as I have indicated above, an important meaning which bears on the metabolic activities going on in the body in connexion with antibody production.

Chart II.—Comparison of Blood-curve and Symptoms.—This is a case of typhoid in which I followed the agglutination titre and symptoms. Fever covered forty-three days, but my record only begins at the seventeenth day of disease.

Symptoms were pretty severe up to the twenty-sixth day, and the temperature indicates that a large fraction of antibody output was being neutralized.

One would then expect the blood to be low, and in the first three Widal tests clumping was extremely weak and only just positive.

In the fourth and fifth tests clumping was strong and definitely positive, and it is from this point the symptoms abated rapidly week by week while the agglutination curve was steadily rising.

From the twenty-sixth day to the end, while the blood was rising to a high level, there is a serious (and rather misleading) blemish in the temperature; this was caused by obstinate constipation which was relieved by doses of cascara on the thirty-second and thirty-third days, from which date lysis was resumed. Apart from this mishap, I think lysis should have been completed about the thirty-first day, because records of other cases of typhoid show that the blood does not reach the high fever normal until some time after the temperature has become normal, i.e., after neutralization of antibody has ceased.

The temperature curve from the seventeenth to twenty-second day includes the highest records, which show a large fraction of antibody production is being neutralized: agglutination is scarcely above normal.

From the twenty-third to thirtieth day the rising blood shows antibody output steadily overtaking neutralization, i.e., the neutralized fraction of output is steadily decreasing, and during this period temperature is falling steadily.

From this point the facts in hand seem clearly to indicate constipation as the sole cause of the further rise of temperature. Without this complication, as I have suggested, lysis should have been completed by the thirty-second day; the record would then have shown events as they occur in their natural sequence. As it stands, the case shows that one must watch for what might be called extraneous causes of a rise of temperature.

Chart III.—This is a case of acute cystitis in which fever covered forty-seven days; the dotted lines indicate rigors, and the blood-culture on January 31 yielded a *Bacillus proteus* and a streptococcus.

The very sharp drop of temperature from February 2 almost certainly indicates the point where, specific antibodies for the bacilli having appeared in the blood, the output of antibody is increased and the bacilli are now able to neutralize only a comparatively small fraction of this increased output.

The predominance of output over neutralization would lead to increased antibacterial power of the blood, and Dr. Turtle tells me that it is from about this point that he noticed the patient was beginning to feel less ill, though temperature continued and the condition of the urine remained unaltered.

On February 15 I saw the patient and found the symptoms mild, and in discussing with Dr. Turtle what dose of vaccine to give I expressed the opinion from what I saw that the blood power was well above the normal of health. To confirm this, fresh cultures were obtained from the urine, and on February 19 the patient's blood, when compared against mine, showed indices of 3·3 to the *Bacillus proteus*, 7·8 to the *Bacillus coli*, which it also agglutinated up to 1 in 3,000 (control nil), and 1·4 to a *Staphylococcus aureus* which was also isolated.

We were then able to increase the dose of vaccine with confidence. But the practical outcome of this paper is that one should have this kind of confidence from the clinical findings alone, and not need to have recourse to blood tests.

Practical Conclusions.—It is possible from the clinical evidences of fever (1) to estimate whether the antibacterial power of the blood is much below or well above the normal of health. This is shown by the severity of the systemic

symptoms. (2) To estimate whether a large or small proportion of the antibody output is being neutralized by the bacteria. This is shown by the rise of temperature. (3) To gain from these two together some idea of the state of the battle between antibody production by the patient and neutralization by the bacteria. (4) From the different relationship of temperature and symptoms to the state of the blood, to explain how of two persons with similar temperature curves one may be very ill and the other comparatively well—a fairly common clinical observation. (5) Though I am not concerned here with the treatment of fever, I would draw attention to the fact that each of the three main sensations or symptoms of fever causes a change which is beneficial to the

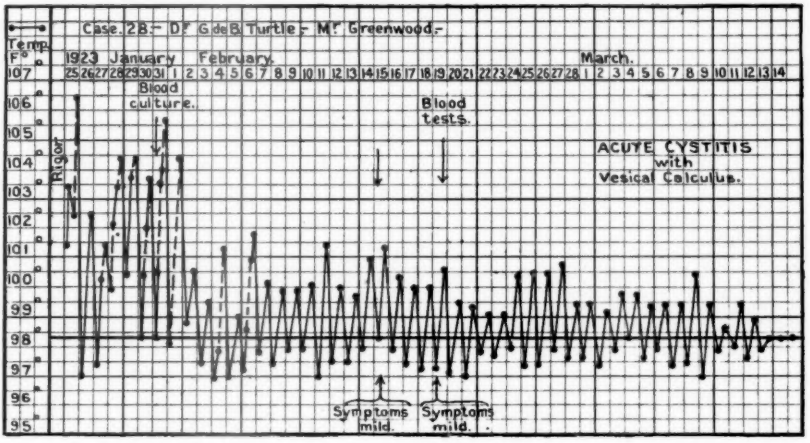


CHART III.

patient: (*a*) Malaise and anorexia cause the transference of energy to defence purposes. (*b*) The sensation of cold excites production of heat. (*c*) Pain in the lesion exerts a controlling influence when things are going badly. (6) If one accept that the rise of temperature in fever accelerates antibody production, that fact would contra-indicate exhibition of antipyretic drugs in fever; and if one add that the slowing of pulse and respiration which accompanies the fall of temperature shows decreased metabolism, it becomes clear that antibody production, the most important and vital event in the reaction, will be retarded by this form of treatment.

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Section of Medicine and Section of Surgery.

JOINT MEETING.

DISCUSSION ON "THE SURGICAL TREATMENT OF PULMONARY TUBERCULOSIS."

AFTERNOON MEETING:

Chairman—Dr. ROBERT HUTCHISON (President of the Section of Medicine).

Ninety-three Cases of Extrapleural Thoracoplasty.

By P. BULL.

(Professor of Clinical Surgery, Christiania.)

WITH regard to the theme of this evening, viz., "The Surgical Treatment of Pulmonary Tuberculosis," as there is much that I wish to say, I will pass over all historical and other details, and so far only draw attention to my papers in the *Lancet*, October, 1920, and the *British Medical Journal*, December, 1922; and as regards theoretical problems I will only briefly touch on those with which I have personally been engaged. My experience is almost exclusively limited to extrapleural thoracoplasty in connexion with apicolysis, whilst I have had little or nothing to do with other surgical methods of treatment in the campaign against pulmonary tuberculosis.

I shall first give an account of my indications and technique; then a general survey of the immediate results of the operation and the results obtained by observations extending over many years. Finally, I propose to show you a number of lantern slides which in various ways will illustrate what I am saying in the course of my paper.

INDICATIONS.

In order to understand the object of extra-pleural thoracoplasty it is necessary to remember that healing of tuberculous processes takes place, as a rule, by the development of fibrous tissue with consequent scarring and retraction. In the lungs, however, the conditions for retraction are less favourable than in many other parts of the human body, in so far as the lungs cannot shrink any further than is permitted by the negative pressure in the pleural cavity, or than the stiff thoracic wall can follow. A maximum shrinkage of the pulmonary tissue therefore cannot take place until the lung has collapsed which, as we all know, we endeavour to achieve in practice by

means of gas inflation of the pleural cavity. In many cases, however, this treatment cannot be carried out, at least not in an entirely satisfactory manner, because there are entire or partial adhesions between the pulmonary and parietal pleura. It is in the latter condition that we resort to extrapleural thoracoplasty, i.e., remove subperiosteally large portions of many, most often ten or eleven ribs.

In the treatment of pulmonary tuberculosis, extrapleural thoracoplasty thus belongs to the same category as artificial pneumothorax: both aim at collapse of the lung with resulting retraction on account of the development of connective tissue, and thereby healing. As, however, artificial pneumothorax is less risky than thoracoplasty, I demand as a rule that inflation shall have been employed either without improvement, or unsuccessfully on account of extensive adhesions, before the patient is advised to undergo thoracoplasty. On the whole it may be said that the indications for thoracoplasty coincide with the indications for artificial pneumothorax, and that the former should be employed when the latter cannot be used.

In cases of doubt, repeated hæmoptyses should hasten the decision to undertake thoracoplasty. To illustrate this, I may mention that a patient of mine had one or more attacks of hæmoptyses every year for seventeen years. Exactly six years ago I performed thoracoplasty, and she has had no spitting of blood since, and is now quite well.

As regards the so-called "healthy" lung, it should be required that there are not, nor have been, demonstrable clinical symptoms in that lung. So strict a demand would mean few operations. We are satisfied, when symptoms in this lung have quite disappeared, or, during a long period of expert observation, have remained stationary and of slight extent. Even though there may be little or nothing to find clinically in the "healthy" lung, it happens not infrequently that in the X-ray photograph we can find such extensive changes that an intended operation has to be abandoned. An X-ray examination of both lungs is therefore absolutely essential.

In my opinion we should never operate on a patient if we believe that there is progressive or greatly extended tuberculosis in the other lung. The chances of a lasting, good result are then so small, that such undertakings will only serve to render unpopular an otherwise valuable form of operation, both amongst surgeons and patients. If it is doubtful whether demonstrable alterations in the "best" lung are stationary or progressive, the operations should be postponed for the time being, and the patient kept under close observation during the immediately succeeding months.

Are there really so many patients with tuberculosis confined to one lung? Yes, undoubtedly there are. As a surgeon I had naturally enough no real experience regarding these matters. I can only say, that I am constantly surprised to find how many patients there are who suffer from unilateral pulmonary tuberculosis, or more strictly speaking, whose tuberculosis in one lung is only slightly extended and has subsided temporarily or for good.

As regards *contra-indications*, a slight tuberculosis of the larynx will not prevent the operation, as it may well be assumed that any further improvement in pulmonary tuberculosis, with diminished cough and expectoration, will have a favourable influence upon the laryngeal tuberculosis, in analogy with the improvement in a tuberculosis of the bladder after removal of a diseased kidney. Neither does tuberculosis in one kidney constitute an absolute contra-indication; in the autumn of 1921 I performed thoracoplasty and four weeks later removed the left kidney on the same patient, who at the

present time, two years afterwards, is doing very well as a chauffeur (cf. fig. 1). Absolute contra-indications are found in an advanced stage of tuberculosis in the other lung, and the general contra-indications which militate against any serious operation, for instance a bad general condition.

It must be strongly emphasized that this branch of surgery demands intimate co-operation between the surgeon and the physician in charge of the case. Best of all, the patient should be observed and treated at a special sanatorium before the operation. In the case of the sanatorium physician, who may be assumed by daily observations to know the patient thoroughly, there appear two difficult problems for solution: (1) Can it be decided whether



FIG. 1.—*Case* 65. Male, 27 years. Duration of disease 1½ years. Total left-sided pulmonary tuberculosis and tuberculosis of left kidney. (i) September 29, 1921, resection of costal ~~cartilages~~, I to XI. 110·8 cm. of ribs removed. (ii) October 26, 1921, nephrectomy. October, 1923: He has remained cured from both affections and is doing very well as a chauffeur. The photograph shows two scars. The upper one after thoracoplasty, the lower one after nephrectomy.

any existing physical signs in the "healthy" lung are due to a healed-up, a stationary or a progressive process? (2) He must ascertain at an early stage the moment when he considers continued expectant treatment, including artificial pneumothorax, to be of any value for the patient's health. It is an easy matter to wait too long, so that when at last the patient is sent for operation, large cavities have already developed in the lungs. Even though I have patients who, in spite of cavities as large as a hen's egg, or even larger, are still healthy from one to six or seven years after the operation, there is no doubt that cavernous unilateral pulmonary tuberculosis gives a much more unfavourable prognosis as regards permanent cure after thoracoplasty than the infiltrating shrunken forms.

I must confess that Norwegian sanatoria physicians from the beginning have been aware of the importance of thoracoplasty, so that co-operation between them and surgeons in Norway may now be said to be almost ideal. This may be connected with the circumstance that the initiative towards the operation proceeded from a man in their own ranks, viz., Dr. Holmboe, of Mesnalien Sanatorium. It may be that co-operation would not have been so satisfactory if the initial steps had been taken by a surgeon. But presumably such subordinate considerations play no part in this country, when there is a question of opening up the way for life-saving operations.

From what I have said, it will appear that it is the physician in charge of the patient who decides upon the time suitable for the operation, whilst the surgeon must form an independent opinion as to how far an operation is really advisable in each particular case. It may happen that the surgeon is more experienced with regard to the importance of contra-indications, when it is a question of such a great operation as thoracoplasty. In this domain, as in others, the surgeon must not condescend to perform operations "to order."

I will now pass on to the

TECHNIQUE.

The patient is placed upon the healthy side, with a large sandbag beneath, in order to press forward the diseased side. I have always performed the operation by the aid of the hook-shaped Sauerbruch incision on the back (fig. 2). The arm is drawn outwards, and the skin and muscles are cut down to the lumbo-dorsal fascia. The scapula with the covering soft parts can then without difficulty be lifted out from the thoracic wall, and it is then possible to remove as large portions of the ribs as may be desirable. The question is now, which ribs to remove and how much of them? As the object of extrapleural thoracoplasty is not only to get the lung to collapse, but also to put the diseased lung out of action, the resection must include many ribs. Even if the infection of the lung is limited, the resection of the ribs should proceed far beyond the diseased part of the lung; indeed, it may well be said that extrapleural thoracoplasty has developed into a typical operation, in which resection is always made from the tenth or eleventh, up to and including the first rib.

It is sufficient to resect 6 to 7 cm. of the eleventh rib, 12 cm. of the tenth and ninth, and 15 cm. of the following ribs up to and including the fourth; of the three uppermost as much as possible is taken, which in general will be 12 cm. for the third, 10 cm. for the second and 2 to 3 cm. for the first rib. The total amount of the resected ribs thus amounts to about 130 cm., varying from 90 to 180 cm. I have resected about 125 yards of ribs from ninety-two patients.

An extremely important point in costal resection is that the ribs ought to be resected as far back as possible—that is to say far beyond the costal angle, right up to the costal tubercle, and the point of the transverse process. It is almost thirty years ago since the French authors Boiffin and Gourdet emphasized how important it is in the treatment of chronic empyema to remove the posterior parts, which are the most rigid and most curved portion of the ribs; if we remove all the posterior part, the anterior part of the ribs, on account of the flexible costal cartilage, will easily be pressed inwards towards the thoracic cavity, and the costo-vertebral angle will to a large extent be effaced (fig. 3). It is this principle in costal resection which has now been transferred to extrapleural thoracoplasty. The criterion of a correctly performed thoracoplasty is, that you can feel the posterior margin of the scapula lying in front of the posterior ends of the resected ribs.



FIG. 2.—*Case* 74. The photograph shows the hook-shaped incision, the scar being marked out with silver nitrate.

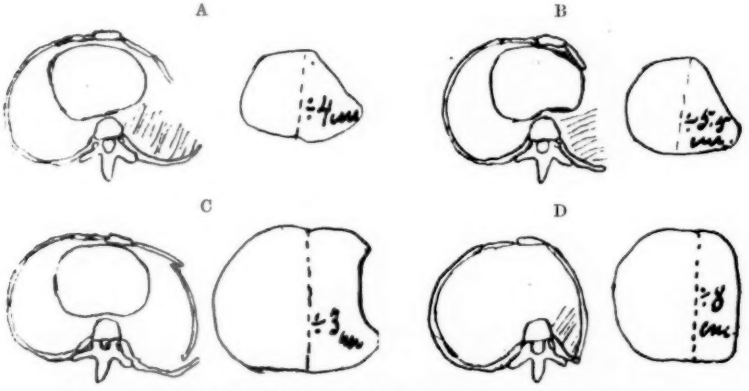


FIG. 3.—Different kinds of thoracoplasty for chronic empyema after *Gourdet*.[1]

(A) Method of *Estlander*, resection of 8th to 4th rib, 38·5 cm. Diminution of circumference 4 cm.

(B) Method of *Estlander*, resection of 9th to 2nd rib, 110 cm. Diminution of circumference 5·5 cm.

(C) Method of *Quénu*, resection of 8th to 2nd rib, 28 cm. Diminution of circumference 3 cm.

(D) Method of *Boiffin*, resection of 11th to 3rd rib, 54 cm. Diminution of circumference 8 cm.

[1] *Gourdet*, Julien: "Étude sur l'aplatissement comparé du thorax par les différents procédés de résection costale et spécialement sur un nouveau procédé de thoracoplastie." Thèse, Paris, 1895.

When, after having resected the ribs lying below, I reach the fourth or the third rib, I always perform *apicolysis*—that is to say, after resection of the fourth or third rib I make an incision through the periosteum and the endothoracic fascia; you can now with the finger, without difficulty, loosen the top of the lung from the thoracic wall, so that between the lung and thoracic wall there arises an extrapleural cavity larger than a hen's egg. If there are then cavities in the apex, we may sometimes both see and feel how they collapse. If apicolysis has been performed, resection of the two upper ribs, the most difficult part of the operation from a technical point of view, will proceed more easily since it is then unnecessary to entertain any fear of injuring the lung. In order as far as possible to avoid the contents of the cavity being forced into



FIG. 4.—Author's raspatory for the first rib, a = 3 cm.

the bronchi, it is necessary for the patient on the morning of the day of operation to cough up as well as he can all the contents of the diseased lung.

Resection of the first rib may be somewhat troublesome. In order to facilitate this I have modified the usual Doyen's costal raspatory, so that the effective part of the instrument does not cut vertically in relation to the handle, but coincides with the axis of the latter (fig. 4).

The muscles and skin are each stitched up separately. A large glass drainage tube is placed in the lower posterior angle of the wound, although I have sometimes entirely closed the wound without any disadvantage. The bandage must be supported by three broad strips of plaster, running horizontally, so that the thoracic wall, which has been mobilized by the operation, may not give way too much to the shock of coughing during the first few days after the operation.

It may be difficult to get *cavities in the apex* to collapse by means of thoracoplasty and apicolysis alone. I have on twelve occasions performed (following the example of Tuffier) *free transplantations of subcutaneous fat*, first resecting the third or preferably the fourth rib in the axilla, and then performing a new apicolysis from the place of resection. In the extrapleural space, larger than a hen's egg, thereby created, there is then placed a large

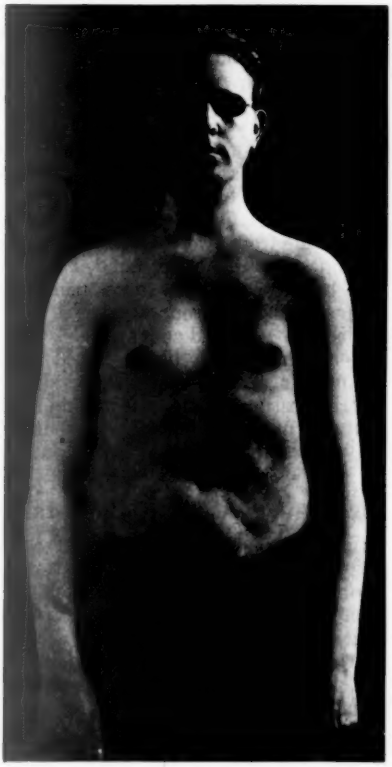


FIG. 5.—*Case* 30. Male, 24 years. Onset of the disease two and a half years ago. Total left-sided pulmonary tuberculosis with ramollissement, cavity in the top. (i) September 24, 1918: Resection c. XI to V. (ii) October 25, 1918: Resection c. IV to I. 129 cm. of ribs removed. (iii) December 3, 1918: Resection c. III plus extrapleural transplantation of fat from abdominal wall. October, 1923: Slight cough without tubercle bacilli. Does the whole work of a teacher. The photograph shows the abdominal scar and the place from whence fat for transplantation was taken.

piece of fat taken from the anterior abdominal wall (fig. 5). In one case I used with good results half of the right breast instead of fat; in another case after resection of the anterior parts of the first to the fourth ribs, I loosened the lung from the thoracic wall and pushed the pectoralis major and minor muscles into the cavity after having severed their attachment to the humerus and the

coracoid process respectively. I must admit that the last operation did not fulfil expectations.

The next important question is: Should the operation be performed in one or two stages? I have tried both ways, and my experience shows that two stages are less risky than one. In seventy-one cases I planned to complete the operation in two stages. It happened on two occasions that the second stage of the operation was not performed, because the patient's general condition was too bad. Two patients died after the first stage of the operation, one after the second, but sixty-six survived both stages—a mortality of only about 4·5 per cent. Of twenty-one patients upon whom the operation was performed in one stage, six died—a mortality of about 30 per cent. I must, therefore, advise those who desire to try these operations to perform the first of them at any rate in two stages.

In performing the operation in two stages, the incision in the second stage is much higher up than in the first, and proceeds downwards to about the angle of the scapula, and from thence farther forward. In the operation in two stages I resect ribs eleven to six or five in the first stage, and five or four to one in the second stage. As a rule from two to three weeks should elapse between each stage of the operation. On the other hand, I have sometimes obtained good final results when I have been obliged to postpone the second stage of the operation for many weeks, because the general condition of the patient did not permit of the operation being performed sooner.

Should general or local anæsthesia be employed? Most of my patients have been operated on under local anæsthesia, but I have gradually resorted more and more to general anæsthesia—chloroform and ether, 1 in 3—and I am astonished to find how well on the whole my patients stand this. In this country, where you have special anæsthetists, I should unreservedly recommend the latter.

Course of the Disease and After-treatment.

During the operation the pulse-rate often rises to 120 or 130 or even more, and the respiration-rate to 40. When the patient has been put to bed again, the pulse and respiration usually improve quickly. In some cases the pulse does not rise to more than 80 or 90, the respiration not to more than 26 or 28 or less. Some patients vomit during the operation.

Immediately after the operation we observe a marked collapse of the thoracic wall with paradoxical respiration, i.e., the mobilized part of the thoracic wall is drawn inwards during inspiration and pushed outwards during expiration. Dyspnœa, troublesome expectoration, and pains in the chest, radiating to the shoulder and the upper arm, especially trouble the patient during the first days after the operation. The dyspnœa is partly due to "mediastinal flutter," partly to the laboured action of the heart on account of displacement. The heart has to become accustomed to the displacement before it can act normally.

Expectoration must be relieved. This is done in various ways. An extra nurse remains with the patient day and night in order to support the operated side during attacks of coughing, and in order to encourage him to expectorate. A good compressing bandage is of importance both as regards the cough and in order to produce collapse of the operated side. But above all, it is important to give the patient sufficient narcotics, morphine or omnopon, in order to relieve pain and thereby facilitate expectoration, and moreover narcotics have a beneficial influence upon an eventual "mediastinal flutter."

The temperature often remains high and the pulse frequent (figs. 6-10) for four or five days, but in cases progressing favourably it then falls to about normal; at the same time the pain also diminishes considerably. The patient soon begins to feel better, and is especially gratified because the cough is so much less troublesome. The large wound almost always heals by first intention and the patient, even two or three weeks after the operation, can begin to sit up, or, if the operation has been performed in two stages, five or six weeks after the first operation.

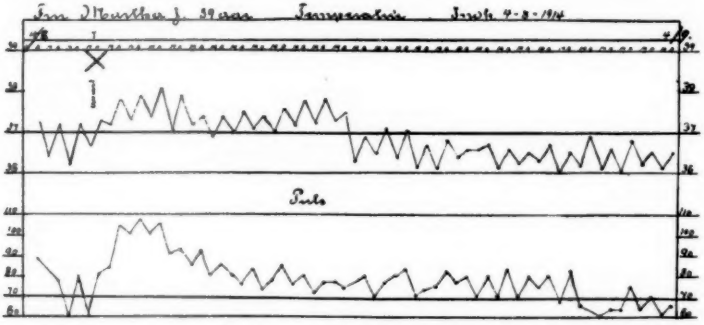


FIG. 6.—*Case* 2. Female, 39 years. Onset of the disease two years ago. Total left-sided pulmonary tuberculosis, cavity in the top. August 7, 1914: Resection c. XI to III. 150 cm. of ribs removed. Slight reaction after the operation. November, 1923: She has remained cured and able to do usual housework.

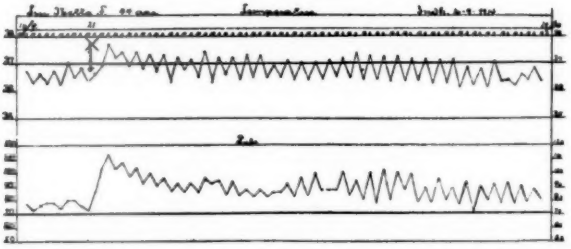


FIG. 7.—*Case* 4. Female, 44 years. Two sisters recently died from rapidly developing phthisis. Onset of the disease eight months ago. Total left-sided pulmonary tuberculosis with ramollissement. September 21, 1914: Resection c. X to II. 147 cm. of ribs removed. October, 1923: She has remained cured and able to do usual housework.

The amount of sputum diminishes often while the patient is still in hospital, sometimes even after the first stage, but especially after the second, and in a number of cases it diminishes in the course of a few weeks from 150 grm. in twenty-four hours to 30 or 25 grm. or even to zero. As a rule a rapid diminution in the amount of sputum is a good but not completely reliable prognostic sign as regards a lasting good result (figs. 8-10).

Special mention must be made of the *quantity of tubercle bacilli in the sputum after the operation.*

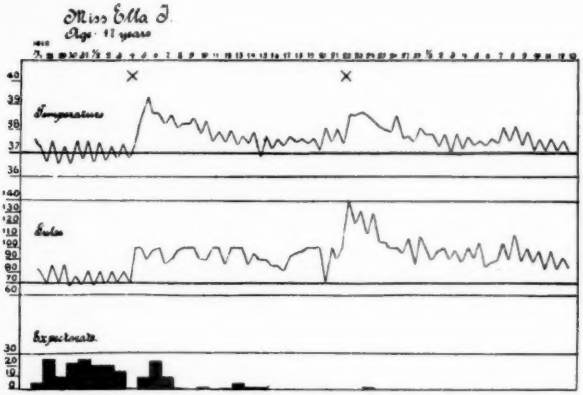


FIG. 8.—*Case* 34. Female, 17 years. Disease began one year ago. Total left-sided pulmonary tuberculosis with ramollissement. (i) February 4, 1919: Resection c. XI to V. (ii) February 23, 1919: Resection c. IV to I. The most striking effect from the operation was upon the expectoration, which completely ceased before leaving the hospital. October, 1923: She remained symptomless for more than four years, but has now for five months coughed with slight expectoration in which tubercle bacilli positive; has returned to a sanatorium. Some physical signs at *right* apex.

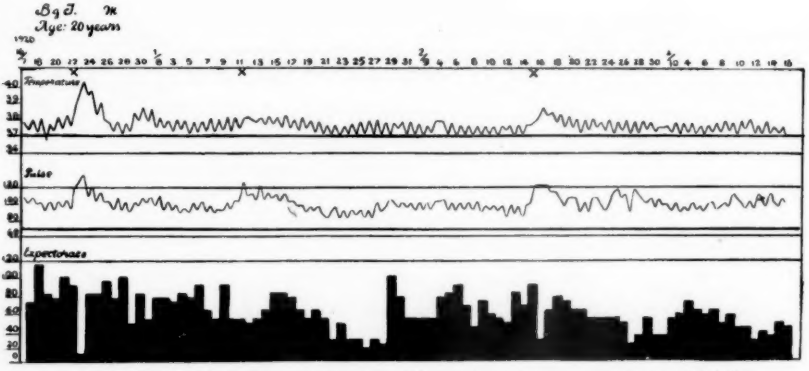


FIG. 9.—*Case* 50. Female, 20 years. Disease began two years ago. Total left-sided pulmonary tuberculosis. (i) July 22, 1920: Resection c. XI to V. (ii) August 11, 1920: Resection c. IV to I. Strong, but short reaction after first stage, slight reaction after second stage. No striking effect from the operation upon the amount of sputum. Patient died from tuberculosis, June 20, 1922.

Of ninety-two patients there were only three in whom it was impossible to demonstrate tubercle bacilli in the sputum before the operation. I nevertheless consider it certain that they were suffering from pulmonary tuberculosis, for the clinical progress of the disease was characteristic, and, in addition, one patient has since suffered from a tuberculous affection of the shoulder.

The operation has often a strikingly rapid influence upon the quantity of tubercle bacilli in the sputum. Even on departure from the hospital *no* bacilli could be found in the sputum of forty-seven patients, including three who were also without bacilli on their arrival, although in many of these cases there were swarms of bacilli in the sputum on entry. I do not consider that the said number of patients without bacilli—forty-seven to be quite correct—since the sputum was only examined once or twice, nor perhaps was this always done in quite a faultless manner. Yet since in twenty-seven of these patients no tubercle bacilli have since been found in the sputum from one to nine years

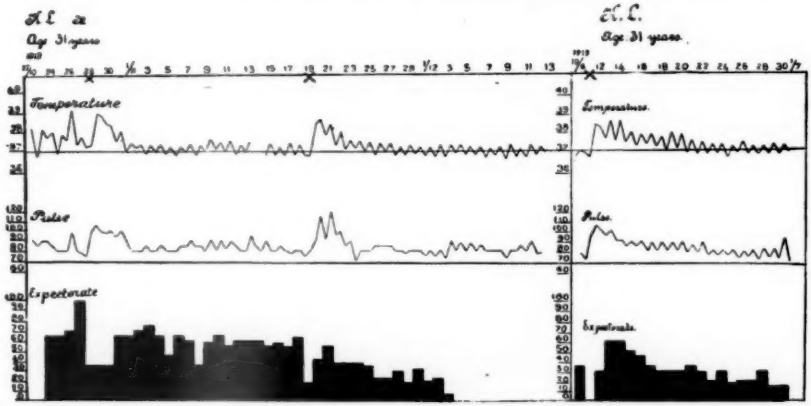


FIG. 10.—*Case* 33. Male, 31 years. Disease began nine to ten years ago. Total left-sided pulmonary tuberculosis, big cavity at apex. Several hæmoptyses, usually small ones. (i) October 28, 1918: Resection c. XI to V. (ii) November 19, 1918: Resection c. IV to I; total length of ribs removed 148 cm. After the second stage of thoracoplasty, the amount of sputum diminished rapidly to about zero. During the following spring the expectoration increased again to 50-60 c.c. daily. Therefore (iii) June 11, 1919: *Extrapleural transplantation of fat.* October, 1923: Has put on 40 lb. Works eight hours a day; he has still a slight cough with very few T.B.

after the operation, we may at least be allowed to conclude that *in a considerable number of patients* the bacilli disappear during their *stay in the surgical section, nor do they return later on.*

If we consider the quantity of tubercle bacilli in the thirty-five patients who must be regarded as healthy and able-bodied from one to nine years after the operation, we find:—

In 27 patients bacilli were absent on leaving the surgical section, and ever since.
In 5 patients bacilli were present on leaving the surgical section, but disappeared after a few days or weeks.
In 3 patients bacilli were present on leaving the surgical section, but disappeared from half to one and a half years afterwards.

We thus see that *it is undoubtedly a good prognostic sign if tubercle bacilli disappear already during stay in the surgical section, but we need not lose hope because this does not occur.*

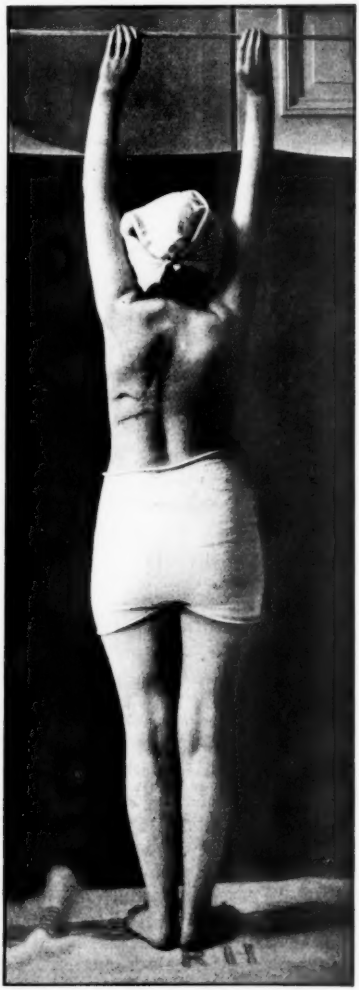


FIG. 11.—*Case* 11. Female, 19 years. Operated March-April, 1916, and February, 1917 (transplantation of fat). Died March, 1921, from pulmonary tuberculosis. The photograph shows the scoliosis with convexity towards the operated side, after second stage of thoracoplasty. The photograph was taken May, 1916.

The operated side of the thorax falls in considerably, so that the anterior margin of the scapula can be seen from the front. With the cystometer we can measure a diminution of the operated side amounting to 5 or 6 cm., or up to 10 cm. We can feel distinctly how the anterior ends of the resected ribs approach the posterior ones, so that the distance between them decreases from 12 or 15 cm. to 5 or 6 cm., 3 or 4 cm., or even less. At the same time the anterior ends move downwards, so that, for instance, the anterior end of the seventh rib rests opposite the posterior end of the eighth rib.

There develops scoliosis of the spinal column with the convexity towards the diseased side—the reverse of the post-empyemic scoliosis—presumably because at the operation we cut or paralyse a larger or smaller part of the long back muscles; the muscles on the healthy side therefore gain the supremacy,



FIG. 12.—*Case* 34. The photograph shows the limitation of the mobility of the shoulder a few weeks after the second stage of thoracoplasty.

so that the spinal column is bent convexly towards the operated side (fig. 11). The mobility of the arm is at first greatly restricted on account of pain, but by degrees it returns (figs. 12 and 13), except where the scapula has been entirely fixed. On the whole it may be said that the permanent inconveniences of the operation *per se* are slight.

AFTER-TREATMENT.

When the patient is discharged from hospital he is sent back to the sanatorium in order to undergo the necessary after-cure for at least three, or preferably six months. In favourable cases improvement continues there, evincing itself by a constantly diminishing amount of sputum, the fall of the temperature right down to normal (if it has not done so before), improved appetite, increase in weight, and better spirits. In order to judge whether the patient has derived any benefit from the operation, we must lay some stress upon

whether the amount of expectoration diminishes, whether the tubercle bacilli disappear, or we must see whether the temperature remains normal or increased, and last, but not least, we must pay regard to the patient's general condition. On the other hand, the physical conditions of the operated lung are so difficult to determine after the operation, that very frequently it is impossible to assign any appreciable importance to them. If before the operation there were



FIG. 13.—*Case* 34. The photograph shows the mobility of the shoulder three and a half years after the thoracoplasty. The mamma on the operated (left) side slips outwards.

marked symptoms of cavity formation, we may, in favourable cases, be able to find that the metallic and amphoric breathing are replaced by crepitations, with or without bronchial breathing. But all abnormal sounds do not disappear; weak amphoric breathing may be found even though it was lacking before, and is due to the large bronchi, which pass through a stiff, retracted pulmonary tissue. Even in the very best cases we never achieve normal physical conditions in the operated lung. The percussion sound continues to

be subdued, in some places very faintly tympanitic, at others quite dull. Abnormal respiratory sounds may be heard, because both lung and thoracic wall have lost their elasticity, so that it is difficult for the lung to get rid of expectoration. Therefore a slight cough and meagre expectoration without tubercle bacilli, in connexion with the above physical changes, cannot be taken to be bad signs, provided that the general condition, temperature, and pulse of the patient are satisfactory.

When those patients who have been cured again resume their work, it must be pointed out to them that they must never forget that they have passed through an illness which has left traces for the rest of their lives. They must never have hard physical work and must avoid exertion of every kind; *inter alia*, sport must only be indulged in moderately. I have not always succeeded in getting my admonitions obeyed, and in spite of my prohibition some of them constantly cycle or go ski-running in winter for hours at a time. A number of them bathe in the sea in summer, some have gone for walking tours in the mountains. One man is engaged as a farmer in a small way and has had constant hard physical work since 1918.

One man will not work because for years he has lived at the expense of his relations or of the public and has grown accustomed to do nothing, and he cannot see why he should work now only because his pulmonary disease has been cured.

Five women have married, two of whom it is true have already had a relapse and have since died. The three others have given birth to children without complications, one of them actually one year after thoracoplasty. Of the married women one gave birth to a healthy child, and in the case of another we produced abortion and then sterilized her as she had had two children already.

What is the Cause of the Rise in Temperature just after the Operation?

Before proceeding to report the results of my operations, I should like to make special mention of the *post-operative rise in temperature*, to which reference has already been made. When we have performed a number of thoracoplasties for pulmonary tuberculosis we are struck by the fact that a very high temperature, 39°-40° C. in the rectum, often occurs as early as the day following the operation, or even in the evening of the day of operation. The following are most readily imagined to be the causes of this:—

(1) Infection in the wound.
(2) Pyæmia from a lung cavity.
(3) Acute infection in the healthy or in the diseased lung.
(4) Spread of tuberculosis in the lungs or other organs.
(5) Parenchymatous degeneration of the liver or kidneys as the result of the narcrosis.

We know, however, from artificial pneumothorax treatment, that a rise in temperature often occurs in connexion with the collapse of the lung, and that many connect this rise with (6) *increased resorption of toxins* from the lung. It appears probable that something similar takes place after thoracoplasty, by which the collapsed lung, like a sponge, is partly pressed free from its contents either through the bronchi or—and this is of special interest in connexion with the questions raised here—also through the lymphatics and the blood-vessels.

In the first days after the operation it is often very difficult to decide which of the factors here mentioned plays the most important part in an eventual post-operative rise in temperature. The difficulty is increased because reliable

physical investigation of the lungs, especially of the operated lung, cannot be performed, and in addition it is difficult to estimate the value of the alterations shown in a lung which was not normal even before the operation. It may be that several of the above-mentioned factors act together in producing a rise in temperature. However, as a rule we see that, in spite of a high rise in temperature during several days, there occurs healing *per primam* without complications of any kind. In such circumstances I have been inclined to

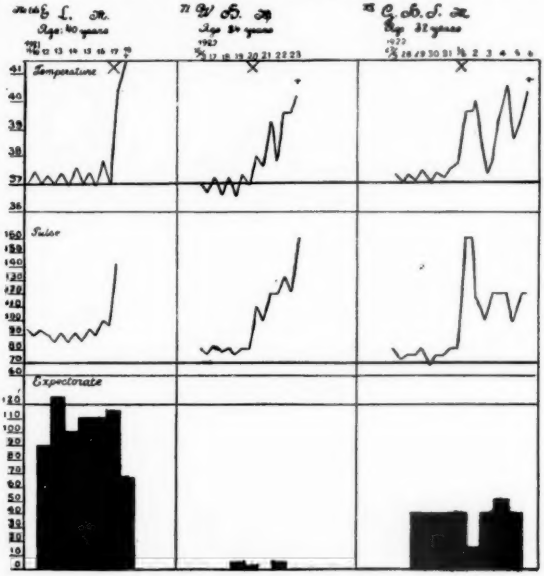


FIG. 14.—*Case* 66. Male, 40 years. Onset of the disease eight months ago. Total right-sided pulmonary tuberculosis, walnut-sized cavity in the top. Slight intestinal tuberculosis. October 17, 1921: Resection c. XI to I. 130·4 cm. Died after three hours (from auto-intoxication ?). Post-mortem examination: Submucous hæmorrhages in the stomach and intestine.

Case 71. Male, 24 years. Onset of the disease two years ago. Infiltration of the whole right superior lobe with cavity. Left apex suspect. March 20, 1922: Resection c. X to I. 123 cm. Died three days afterwards (from auto-intoxication ?). Post-mortem examination: Submucous hæmorrhages in the stomach, and subpleural hæmorrhages.

Case 75. Male, 37 years. Onset of the disease five months ago. Total right-sided pulmonary tuberculosis, walnut-sized cavity in the apex. June 1, 1922: Resection c. XI to I. 164 cm. Died five days afterwards. Post-mortem examination: Tuberculous pneumonia; intestinal tuberculous ulcers.

explain the rise in temperature as an essential result of increased resorption of toxins.

Exactly two years ago I was greatly alarmed by a dramatic death after thoracoplasty, the patient dying twenty-three hours after the operation, during a rise in temperature which only six hours after the operation reached 104° F., and later on rose to 106° F. (fig. 14, Case 66). The post-mortem examination revealed, in addition to tuberculosis, only submucous hæmorrhages

in the stomach and intestines. For several years previously I had a suspicion that some of my earlier deaths were due to a marked resorption of toxins, and I believed this view to be confirmed by the above-mentioned case of death. But even if resorption of toxins *per se* would not cause death, it must be feared that, nevertheless, it would place the patient's life in peril, because in a fatal way it would increase the importance of other complications. In short, the question of resorption of toxins and its importance had by degrees come to play so great a part in my experience that I considered it my duty to investigate the matter. I therefore referred to a specialist in serology, Dr. T. H. Thjötta, who has studied at the Rockefeller Institute. After having discussed the matter with him we agreed upon the following plan of work:—

(1) To ascertain whether resorption of toxins really takes place, and

(2) If possible, to find means whereby we should be able to select before the operation those cases which beforehand caused anxiety as to marked resorption of toxins after the operation.

(3) Finally, to find means of combating resorption of toxins, either by means which would be employed before the operation, or by means to be employed after the operation, as soon as resorption of toxins became evident.

The day before and after the operation every patient was subjected to Pirquet's test, with a standardized tuberculin in three different concentrations. An enumeration of the white blood corpuscles the day before and after the operation was also undertaken, and finally the patients had 20—50 c.c. of venous blood removed the day before and after the operation for serological examination.

I may say at once that Pirquet's test in twenty-two cases did not lead to any instructive result, these reactions sometimes being strongest before the operation, at others after the operation, without any constant agreement being found between this and the clinical course after the operation. In some very few cases the reaction was performed with the patient's own serum, but we obtained no positive result.

The white blood corpuscles were counted on forty occasions, before and after the various stages of the operation. A constant increase in the number of white corpuscles in the course of the first day after thoracoplasty occurs. The increase may be small, e.g., 9,000 to 10,200, but in other cases it was quite considerable, e.g., on one occasion from 7,200 to 31,000, and another time from 9,600 to 33,400. Hitherto there has been no difference in the course after the operation whether the increase in the number of white corpuscles has been large or small. On two occasions only was there found a decrease in the number of white corpuscles, once from 12,600 to 12,400, i.e., quite within the range of faulty counting, and in this case without clinical importance to the patient. On the other hand there may possibly be greater prognostic and diagnostic importance as regards the number of white corpuscles in the case of patient No. 71, in whom the number decreased from 12,400 before the operation to 7,600 nine-ten hours after it. The patient died three days later during a great rise in temperature and rapid pulse (fig. 14, Case 71). The post-mortem examination revealed the same features of submucous hæmorrhages in the stomach as in the case previously mentioned, also subpleural hæmorrhages; otherwise there were only the anticipated changes in the lungs.

As to the object and results of the serological investigations, my colleague, Dr. Thjötta's report is as follows:—

To find an explanation of the clinical reaction following some of the thoracoplastic operations one might follow two different ways:—

(1) One might try to demonstrate the presence of toxic products in the blood of the patients and compare the quantities present before and after the operation.

(2) Or one might try to demonstrate tuberculous antibodies present and compare them before and after the thoracoplasty.

The presence of *toxic products* was demonstrated in tests carried out on tuberculous and healthy guinea-pigs, partly consisting of large intraperitoneal injections of serum, partly of intradermal reactions with minute quantities of serum. The healthy pigs never gave any sign of reaction, while the sick pigs in several instances gave very typical reactions thus indicating that the reactions are specific for the tuberculous organism. In one instance where the patient died three days after the operation (fig. 14, Case 71), the serum killed the tuberculous pig both before and after the operation, although the serum after the operation gave the reaction in the pig that resembled the clinical picture in the patient most.

The intradermal reactions in pigs showed in four cases a very strong red and œdematous area of skin both before and after the operation, one case showed a weak reaction before and a very strong one after, and three cases showed no reactions before and very strong ones after the operations.

We have taken these tests as an evidence that the serum of tuberculous individuals is to a certain degree toxic to other tuberculous organisms, and that this toxicity during the operation in some cases is increased.

The demonstration of tuberculous antibodies was performed after the technique of the complement fixation with de-fatted tubercle bacilli as an antigen.

These tests have in ten instances shown a decreased or extinguished complement fixing property of the serum after the operation as compared to that before the same. In six cases the fixing property has been the same before and after, and in some few ones there has been no complement fixation either before or after.

The main feature of the experiments has however been a tendency to show a decreased amount of free tuberculous antibodies in the serum after the operation as compared to the same before this one.

We suggest the following explanation for the results of the serological reactions:—

When the lung tissue is compressed owing to the collapse of the lung, tuberculous antigen is resorbed from the tissue to the circulating blood, where it is fixed to the free tuberculous antibodies. The test after the operation will consequently show a decreased or a quite extinguished property of fixing complement in the test tube. This decrease in the amount of antibodies must naturally go hand in hand with the increase in toxicity of the serum. Thus the two different kinds of experiments must be said to point towards the same solution of the problem of the clinical reaction, namely a tuberculous auto-intoxication of the patient resulting from the sudden collapse of the lung and the resorption of greater or less amount of tuberculous toxic substances from the pathologic tissues of the lung. The reaction may consequently be regarded on the same premises as a large intravenous injection of tuberculin into tuberculous organism.

Although these investigations are not nearly finished, I have nevertheless thought it of interest to mention them here hoping that you will take part in these investigations and help us to find a satisfactory solution of the problem.

RESULTS.

TABLE I.—92 EXTRAPLEURAL THORACOPLASTIES, MAY 1, 1914, TO NOVEMBER 1, 1923.

	Males 46	Females 46	Left side Right side	Deaths from operation
Left side	26 ...	30 ...	56 ...	2, i.e., 3·5 per cent.
Right side	20 ...	16 ...	36 ...	7, i.e., 19·4 per cent.
	46 ...	46 ...	92 ...	9, i.e., 10 per cent.
Deaths from operation	5 ...	4 ...	— ...	9

Between May 1, 1914, and November 1, 1923, I performed ninety-two extrapleural thoracoplastic operations for pulmonary tuberculosis. Of those operated upon there was an equal number of men and women, namely forty-six of each sex (Table I). The operation was performed fifty-six times on the left side and thirty-six times on the right side. In men the operation was performed twenty-six times on the left side and twenty times on the right side; in women the corresponding figures are thirty and sixteen. Of these, nine (five men and four women) died from the operation; seven when the disease was in the right lung and two when in the left. Thus out of fifty-six left-sided pulmonary tuberculosis I have had two deaths from operation, i.e., 3·5 per cent., of thirty-six right-sided cases seven deaths, i.e., 19·4 per cent. The operation is thus more dangerous when right-sided.

TABLE II.—RESULTS OF 92 EXTRAPLEURAL THORACOPLASTIES.

YEAR	NUMBER			DEAD			STILL ALIVE		
	Total	Males	Females	From operation	From tuberculosis	From other causes	Symptom-less	Tuber-culous	Result not yet fixed
1914	5	1	4	—	3	—	2	—	—
1915	4	2	2	2	2	—	—	—	—
1916	4	1	3	1	3	—	—	—	—
1917	14	3	11	1	7	1	3	2	—
1918	6	4	2	—	1	—	4	1	—
1919	12	6	6	1	3	—	5	3	—
1920	10	5	5	—	3	—	3	4	—
1921	14	10	4	1	—	—	9	4	—
1922	13	8	5	3	—	—	9	1	—
1923	10	6	4	—	1	—	—	—	9
	92	46	46	9	23	1	35	15	9
Total number		92			33			59	

Those who died from the operation lived from one day up to twenty-three days after it, most often only eight or nine days. It has not always been easy to ascertain the cause of death even at the post-mortem examination. Two died of pneumonia (fig. 14, Case 75; and fig. 15, Case 8), one of acute endocarditis and pericarditis and one of mediastinal flutter (fig. 16). In five cases the cause of death was uncertain and the clinical symptoms were as follows:—

One or two days after the operation the temperature rose to 102·2° or 104°, the pulse-rate to 120 or 140 or more and remained so until death, after the lapse of four to eight days (fig. 14, Cases 66 and 71; and figs. 17 and 18). In one case a patient died the following morning (twenty-three hours after the operation) with a temperature of 106°. These patients had usually only slight dyspnœa, and the tongue remained moist. At the post-mortem examination there were found in some of them small mucous hæmorrhages in the stomach and intestines and flabby cardiac muscles, but otherwise nothing to explain the cause of death. This is regrettable, for it has happened several times that precisely in these fatal cases the operation passed off particularly well, the patient has not been visibly affected, and we considered ourselves justified in entertaining the highest expectations: then fate suddenly delivered one of those unexpected blows which to a special degree it seems to have reserved for us surgeons, and in the course of a night our attitude becomes changed from one of self-confidence, to one of humility and self-reproach. I believe that these deaths were due to increased resorption of toxins from the lung collapsed by the operation.

What is the fate of those patients who survive the operation? To judge of this it is necessary, in any case, to wait at least one year after the operation, and it is preferable, as in the case of cancer, to wait for three years. (Tables III and IV.)

After accepting your invitation to this meeting, I immediately began fresh after-researches into my whole material, and I am pleased to be able to tell you that I have obtained information about all patients without exception.

TABLE III.—RESULTS OF 82 EXTRAPLEURAL THORACOPLASTIES OBSERVED FROM 1—9 YEARS AFTER OPERATION.

DEAD 32, I.E., 39 PER CENT.				STILL ALIVE 50, I.E., 61 PER CENT.	
From operation	From tuberculosis	From other causes		Symptomless and able to work	Tuberculous
9, i.e., 11 per cent.	22, i.e., 26·8 per cent.	1, i.e., 1·2 per cent.		35, i.e., 42· per cent.	15, i.e., 18·3 per cent.

Of seventy-three surviving the operation, *thirty-five patients*, i.e., 48 per cent., are free from symptoms after observation for more than one year.

Fifteen (i.e., 18·3 per cent.) are now tuberculous, but I may safely venture to assert that the operation has been of benefit to nine of these, three of them having been without symptoms for from three to five and a half years after the operation, though they have now had a relapse. Six others, from two to six and a half years after the operation, are more or less able-bodied, in some cases quite so, with scanty expectoration, in which there are few tubercle bacilli, whilst the majority of them had cavernous phthisis before the operation.

Of the thirty-two deaths, nine were due to the operation, twenty-two to tuberculosis and one to influenza. Of those who died from tuberculosis, three were without symptoms for three to four years, but then died from recurrence in the "healthy lung" within the lapse of a short time. Thus these three cases undoubtedly derived benefit from the operation. I therefore consider myself entitled to say that in addition to the thirty-five patients who were without symptoms after the operation, twelve derived benefit from it. *All of these forty-seven, i.e., 57·3 per cent. or 64·4 per cent. of those surviving the operation, benefited by the operation. Whilst the remaining thirty-five, i.e., 42·7 per cent., including nine cases of death from the operation, would have been better or at least just as well without the operation.* I here quite disregard a brief transitory improvement.

Before the end of 1920 I had operated on fifty-five patients, who have thus been *under observation from three to nine years after the operation.* (Table IV.)

TABLE IV.—RESULTS OF 55 EXTRAPLEURAL THORACOPLASTIES OBSERVED FROM 3—9 YEARS AFTER OPERATION.

DEAD 28, I.E., 51 PER CENT.				STILL ALIVE 27, I.E., 49 PER CENT.	
From operation	From tuberculosis	From other causes		Symptomless and able to work	Tuberculous
5, i.e., 9 per cent.	22, i.e., 40 per cent.	1, i.e., 1·8 per cent.		17, i.e., 31 per cent.	10, i.e., 18·2 per cent.

Twenty-seven patients (i.e., 49 per cent.) are still alive, twenty-eight (51 per cent.) are dead. Seventeen patients (i.e., 31 per cent.) are alive, are without symptoms and are more or less able to work. If I only include those who survived the operation (50 patients) the percentage of cures rises to 34 per cent. with more than three years of observation. Ten patients are tuberculous, still living from three to six and a half years after the operation. Of the twenty-

eight deaths, five were due to operation, twenty-two (46 per cent.) to tuberculosis and one to influenza.

These results show that the danger to the patients resides in a new development of tuberculosis in the other lung, and further, they agree to a considerable extent with the results of our operations for cancer, resembling especially, as far as my own cases are concerned, the statistics of the operative results of cancer of the rectum. The immediate mortality resulting from the operation is 10 per cent., and the number of cures after three years' observation amounts to one-third. As the years pass, more and more of the patients die of the original disease: cancer patients of recurrence or metastases, phthisis patients of tuberculosis in the lungs or elsewhere. Just as unexpected cases of recurrence or metastases can be met with in cancer patients several years after

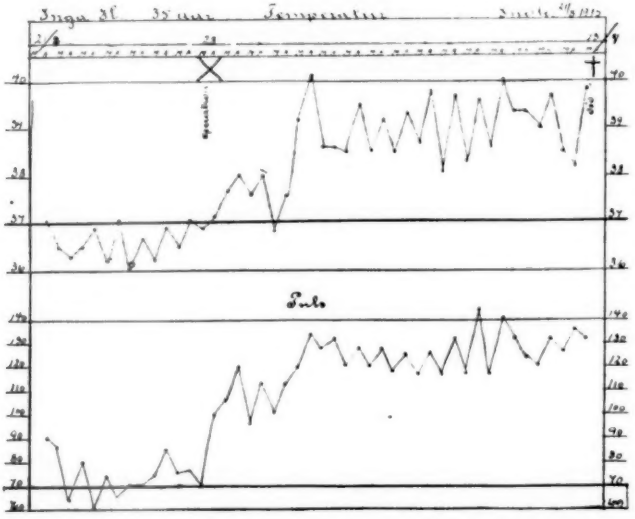


FIG. 15.—*Case* 8. Female, 35 years. Onset of the disease six years ago. Total left-sided pulmonary tuberculosis; cavity in the lower lobe the size of a hen's egg. August 28, 1915: Resection c. XI to II. 155 cm. of ribs removed. Died sixteen days afterwards. Post-mortem examination: Broncho-pneumonia of the *right* inferior lobe with empyema.

the operation, so after thoracoplasty, I have met with individual cases where patients can be for three to four years, or even longer, without cough or tubercle bacilli, until one day they die on account of a new development of tuberculosis in the other lung.

Discouraged by the results of our cancer operations, we make a point of impressing upon practitioners that the diagnosis must be made earlier so that patients may come up sooner for the operation. An improvement of the results of extrapleural thoracoplasty is, on the other hand, not mainly a question of diagnosis, but, as mentioned before, *a question of early correct prognosis.* Just as an early diagnosis cannot save all cancer patients, no more can an early correct prognosis save every case of unilateral pulmonary tuberculosis. As

long as we have no specific against cancer and tuberculosis there will always be, in spite of our best efforts, a number of patients for whom our science can do nothing. We must not give in however; the more practitioners who work along the same lines, the greater are the prospects of obtaining better results from all operations, including thoracoplasty.

Since I began to perform these operations in Norway more than nine years ago, twelve of my colleagues have followed suit, each of them having operated on from one to thirty-one patients and in all 135 patients with fifteen deaths, i.e., 11·1 per cent. (see Table V).

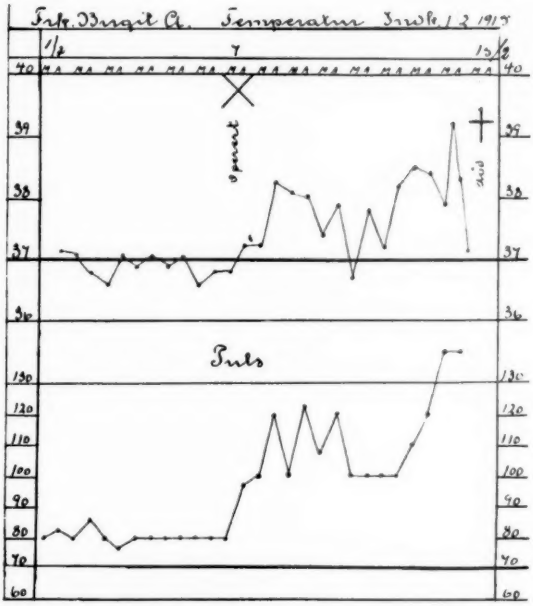


FIG. 16.—*Case* 10. Female, 28 years. Onset of the disease four years ago. Severe hæmoptysis last summer. Total right-sided pulmonary tuberculosis with ramollissement. February 7, 1916: Resection c. XI to III + extrapleural transplantation of fat. Died eight days afterwards from mediastinal flutter.

TABLE V.—227 EXTRAPLEURAL THORACOPLASTIES IN NORWAY.

Operated by	Number	Died from operation
12 Norwegian surgeons...	135 ...	15, i.e., 11·1 per cent.
P. Bull	92 ...	9, i.e., 10 per cent.
	227	24, i.e., 10·6 per cent.

I will now proceed to illustrate the effects of the operation by means of X-ray photographs (figs. 19-26).

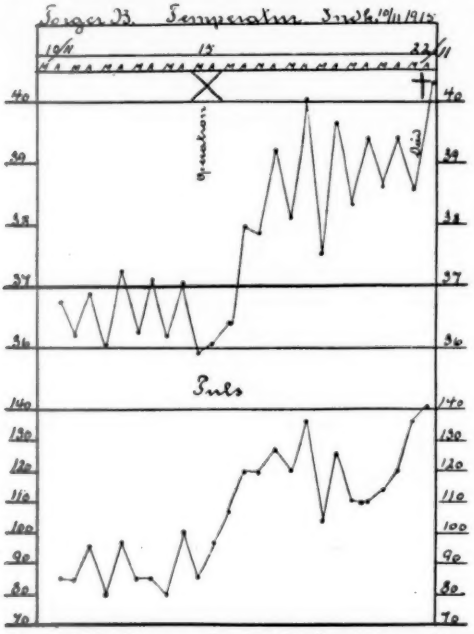


FIG. 17.—*Case* 9. Male, 38 years. Onset of the disease one and two-thirds years ago. Total right-sided pulmonary tuberculosis. November 15, 1915: Resection c. X to I, 99 cm. of ribs removed. Died seven days after the operation. No post-mortem examination. Cause of death uncertain, ? auto-intoxication.

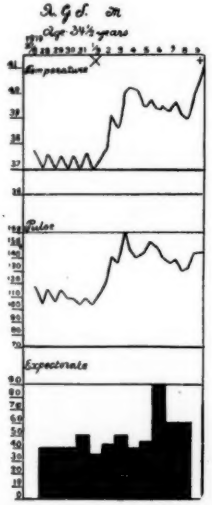


FIG. 18.—*Case* 38. Male, 32 years. Onset of the disease three years ago. Total right-sided pulmonary tuberculosis; cavity in the apex the size of a hen's egg. September 1, 1919: Resection c. XI to V, 104 cm. (first stage of thoracoplasty). Died nine days afterwards. Post-mortem examination: The wound without reaction. The spleen thrice the normal size, soft. Cause of death: Pyæmia ?

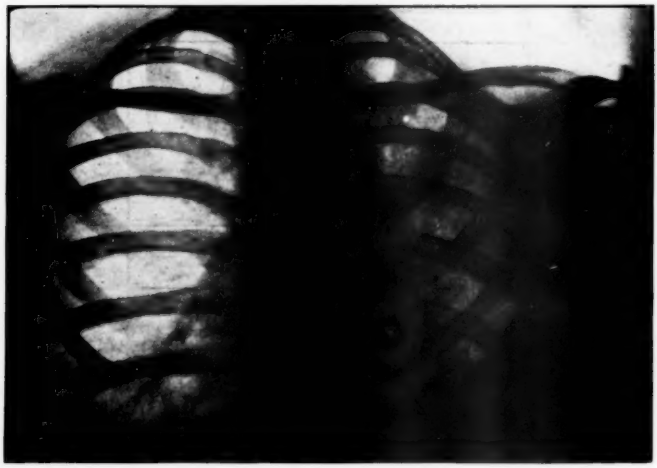


FIG. 19.—*Case* 34. Female, 17 years. Onset of the disease one year ago. Total left-sided pulmonary tuberculosis. Before operation.

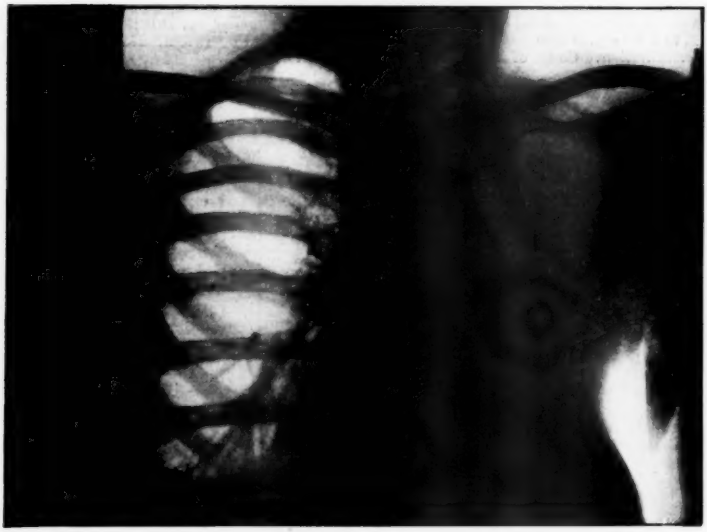


FIG. 20.—*Case* 34. X-ray photograph three years after thoracoplasty. Ideal collapse of the thoracic wall and the left lung. One year later, i.e., four years after thoracoplasty, relapse in the right apex.



FIG. 21.—*Case* 33. Male, 31 years. Onset of the disease nine and three-quarter years before. Total left-sided pulmonary tuberculosis, big cavity in the apex. Before thoracoplasty.

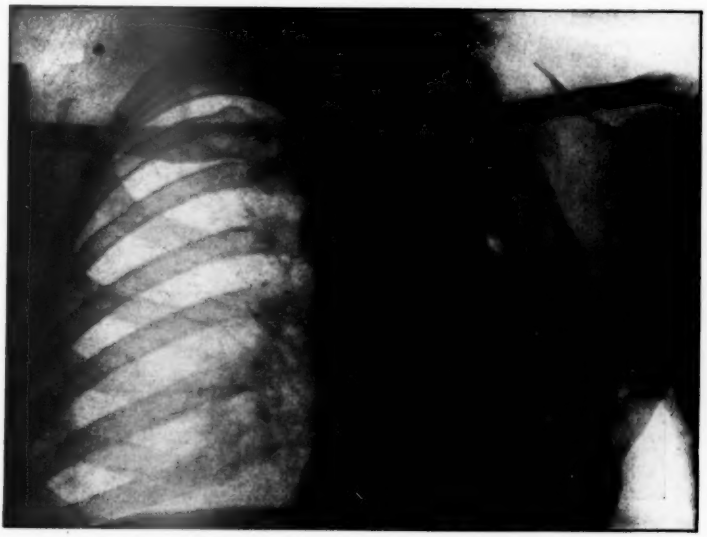


FIG. 22.—*Case* 33. Two months after second stage of thoracoplasty. The big cavity not completely collapsed.

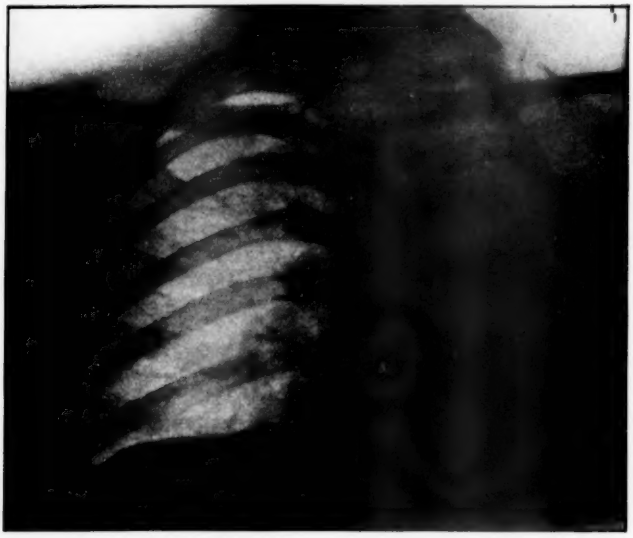


FIG. 23.—*Case* 33. Extrapleural transplantation of fat six months after second stage of thoracoplasty. X-ray photograph two years after thoracoplasty; cavity no longer visible.



FIG. 24.—*Case* 70. Female, 23 years. Onset of the disease three years ago. Total left-sided pulmonary tuberculosis, with a cavity in the apex the size of a hen's egg. (i) December 14, 1921: Resection c. XI to V. (ii) January 19, 1922: Resection c. IV to I, 135 cm. October, 1923: Cured and able to do usual housework. Before thoracoplasty, cavity in the left apex.

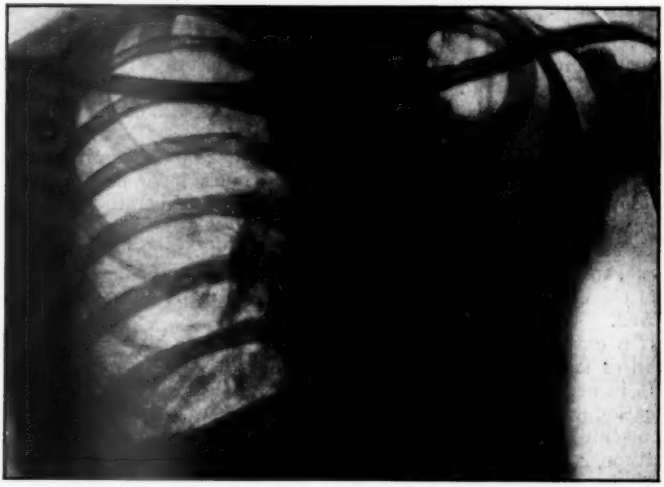


FIG. 25.—*Case* 70. After first stage of thoracoplasty. The cavity in the apex more distinct than before operation.

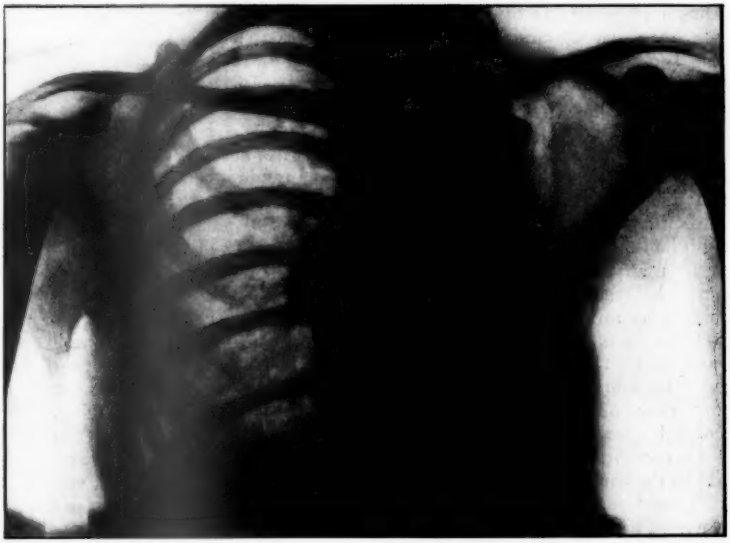


FIG. 26.—*Case* 70. X-ray photograph after second stage of thoracoplasty; the cavity in the apex nearly completely collapsed.

SUMMARY.

The concluding points which I wish to make are as follows: (1) In unilateral or mainly unilateral pulmonary tuberculosis which is not cured by rational expectant treatment or pneumothorax good results can be achieved by means of extrapleural thoracoplasty.

(2) The operation should only be performed after an exhaustive conference with the physician treating the patient, who must have been enabled, by observation during a considerable period, to form a deliberate opinion of the prognosis with continued expectant treatment.

(3) Resection of the ribs should be carried out, under local or general anæsthesia, through a paravertebral incision so that the posterior part of the ribs from the eleventh, or in any case from the tenth to the first inclusive, can be removed.

(4) If a cavity remains, it can be made to collapse by extrapleural transplantation of fat.

(5) It is necessary that medical practitioners should acquire knowledge of the indication and results of extrapleural thoracoplasty. As one can rely upon saving one-third of the patients who, without the operation, would be doomed, no practitioner has now the right to withhold the chance of operation from suitable patients.

In conclusion, I must admit that these operations of extrapleural thoracoplasty in common with so many other great ones often bring sleepless nights both for patient and surgeon, and disappointment to both, immediate and subsequent. Every death from the operation, and all cases without appreciable improvement after the operation, are like an accusation against the doctors in charge, also against the surgeon for lack of knowledge of pulmonary tuberculosis and its complications, or for lack of critical judgment when considering the indications, or for deficient technical skill. It is hard to have to admit this, but it is necessary to be aware of this failure to cure, if we are to hope to be able to proceed further and improve the results. I therefore look forward to the impulses towards improvements which I am sure will come from British colleagues when extrapleural thoracoplasty has made its entry into this country. But if disappointments are many, there are as compensations the gratitude of patients in cases with a fortunate course, and the surgeon's joy in the craft that he has made his life's work, a joy that is perhaps greater in thoracoplasty than in many other operations. And rightly so, if we consider that these patients before they go to the surgeon have usually wandered from one physician to another, and from one sanatorium to another, where it may be they have seen one after another of their fellow sufferers succumb to the disease, until they themselves are on the point of losing all hope of being cured. In addition, there is the crushing feeling that even their nearest and dearest more or less openly shun them for fear of infection. Imagine, after a successful operation, taken as a last resource, the joy of the patient in finding that his cough rapidly diminishes and his strength increases, and then to hear from the doctor that the bacilli have disappeared from the sputum, so that the patient, without being feared by others, can live with those he loves best and be a useful member of society. Is not this a result that is worthy of the best traditions of surgery?

Dr. CLIVE RIVIERE.

Pulmonary tuberculosis being what is conveniently styled a "medical disease," its control up to and following such surgical measures as are invited must remain for the most part in the physician's hands. On him must fall the responsible decision that surgical intervention is needed, and the artificial pneumothorax, or attempt at this, which practically always precedes other surgery is also his province. Not yet, in this country, has there arrived the surgeon-physician to undertake a major operation when his pneumothorax has failed, though he is to be seen in other lands. Here the operating surgeon has still his part to play.

In practice the physician's responsibilities in any individual case may be conveniently divided into two periods—first, those connected with pneumothorax therapy, secondly, those concerned with further surgical measures when pneumothorax fails. Since the aim of modern surgery, as applied to the treatment of pulmonary tuberculosis, is practically always the collapse of diseased lung, we may conveniently confine our attention in the present discussion to measures directed to that end.

Pneumothorax Therapy.—To begin, then, with pneumothorax therapy, I should like to make certain remarks about its indications, and the class of case for which it and other surgical measures are especially suitable.

Now from general impressions one might expect that most cases qualified for pneumothorax treatment are being carefully watched in sanatoria or similar institutions till they are ripe for this operation; that disease is seen to be steadily spreading down one lung in spite of rest, open air, and other measures, and that at a certain moment the physician brings out his pneumothorax apparatus and arrests the "descensus Averni." In practice this happens but rarely; in my experience as a rule the case suitable for pneumothorax comes to the physician with disease already crying out for this treatment. The reason for this is, that it is, for the most part, a particular type of disease which is suitable for collapse therapy, and only a much smaller proportion of cases of ordinary apical phthisis really qualify for it.

The suitable case but too frequently reaches a definite diagnosis only when moist sounds already cover a good part of one lung, disease having been overlooked at earlier stages or its gravity unrecognized, because it was deep and out of reach of the stethoscope. In the common type of deep-seated or hilum tuberculosis the disease is double, but in the fortunate cases which are suitable for pneumothorax treatment disease has flourished throughout the one lung, but for some curious reason has failed to establish itself firmly in the other. The X-rays show an "interstitial" spread on both sides, but the parenchymatous outbreak, which tends to follow this, is in these cases confined to a single side. The majority of such cases, as also of ordinary hilum tuberculosis, are found in adolescence.

Now it is common to see patients with advanced one-sided disease clinically, but on both sides by X-rays, and a radiogram must, therefore, always be used in deciding the suitability for pneumothorax. The better lung is seldom or never quite free, but the disease remains peritubular; there is generally a thickening of strands and network, and if these are not very lumpy or of woolly outline, and the lung field is clear, or contains but few and sharp-edged shadows, the lung will probably function safely while the other is collapsed. Especially can we rely on the response of the better lung if we remove auto-

inoculation and fever by the pneumothorax operation, but it needs much experience to decide on the lung appearances which are safe, and those which must be rejected, the question of active disease being the main test. To the stethoscope the better lung will probably be quite clear, and if at a later stage, in these cases, disease spreads and becomes active in this lung you will nearly always find the first clinical signs, not at or near the apex of the lung, but about the mammary region or in the axilla. I have said this much about the type of case especially suitable for pneumothorax or other surgical treatment because I consider its recognition an important point, and I do not feel that its peculiar characters are sufficiently recognized.

Now it is very striking how, in the class of case described, disease may have spread through the whole lung, and surface signs may have been present a long time, and yet the lung remain unadherent to the chest wall, and complete pneumothorax collapse be obtainable. Especially, it must never be presumed that a lung is adherent, and therefore a pneumothorax impossible, without trial being made. Mere negative pressure may draw heart and mediastinum into one side of the chest, and deform the diaphragm, without any pleural adhesions being present. Displacements are not evidence of adherent pleura, hence artificial pneumothorax must always be put to the test before other surgical measures are invited.

I shall not discuss ordinary artificial pneumothorax, as I think it is hardly included in our subject, and its utility in suitable cases is no longer open to controversy. The cases which must now concern us are those in which pneumothorax fails, cases with a completely adherent pleura, and cases in which only a partial and ineffective pneumothorax can be obtained. In all such the question of further surgical measures will arise, and the indications and contra-indications will need to be reviewed afresh. In the case of a partial pneumothorax, a patient trial will often be needed before its worth can be fully gauged. If its effect on symptoms is unsatisfactory, if large cavities are left uncollapsed, and if, in spite of measures to prevent it, large febrile reactions follow the refills, some other or additional methods of lung collapse will need to be used. The cauterization of adhesions may help in some such cases, but more commonly it becomes a matter of some larger operation, such as thoracoplasty. This may be done in conjunction with a pneumothorax, or the latter may be abandoned. I have not myself had experience of a case of combined pneumothorax and thoracoplasty, but it should prove a rational method in certain cases. It gets rid to some extent of the permanency which is perhaps the most embarrassing feature of thoracoplasty and similar operations. Should the other lung prove recalcitrant there will still remain some chance of redressing the balance by re-expansion of collapsed lung tissue: for we find in pneumothorax treatment that a temporary partial re-expansion will at times avert a threatened breakdown of the better lung, and after an interval pneumothorax may again be resumed. After thoracoplasty alone there is no such turning back, and it is partly this inevitableness and permanency which restricts the indications for this class of operation.

Indications and Contra-indications for Operative Measures.—We have considered the "better lung" under the indications for pneumothorax—if this operation fails, and a thoracoplasty is contemplated, the lung must be re-examined with yet more meticulous care. For we have now new factors to take into account—the inevitability of this further move, the strain of operation, and the more violent mechanical and toxic disturbance brought about, the lung collapse being rapidly produced instead of slowly as in pneumothorax

treatment. If the better lung contains any active foci of disease it is not likely that these will remain quiet under such adverse conditions, and we must not subject our patients to the distress of operation unless there is reasonable hope of recovery. Nevertheless there is another side to this matter, since, in the cases for which surgery is indicated, it must often be a choice between an operation of doubtful outcome and, on the other hand, certain death. The patient's interest must be placed before the physician's reputation, but these operations should not be dragged into undeserved discredit—there is here, as in all things, a happy mean at which to aim.

Of other indications and contra-indications I will only remark that good nutrition is a most important asset, and cachexia a contra-indication—that the heart will have to meet considerable strain and must be fairly sound, often a matter of anxiety in the subject of prolonged fever—that intestinal tuberculosis is an absolute contra-indication, but that a moderate laryngeal lesion will not prevent operation being carried out.

Operations other than Thoracoplasty.—As the alternative to pneumothorax I have spoken hitherto of thoracoplasty, and I have done so because I think this is usually, in these days, the operation of choice. Having made trial, however, of certain other methods, I think it may be well to mention them here, and record my impressions of their merits and demerits.

Separation of Adhesions.—In one case of thick multiple adhesions I got Mr. Romanis to separate these by opening the pleura through an intercostal space. This is a fairly severe operation, but in a vigorous patient with the other lung good it might be done with success. It has the great advantage of afterwards conferring all the benefits of a complete pneumothorax, as compared with the more doubtful benefit of a thoracoplasty. Single or few adhesions can of course be divided without opening the pleural cavity, though I find this is but rarely needed.

Pneumolysis.—The parietal pleura is easily stripped from the endothoracic fascia, and thereby the lung is collapsed, but a space left between lung and chest wall. This may be filled with gas or air, or with solid substances such as fat or paraffin. Under both conditions the danger of sepsis is considerable, and with paraffin there is great probability of its extrusion after a longer or shorter interval. The operation is, however, much less severe than thoracoplasty, and in one of my cases a paraffin replacement enabled the patient to reach a condition of health where thoracoplasty could be done with little disturbance. The pleura was stripped and paraffin put in through the third interspace in front, and after an interval the tuberculous symptoms abated, fever disappeared, sputum was reduced, and 8 lb. were added to the patient's weight. Unfortunately the cavity suppurated, though mildly, and the paraffin began to extrude. Before its loss could lead to renewed lung symptoms, a thoracoplasty was done with complete success; the remains of the paraffin were removed later, and the small sinus remaining healed rapidly. I think there are cases of active disease in which a pneumolysis may be advisable owing to the slighter severity of this operation, and with the mental reservation that, if paraffin is eventually returned, the patient's condition may be so far benefited by the operation that other methods may then be undertaken with success. If only a suitable packing medium could be discovered, pneumolysis might take a high place in the thoracic surgery of the future. It would then, however, still suffer from the disadvantage that the collapse it produces is not accompanied by immobilization as happens in the case of thoracoplasty.

Thoracoplasty.—Having returned to thoracoplasty, it is well that we should

consider what are the disadvantages of this operation as compared with artificial pneumothorax.

(1) There is, as already mentioned, the strain on recovery due to a rather severe operation, accompanied by rapid collapse of a diseased lung, and with severe auto-inoculation as a result, together with the serious risk of aspiration of septic and tuberculous sputum. Among Sauerbruch's large material 2 per cent. died of the operation (but most of these at a time when the severe Brauer-Friedrich operation was done), and 12 per cent. during the first three weeks from aspiration pneumonia and tuberculosis of the better lung.

(2) As already remarked, the collapse is permanent, and, on this account, we are left helpless in face of an exacerbation of disease in the better lung.

(3) The amount of collapse attainable by thoracoplasty is far less than that obtained by an average pneumothorax, and may not be sufficient to arrest disease. Brauer put it, at highest, at three-quarters of the collapse obtained by pneumothorax, and Dumarest has rightly remarked that the apex of the lung, often the most vital spot, is always less collapsed than that. It is true that an insufficient collapse can be partly remedied by further removal of ribs, as, for instance, of parts of the first five ribs anteriorly as Sauerbruch has recommended; also a basal collapse can be added by section of the phrenic nerve in the neck, as has been done with advantage in cases under my care. It might seem at first sight as if the partial lung collapse, but atmospheric pressure at best, would be bound to fail in many of the cases for which it is applied. Its success shows that immobilization and the release of tension are all that the lung demands in many cases to enable it to fibrose and recover. In connexion with this it is of interest that many experienced operators are of opinion that evidences of fibrosis must be observable in the diseased lung before operation if a thoracoplasty is to succeed; in other words, that thoracoplasty is suitable for fibro-caseous disease, but not for caseous or acute tuberculous lesions. It is difficult to say how far this doctrine should influence us in practice—one of my patients, so far doing pretty well, was a case of active though not acute progressive disease without evidence of fibrous change, but the outcome is still uncertain. Undoubtedly the lung best suited for thoracoplasty is that with chronic fibro-caseous disease and considerable cavitation.

It is not my province to discuss the question of operative procedures unless to remark that the term thoracoplasty in reality covers many different operations, some of which, such as the old Brauer-Friedrich procedure and Wilms' operation, are, I believe, little used at the present time. The paravertebral resection initiated by Sauerbruch and used by Saugman seems to be the most satisfactory operation known to us, since it provides a very efficient collapse without the dangerous "open pneumothorax" effects experienced with Friedrich's proceeding. After it the hæmothorax is greatly narrowed, but nevertheless there is practically no deformity when the patient is dressed owing to the integrity of the shoulder-girdle. Upon examination one discovers enfeebled breath sounds over the side, often of a blowing character, and this even up to simulation of cavitation below the clavicle, where the air entry generally remains greatest. The apex, even where the first rib has been thoroughly dealt with, and the base, are the parts where collapse appears least satisfactory. For the former, if necessary, an apicolysis can be later devised, or further resections of the upper ribs in front, as done by Sauerbruch; for the latter a phrenectomy may be required. The heart falls into the collapsed side, and is often palpable in the axilla and even behind, and one of the advantages of this, and indeed of the operation as a whole, is the full freedom of the other lung from such encroachments as pneumothorax generally brings about.

It would be expected that considerable dyspnœa and cyanosis might follow the operation, but this has not occurred in the cases I have seen done, though it is described as usual in the literature. The main discomfort complained of is inability to cough, and pain when this is attempted. Fever generally follows the operation for a variable period up to about a week and expectoration, when it begins, is temporarily increased, to diminish again later.

A satisfactory pneumothorax case, once the initial stages are past, does best in many cases in the patient's own home if conditions are good, and later, working at his ordinary occupation if this is not too strenuous. In thoracoplasty, on the other hand, the lung collapse is much less complete, and convalescence and after-treatment must be long persisted in, either in a sanatorium or on sanatorium lines. The question of ultimate recovery must depend very much on the behaviour of the patient during the year following the operation. Our experience of thoracoplasty has so far been somewhat limited in this country, but it will, I feel sure, steadily increase as we appreciate more and more the precise conditions under which it is indicated and the results which it can give.

Mr. H. MORRISTON DAVIES (Vale of Clwyd Sanatorium, Ruthin) said that the question of thoracoplasty had been much obscured in this country, as Professor Bull had rightly pointed out, and it was a great advantage to receive enlightenment about it from men like Professor Gravesen and Professor Bull. The question of the surgery of the lung had interested him (Mr. Morriston Davies) for thirteen years, but his experience in that domain had been quite small in comparison with that of Professor Bull. He would illustrate what he had to say by means of lantern slides. The first two showed the state of disease in cases which were eminently suitable for treatment by one or other of the means available in surgery.

The first slide enabled one to realize, from the pathological and mechanical standpoints, how impossible it was for such a case to respond, more than temporarily, to any other line of treatment than that of some surgical procedure; the changes that had taken place, not only fibrosis, but dilatation of bronchi and bronchioles, with retention of the secretions, could only respond to some treatment which would produce collapse of that lung and obliteration of the dilated spaces.

In the next, not only was there a change involving the lower part of the lung, but there was a large cavity in the apex, and no real benefit could ensue except from bringing about collapse. The method that should always be tried first for producing collapse was artificial pneumothorax. He would not deal with that; but, as Gravesen had pointed out, in many cases pneumothorax had not been successful because of adhesions. The statistics that authority on the subject gave at Plymouth showed that the results in complete pneumothorax were wonderful, for over 70 per cent. were able to return to work. That proportion, however, diminished the greater the number of adhesions present. With a complete pneumothorax, but with localized adhesions, 33·3 per cent. were able to work, but when the pneumothorax was incomplete owing to adhesions, 11·1 per cent. only returned to work.

The next slide showed a type of adhesion which was very typical, and it would be seen how dangerous it would be to proceed beyond a certain point with pneumothorax, as this band was exactly over a cavity in the lung. He had seen cases of this kind in which pneumothorax had been proceeded with, and the result had been, not rupture of the adhesion but rupture of the lung, and

pyo-pneumothorax resulted. Only the finest filaments would rupture during the production of a pneumothorax. In all cases in which such a course was attempted, one should be certain to have the case all the time under X-ray control. The small adhesion in the case just depicted contained part of the lung, and there was no diseased tissue in it. Therefore it was justifiable in these conditions to maintain a constant high intrapleural pressure in the attempt to stretch the adhesion.

The next slide showed how this stretching had been done. Only the finer ones could be stretched, and those in which the lung was sound at the point of attachment of the adhesion. The next showed an adhesion over a weak part of the lung; to attempt to stretch that would be very dangerous.

There were three methods of dividing adhesions. First, there was the method of Jacobaeus, which had been successfully used for certain types of adhesions; in this, a thoracoscope was introduced for viewing, then a cautery was passed in through another tube and the adhesion divided. That was the one operation on the lung which he had not so far done. He used one of two methods: where adhesions were like those now shown he used a tenotome he had devised; this was inserted through the chest wall, previously anæthetized, the movements being viewed and controlled by means of the X-rays. It was necessary to have the adhesions taut by the pressure of the intrapleural gas so as to resist the knife. The tenotome could be seen coming down on the adhesion and dividing it; also the operator could feel it doing so. In Jacobaeus's method the bleeding was controlled to some extent by the cautery; with the tenotome one might get some bleeding, but it soon stopped because the intrapulmonary vascular pressure was low and there was considerable intrapleural pressure due to the gas, and this pressure was increased by a small hæmothorax. The ensuing emphysema might cause some inconvenience owing to the tension under the skin, but it had not caused worry in any of his cases.

The next slide showed the condition immediately after the division of the adhesion, and the following one was of the condition a week later. Here it was possible to produce complete collapse of the lung.

The following slide illustrated the case of a patient who came with pneumonic tubercle and extensive ulceration of the larynx. He (Mr. Morriston Davies) attempted pneumothorax, and got to the stage depicted, but he was unable to get the lung collapsed further, because of the adhesions. After he had divided the adhesions, however, the lung collapsed, and later he was able to get the lung completely down. After eight months the temperature had come down to normal, and now three years later, the patient was quite well, and there was no ulceration of his larynx, though the voice was still defective, owing to the loss of tissue there.

The next case showed multiple adhesions, extending on the right side to the apex. After the main adhesions had been divided one appeared as a loose band, and as he could not get sufficient tension on it, he left it. Still it was seen that the lung collapsed almost completely. In that case only was there hæmorrhage afterwards. A few days later he (the speaker) aspirated it and replaced it.

There were cases in which the adhesions were too extensive or too broad to justify the use of the tenotome or the cautery, and then he found it preferable to open the chest by making an incision over a rib adjacent to the main adhesion, to expose the adhesions to view, and to ligature them. In one case in which he had recently operated there were multiple adhesions, some as large as two fingers, and extending in various directions. He transfixed them

with sharp hernia needles and ligatured them, and there was no subsequent trouble. The patient had had a partial pneumothorax done some time before, but her cough had never subsided, because the lung was kept out by these adhesions. Since the procedure he had described she had not coughed at all, and her symptoms had subsided. In that type of case also there was emphysema, but it was a comparatively small matter.

The next slide showed a case in which the adhesions were too extensive for division by the tenotome, and in that patient he was never able to get a reduction of the symptoms; he could not persuade the patient to have the adhesions divided, and he had gone downhill. Where extensive adhesions were interfering with the pneumothorax, some other method must be used, and thoracoplasty was the one line of treatment to which resort must be made. An existing partial pneumothorax tended to diminish the seriousness of the reaction following the operation.

With regard to section of the phrenic nerve, he had done that in various types of cases, and it was very useful in certain pneumothorax cases in which there were adhesions to the diaphragm from the base of the lung. Sometimes these patients had a constant dry irritating cough, which greatly disturbed their rest; on that side division of the phrenic nerve paralysed the diaphragm and so abolished the cough. He had done it on the left side in cases in which the cough was constantly followed by vomiting, and the resulting paralysis had stopped the vomiting as well.

The next slides showed a case before and after division of the phrenic nerve; it showed how the diaphragm was raised up when paralysed; during quiet respiration the diaphragm did not move, but on deep inspiration it rose instead of falling.

With regard to thoracoplasty, one wanted to do as little of this as possible, and in cases in which the disease was localized to the upper lobe he had not carried out removal of ribs, but had limited himself to apicolysis, not using gas, as he thought it unsatisfactory, but using either paraffin wax or fat to fill the extra-pleural space after separation of the lung and parietal pleura. The slide now shown was of a case suitable for apicolysis, and another showed a case in which there was a big cavity in the upper lobe partially filled with secretions. In that case he replaced lung by paraffin, and the next slide showed the paraffin *in situ*. The objection to paraffin was that it might be extruded later, and that happened in this case. It did its work however, as it abolished the cavity permanently. But if paraffin became extruded, there was the risk of a secondary infection, and rib resection might have to be done to abolish the sinus. He had found fat more satisfactory; but fat might liquefy and there might be expulsion of the liquefied part of the fat. In those cases even, the lung would remain collapsed, and the benefit obtained immediately by the replacement by fat would continue and would increase.

In his last case, a fortnight ago, he did a different type of operation. The patient—a girl—was plump, and he removed a part of the second rib and costal cartilage, and stripped away the lung and parietal pleura; then he carried the incision round the outside of the breast up to the mid-line and dissected out a large layer of fat and half her breast, leaving them attached to the pectoral fascia along the line of the first incision. He turned the graft upwards, and slipped it into the cavity he had made, and sewed the pectoral muscle over it. So far, the result of this living graft had been most satisfactory, and her symptoms had almost completely disappeared.

He did not consider apicolysis was suitable for cases in which the disease

was more extensive, unless it were used in addition to resection of ribs. Thoracoplasty should not be done except when pneumothorax had failed, but the results were very satisfactory. There was, of course, a certain mortality from it, therefore the cases had to be carefully chosen. In the last ten cases on which he had done it—for tuberculosis and bronchiectasis—he had had one death: that was in a case of putrid bronchiectasis, which he regarded as almost hopeless, and he did the operation as a desperate measure. The other cases did remarkably well, and the more he saw of the results of thoracoplasty, the more was he satisfied with the operation.

With regard to the anæsthetic, when he started to do the operation in 1912, he did it under local anæsthesia, but he now thought that to do the complete operation under local anæsthesia was asking too much of the patient. He now used local in addition to general anæsthesia, the general anæsthesia being only sufficiently deep to keep the patient asleep. He did a local and regional anæsthesia with 2 per cent. novocaine, and then, as nearly as possible—and facility came with practice—he hit off each intercostal nerve, and injected 5 minims of absolute alcohol into the nerve. This greatly reduced the shock of the operation, and abolished the after-pain, which had been in former cases a distressing feature. If only novocaine was used, the patient had much pain next day. The general anæsthetic he used was chloroform, and only a little of this was needed. The shock was not as great as usually seen after a major operation. If pain occurred, or there was difficulty in regard to coughing, he gave a hypodermic injection of omnopon, and encouraged the patient to cough so as to clear the bronchi. In one of his last cases, in which the operation had been done eleven weeks ago, the patient had not been free from cough for two years, and since the operation she had not coughed at all. The keenness of her appreciation of that alone had been well worth the operation.

In former days he did Wilms' operation, and the results were satisfactory, but it was a severe operation. He now used the para-vertebral incision; it was the most satisfactory, and gave adequate access to the posterior parts of the ribs, which were the most essential parts to remove.

There were two important points in regard to rib removal. One was, that the ribs must be removed right up to the transverse process. The second was that the first and second ribs were the most difficult but the most essential to resect: at least ten ribs must be removed. He was aware that most colleagues abroad did the operation in two stages, but he himself usually did it in one stage. In his last ten cases he did one only in two stages, and that one was the case of the patient with putrid bronchiectasis who died after the first stage. If the precautions as to preparation of the patient, &c., he had mentioned were carried out he thought it was justifiable to do the operation in one stage, unless the patient were very weak.

The skiagram now shown gave the result after a Wilms type of resection; the collapse was not so great as after pneumothorax. The next showed a case before operation. This patient had been in bed three years with considerable pyrexia, and after he had operated upon her she was able to get about, not only walking, but bicycling, and she had remained free from symptoms three years. She was then attacked with a recurrence in the other lung, after influenza, and died.

The next slide showed a child on whom he did the paravertebral incision, and in the next, the slight deformity resulting was apparent. There was no interference with the shoulder girdle. Sometimes a little padding was wanted. A posterior view showed that there was no interference with the movements of of the arm.

He showed ribs taken from one of the patients; in that case, a woman, 126 cm. of ribs were removed, a gradation increasing from the first to the seventh, then diminishing again. He had devised a special periosteal elevator, which fitted over the rib, and could be simply pushed along the rib, clearing the periosteum from it as it went. He then divided the posterior part of the rib, slipped the ring of his bone forceps over it and it followed the rib as far as the periosteum had been resected. By the use of those two instruments he had shortened the time of the operation considerably, and this had further reduced shock.

He now showed a slide of the same patient to whom he had referred, with ribs removed, and the collapse of the lower part could be seen to be complete; only in the upper part was a little lung area visible. Should there be a cavity which was not completely collapsed, it could be dealt with by transplantation of fat. This patient had remained free from cough ever since the operation; she had previously been under treatment in Switzerland and elsewhere. She came to him (the speaker) before last Christmas, and he immediately tried pneumothorax. It failed. She had some signs on the right side, and he waited until September this year before operating, in order to see whether she would recover sufficiently without doing an operation, and also to secure all possible quiescence on the disease on the right side. The symptoms and signs diminished on the left side, but every time she over-exerted herself she relapsed. Operation was therefore done, and since then all the symptoms had disappeared.

Another incision was the axillary. In children that was very satisfactory; it enabled one to get up to the second rib, and even the first. When the ribs had been cleared of periosteum, it was found that they could be snapped off posteriorly at the transverse process and anteriorly at the costo-chondral junction. When that extensive resection operation extending so far forward was done, there was greater danger of mediastinal flutter. In cases in which one left the anterior part of the lung supported by the ribs, if immediately after the operation one bound up the chest securely and left the bandage on as long as possible, mediastinal flutter would be avoided.

He concluded by saying that the benefits obtained by these various operations for the treatment of pulmonary tuberculosis were so great and so satisfactory that those who saw or experienced them could not help being converted.

Dr. G. E. Beaumont

said that in a combined discussion such as this physicians should consider: (1) Whether operative treatment was ever necessary. (2) If so, when should it be done? (3) What were the after-results of the operation?

In the consideration of the surgical treatment of pulmonary tuberculosis, the matter was naturally on a different footing to many other surgical procedures; the boundary between the surgical and the medical sides of treatment in this disease was much less clearly defined. On the one side there was the question of pneumothorax treatment which at present rested largely with the physician. On the other side was the question of the operation of thoracoplasty, a severe operation requiring a surgeon's highly trained experience and technique. The question of thoracoscopy, and cauterization of adhesions by its agency, occupied a midway position between these two. Even physicians looked inside the pleural cavity, sometimes they saw adhesions and generally they asked the surgeon to divide them. At times the surgeon failed to do so. He (the speaker) had been inclined to look upon chest surgery as being in an immature

stage in this country, somewhat resembling that of abdominal surgery thirty years ago, but the remarks of speakers in this debate, especially those of Professor Bull, seemed to show that it was in a very advanced stage. But he thought the physician's position in this matter was important, because it was clear that the amount of chest surgery done in any chest hospital in England would largely depend on the physicians on its staff, who were primarily concerned in the treatment; it was they who called in the surgeon to see whether he could do more to aid the recovery of the patient, or no. Physicians should therefore have a clear idea as to when a surgeon should be called in consultation over these cases.

Again, physicians must necessarily have rather a broader outlook on the question of the treatment of pulmonary tuberculosis than the surgeon, because only a small proportion of cases of this disease required even the simple operation of artificial pneumothorax; some authorities put the proportion at 2 per cent., others gave as large a figure as 15 per cent., or even 20 per cent. If artificial pneumothorax was possible the question of other operations did not arise. It was very important for physicians to try to adopt some sort of standard on this matter, and he would like to see a lead in that given at this discussion.

With regard to the kind of cases suitable for the production of artificial pneumothorax, the standards he adopted were the following:

(1) Cases were suitable for this in which the disease was unilateral and the patient failed to respond to absolute rest in bed for at least a month.

(2) Another group was that in which the conditions in the lungs were apparently very similar to the last, but they responded to absolute rest. When, however, the patient attempted to get up and to do any work, even for a short time, signs of activity of the disease became manifest by a rise of temperature or increased pulse-rate.

(3) Another suitable group was that in which there was hæmoptysis which did not yield to ordinary measures.

It was not for the physician to consider the varieties of operation, or which form was likely to yield the best results; rather he should try to decide upon which form of case pneumothorax should be tried. If anything further seemed to be indicated, the performance of thoracoscopy should be the first question, with possibly, division of adhesions, and if that was not possible, one of the forms of thoracoplasty must be considered.

Section of Medicine and Section of Surgery.

JOINT MEETING.

ADJOURNED DISCUSSION ON "THE SURGICAL TREATMENT OF PULMONARY TUBERCULOSIS."

EVENING MEETING:

Chairman—Mr. CYRIL NITCH (President of the Section of Surgery).

Dr. S. VERE PEARSON (Mundesley Sanatorium, Norfolk)

said that he had been asked to take part in this discussion from the point of view of the sanatorium physician. It was his intention to deal with thoracoplasty and thoracoscopy.

With regard to thoracoplasty, it was a little more than eleven years since he first urged a patient to submit to this operation, and he applied to the late Mr. Stanley Boyd for the requisite surgery, but Mr. Boyd did not feel himself competent to undertake the operation. At the present day, both the surgeons attached to Brompton Hospital were experts at the operation. Since that time, it was only in regard to about fourteen of the patients who had been under his care that thoracoplasty had had to be considered, and in only six of them had the operation been done. In the earlier period, if there had been British surgeons available for the operation, he thought the proportion would have been higher. Under 1 per cent. of patients in the sanatorium had been subjected to it. The proportion was far different with regard to artificial pneumothorax. The six thoracoplasties (one with phrenicotomy subsequently) were done, two by Mr. Morriston Davies, two by Professor Saugman, one by Mr. Romanis, and one by Mr. Tudor Edwards.

He thought a little more might have been said, in this discussion, with regard to the alternatives to thoracoplasty. Professor Bull and others had spoken of pneumolysis with paraffin or fat introduction, and the speaker was sure, from a study of the literature, that this procedure was now taking a back seat, as compared with thoracoplasty. That view was well summed up in an article in *Tubercle*, in January, 1922, by Dr. Ulrici, of Charlottenburg Sanatorium. When he met that gentleman, in Denmark, he told him his experience was founded on fourteen cases of pneumolysis. In the article, he summed up the advantages and disadvantages mostly in favour of thoracoplasty. Mr. Morriston Davies had also put the case clearly, in *Tubercle*[1] last month. Of course, there were exceptional cases in which apicolysis or even pneumolysis were suitable, but their number was very small. He (the speaker) thought thoracoplasty should take its place in nearly all cases in which pneumothorax had failed and surgical interference was thought to be an advantage.

[1] *Tubercule*, 1923, v, pp. 1-13.

In considering results, it was necessary to remember what a severe operation thoracoplasty was. Still, Professor Bull had shown that at an interval of not less than three years 31 per cent. of the patients were without symptoms; this was a very great triumph, because otherwise the majority of those cases would have had no chance of remaining alive, to say nothing of becoming free from symptoms.

The other point with regard to results was the following—and he said this more for the information of the general practitioner, who could not be expected to have any special knowledge on this subject. In some quarters there was a tendency to suggest that the results were not as good as they might be, because the operation was not carried out early enough. Quite often there was delay; certainly in the case of better class patients, much preparative propaganda had to be carried out before they would "jump" this particular operative "fence"; they were always hankering after the latest things which they had seen advocated in the Press, such as de-fatted antigens and garlic. But he did not think there should be any rush to do the operation, as it was such a formidable one. It had been said to-day that the indications were the same as those for artificial pneumothorax, but he did not agree, as the selection of the cases for thoracoplasty had to be stricter. Therefore it was important both for surgeons and physicians to be as clear in their minds as they could be as to the grounds on which the operation should be advised. In this choice, especial stress must be laid on the general condition of the patient and on his symptoms, rather than on physical signs. Professor Bull had just said that it depended not so much upon diagnosis as upon the judgment exercised in respect of prognosis. People found it very difficult to judge as to the activity of lesions in the sounder lung of the two; he (Dr. Vere Pearson) thought the best way to do that was to study the symptomatology and the general condition of the patient. Prognosis must be considered from two points of view. First, what was the prognosis of a case if the operation was undertaken; secondly, what was the outlook if no operation was done? To regard the matter separately from those standpoints, and then combined, was very helpful.

With regard to the choice of the anæsthetic, he hoped that before the conclusion of the discussion some anæsthetists would give the meeting the benefit of their views. At present he had an open mind in reference to the ideal anæsthetic, because there was a good deal of difference of opinion, and his own experience in this subject had not been wide enough to enable him to form an opinion of value. He did not think local anæsthesia should be abandoned, even if a good general anæsthetic was available and allowable. And he thought it was good to give omnopon and scopolamine; morphia was not so good, because of occasional subsequent sickness.

As to the choice of doctor, and the nursing, he was sure the conditions in this respect in Christiania must be very good. He was also sure that the surgeon who was not well versed in the operation, or had not an intimate knowledge of the technique, had better avoid attempting thoracoplasty. The ideal conditions were met with at the Vejle-fjord Sanatorium in Denmark, where the chief physician (now deceased) had also been a rapid and expert surgeon, and where everything was carried out under the best and most hygienic conditions. There were also nurses who could look after the patient well in the early critical stages after the operation. An effort should be made to get as near that ideal in this country as possible.

He was sorry more had not been said about thoracoscopy, as he thought that where a pneumothorax had been done, examination of the inside of the

thorax was a comparatively simple matter. He could not quite forgive his friend, Mr. Morriston Davies, for having stated that afternoon, that this was the only operation for this disease he had not performed. He would like to persuade Mr. Davies still further now to do it. It seemed to him the more scientific method of proceeding when there were band-like adhesions present. He confessed to having looked into the thorax nine times, and having five times introduced the instrument himself and having divided two adhesions in a pneumothorax cavity by means of the electro-cautery under the control of vision through the instrument. There was also another purpose for which the thoracoscope could be used. A few weeks ago he had had the opportunity, afforded him by Mr. Tudor Edwards, at Brompton Hospital, of looking inside an artificial pneumothorax case. What he saw there cleared up something that had been puzzling him for a long time in a few cases owing to an appearance he had seen by X-ray photography and by screening. He went back to his sanatorium and introduced a slight modification of his treatment of a case with benefit to the patient. Jacobaeus had now recommended the instrument in the treatment of other intra-thoracic diseases. He was sure that looking down through a scientific instrument, being guided by the vision that it gave and using an electro-cautery rather than a tenotome, was much better practice than groping about, if such a term was permissible.

A detail in connexion with thoracoscopy was worth mentioning; there was considerable risk of some surgical emphysema occurring, but that risk was diminished if one or two precautions were taken. If the skin at the site was pulled down considerably over the intercostal space where the instrument was introduced, a more valve-like opening was produced, and so there was less likely to be an escape of gas afterwards. And it was advisable to put a big pad of wool and a tight bandage on to the chest immediately after the operation. In the cases with which he had been connected, the patients usually slept for an hour or two after the operation, as they were still under the influence of the scopolamine and omnopon. Some, indeed, required a good deal of rousing even during the operation, although they never had a general anæsthetic. Coughing should be avoided as much as possible, and that was largely secured by giving the scopolamine beforehand, and employing a linctus for twelve hours or so following the operation.

He would mention just two general principles with regard to extra-pulmonary tuberculous chest cavities and their treatment. He thought the general profession did not sufficiently realize these two points: First, one ought not to attempt to do anything in the way of establishing free drainage; at all costs, secondary infection must be avoided, therefore tuberculous cavities of this nature must be kept closed. The other point was that the endeavour must be to bring the cavity walls into approximation, and that meant, often, a thoracoplastic operation.

He showed a photograph of a somewhat bilateral case before anything had been done. The left side was extensively diseased, the right side badly so. Pneumothorax was performed, as it seemed to be the only hope. When he was in London once, in February last, Dr. J. A. Torrens asked him to see the case at St. George's Hospital, when it was at the stage shown in the second skiagram. A pneumothorax with adhesions had been produced; some of the band-like adhesions were evident. From screening with X-rays, it was thought that those bands might easily be cauterized under thoracoscopic examination. Obviously that lung was not collapsed, as it should be. At that date the patient's condition was somewhat precarious, and it was agreed it was better

to leave him without even the introduction of a thoracoscope for a month. At the end of that period he was progressing favourably, and he progressed subsequently so well that he (the speaker) did not feel justified in introducing the thoracoscope, in spite of the fact that compression was never complete. Dr. Torrens told him that that patient was now walking about three miles; he was without fever and without cough, and was eating and sleeping well.

The next skiagram shown illustrated the fact that one must not pay too much attention to physical signs. A pneumothorax had been produced in the patient, a girl, aged 17. The skiagram had been taken a month or two ago, just before a refill, therefore the compressed lung was out a good deal. According to the skiagram, she had much disease on the other side, but physical examination showed there was only a little; she was practically a "fit" girl. Tubercle bacilli had been numerous, but there had never been much expectoration. At present she was symptomless and apparently "fit" in every way.

The next skiagram showed one of the cases operated upon by thoracoplasty; it was taken over a year after the second stage of the operation had been performed. It was one of Professor Saugman's cases; the speaker took the patient over to Denmark in August, 1921, with great trepidation, because he thought the Professor might turn him down with the remark that there was too much disease on the other side. That other side was at that time functioning well, and it now looked much better than it did before the operation. Before the thoracoplasty for eighteen months or more he had had continual fever and a continual coloured expectoration, often very much blood-stained, and once or twice his hæmorrhages were so bad that the patient's life was almost extinct. Within a few months of the operation he was up and about, and had lost nearly all his expectoration, and was able to walk two or three miles.

The next picture looked at first sight rather like that of a case which seemed suitable for thoracoscopy and cauterization. There was a partial left pneumothorax with some adhesions, and a fair amount of disease on the other side according to the skiagram. But, as a fact, that pneumothorax was begun at Mundesley eleven years ago, and until this year, when the skiagram was taken, he had not seen the man for ten years. He had been following his profession during all this time, but refused to give up having refills, because he felt more comfortable with them, therefore each month he went to his practitioner and got a refill.

Mr. J. E. H. Roberts

said that in this country, except for Mr. Morriston Davies, who had begun doing thoracoplasty some years ago, no British surgeon had cases on which the operation had been done a sufficient length of time to enable a good review of cases to be given. He therefore proposed to show a few figures in tables on the screen, taken from the writings of various Scandinavian surgeons.

SAUGMAN AND GRAVESEN.

(*Cases where pneumothorax was attempted.*)

47 cases. Complete pneumothorax without adhesions.

Able to work	70·2 per cent.
Not able to work	2·1 ,,
Dead of tuberculosis	23·4 ,,

42 cases. Incomplete pneumothorax with localized adhesions.

Able to work	33·3 per cent.
Dead of tuberculosis	66·6 ,,

45 cases. Incomplete pneumothorax with larger adhesions.

Able to work	11·1 per cent.
Dead of tuberculosis	86·7 ,,

77 cases. No pneumothorax possible.

Able to work	11·8 per cent.
Dead of tuberculosis	81·8 ,,

It is from the last two classes that the cases for thoracoplasty are drawn, and it is with these figures that the results of the operation must be compared.

SAUGMAN AND GRAVESEN.

105 cases of thoracoplasty.

Positive results	69·9 per cent.
Negative results	30·8 ,,
Operative mortality	8·6 ,,

SAUGMAN AND GRAVESEN.

69 cases. Two to seven years after thoracoplasty.

Able to work	44·9 per cent.	55 per cent.
Not able to work	10·1 ,,	
Dead of tuberculosis		42·1 ,,

JACOBAEUS AND EINAR KEY.

44 cases of thoracoplasty. Three to seven years.

Able to work	27 per cent.	52 per cent.
Better	18 ,,	
No better or worse	7 ,,	
Dead of operation	11 ,,	37 ,,
Dead of tuberculosis	26 ,,	

P. BULL.

37 cases of thoracoplasty. Three to eight years.

Able to work	32·4 per cent.	34·7 per cent.
Not able to work	2·7 ,,	
Dead of operation	10·8 ,,	54 ,,
Dead of tuberculosis	43·2 ,,	

These figures do not represent the present practice of any of these authors as they include all their early cases and those done when the one-stage operation was performed, but are shown because they are all cases which have been completed more than three years ago.

Professor Bull had told the meeting that day that the mortality, in the one-stage operation was 30 per cent., whereas in the two-stage operation it was little over 4 per cent. It was very likely, therefore, that, as technique improved, the mortality from the operation would be lower than at present, although in the hands of all the surgeons who had had a sufficient number of cases, the total mortality was very uniform. There was a good *prima facie* case made out for thoracoplasty, and the figures justified at least careful consideration of the claims of the operation. He had recently made a trip to Denmark,

Sweden and Norway, and by the courtesy of the Scandinavian surgeons and physicians, he had been able to study and compare their methods, and see some of their results. He had found that, on the whole, the same type of operation was being done in all those countries, i.e., posterior resection of the ribs, from, and including, the first to the tenth and eleventh, through a hook incision. The rib was removed as far as the tip of the transverse process, as against Sauerbruch's operation, which extended only to the angle of the rib.

He had found that in the various centres the selection of cases for operation differed considerably. At the Vejle-fjord Sanatorium, Dr. Gravesen, having convinced himself of the utility of the operation, did not now feel he ought to withhold it from the patients who, after a prolonged stay in the sanatorium, when artificial pneumothorax was not available, were going steadily downhill, though they had a considerable amount of active disease in the better lung. Dr. Gravesen, he thought, undoubtedly saved the lives of some of these patients. It was equally true that some of the patients went downhill after the operation, and that if the Professor continued to operate on that type his statistics would suffer in comparison with those of more conservative surgeons, though it was probable, on the present figures, that at the end of ten years there would be more patients living than would otherwise have been the case. He (Mr. Roberts) had seen some of Dr. Gravesen's cases, and that physician showed conclusively that even with a considerable degree of active disease in the better lung the condition of the bad lung improved, and with it the better one, just as when a tuberculous kidney was removed, the tuberculous bladder of the patient might get better also. Still, he (the speaker) did not recommend that in England surgeons should attempt to operate on that type of case at present. In Christiania, Professor Bull was very much more conservative; he demanded that there should first have been a failure of the pneumothorax treatment, and that there should have been a period of sanatorium treatment before operation was contemplated, and that, even then, the operation should only be decided upon if in the better lung there was little or no active disease.

In Stockholm, Professor Jacobaeus took the intermediate position; he was having cases operated upon by Dr. Key which, the speaker thought, Professor Bull would not have dealt with so; yet, on the other hand, he was operating on some cases as bad as those upon which Dr. Gravesen was operating.

Until surgeons in this country had had a great deal more experience of the operation, the right attitude was a very conservative one; no other attitude was justifiable until that larger experience had been gained, and until we were fully aware of the results the operation had yielded.

He found in every centre that partial thoracoplasties were being abandoned, and that in the majority of the cases the complete operation was done; also that the operation was being done in two stages.

With regard to the anæsthetic, he was pleased to find that all over Scandinavia local anæsthesia was being abandoned, not only on the ground of greater comfort for the operation, but also because there were fewer post-operative disabilities, and the convalescence was smoother after general than after local anæsthesia. In Stockholm operations were done under local anæsthesia, but they had had several cases of novocaine poisoning. At Vejle-fjord the operation was continued under a general anæsthetic as soon as the patient felt pain.

Professor Bull combined thoracoplasty with apicolysis (in which practice he acted alone) on the ground that the free upper apex sank into the thorax,

and was much more readily and fully compressed than if it were not free; that disposed of one of the objections, that the thoracoplasty did not compress the apex as much as the rest of the lung, whereas the apex should be compressed most.

He (Mr. Roberts) wished to emphasize one or two points:—

(1) That the patient required preliminary treatment before the operation, but it was better to administer glucose and saline twenty-four hours before the operation, per rectum.

(2) That the after-treatment was as important as that during the operation. He was impressed with the need of immobilizing the chest wall by putting on broad strapping at the end of the operation, and compressing the chest on that side only, as much as possible.

(3) A point no speaker that day had mentioned was the effect of doing thoracoplasty on cases in which there was already a pneumothorax. If there was a fairly large pneumothorax and the pressure was fairly high, one had to be very careful how one did thoracoplasty without disturbing that pressure, otherwise the heart might be displaced. On the other hand, if a pneumothorax had been done before the operation, the immediate effects of the mobilization of the chest wall were much less marked and convalescence was smoother.

Dr. L. S. T. Burrell

said that he had often found phrenicotomy to be of considerable service in the treatment of pulmonary tuberculosis, and he did not think its value was appreciated. In a very large number of cases of artificial pneumothorax there were adhesions to the diaphragm, and although these often did not affect the treatment, in some cases they did affect it. Division of the phrenic nerve in such cases gave additional rest to the lung for six or nine months. After that time the diaphragm would have recovered. The operation was simple and safe, and did not permanently paralyse the diaphragm.

With regard to thoracoplasty his experience had been small but not fortunate. The mortality from the operation itself had been small in the cases he had had operated upon, but in several instances it had been "touch and go" for the first two or three days after the operation. Unless a general anæsthetic was given there were usually pain and shock at the operation and most patients had had considerable pain for the first two or three days afterwards. More than one patient, both in this country and abroad, had told him that he could never again go through such an ordeal. In six of his (Dr. Burrell's) cases the operation had been done for non-tuberculous disease and so they did not concern us that day, but they confirmed his opinion as to the serious nature of the operation. In the tuberculous cases the ideal case was not often found. In one of his patients the immediate result of thoracoplasty was dramatic, the symptoms almost disappeared and the condition improved remarkably, but in four months the patient had relapsed and had gone rapidly down hill. One great objection to the operation was that it could not be undone. When a lung was rested by medical means, such as by sandbags on the chest or by artificial pneumothorax, it was possible to allow the lung to work again if for any reason it was necessary to do so. He had often been struck—and especially in cases where the disease was spreading rapidly—by the way in which the other lung sometimes showed increasing signs whilst the refills were being given, but when they were stopped and the treated lung was allowed to re-expand there was at once improvement in the other lung. In such cases the pneumothorax treatment

seemed to have converted acute cases into slowly spreading chronic cases. But the pneumothorax must be only temporary and the lung allowed to re-expand before there was too much invasion in the other. Artificial pneumothorax would often be justifiable where the condition of the other lung absolutely contra-indicated thoracoplasty. But there were a few cases for which the operation seemed to be the only method of treatment, and in these he thought it certainly should be recognized as a useful weapon against pulmonary tuberculosis. He (Dr. Burrell) wished to express his opinion from the medical point of view that it was not a procedure to be lightly recommended.

Mr. A. Tudor Edwards

said that his experience was confined to two main methods: (1) Thoracoscopy and cauterization of adhesions and (2) thoracoplasty. In regard to the former, the method was confined to cases in which complete collapse of the lung was prevented by band-like or film-like adhesions between lung and parietal pleura but was of no value where there were large surface adhesions. Preliminary stereoscopic skiagrams were essential. An account of the routine method adopted was then given and an account of the resulting collapse shown by three skiagrams.

With regard to thoracoplasty, he stated that it was not to be looked upon as a cure for pulmonary tuberculosis, but as a method which put the patient under the best circumstances to establish a cure, by further adequate treatment, preferably at a sanatorium. The indications and contra-indications were then discussed by the speaker and he pointed out that the presence of old quiescent tuberculous changes or of recent slight involvement of the opposite lung did not debar operation; but he said that the major operation should never be done without a preliminary attempt at the induction of artificial pneumothorax.

In his own cases local anæsthesia had been used, except for the last two, in which gas and oxygen had been administered. A comparison of the fall in blood-pressure during operation by the two methods of anæsthesia showed no definite difference, the average being about 6 mm. Hg. Local anæsthesia was indicated, where considerable cavitation was present in the lung and considerable excess of secretion was poured into bronchi during operation.

Points in pre-operative and post-operative treatment were then mentioned. Ten cases had been operated upon, all done by two stages with one operative death; in this case the systolic blood-pressure before operation was only 90 mm. Hg and death occurred on the third day.

In three cases the tubercle bacillus had disappeared from the sputum within three weeks of the operations.

The period following operations had been too short to judge as to the end-results in the cases.

[A series of coloured representations of thoracoscopic views of the interior of the chest, normal and pathological, were then shown on the epidiascope.]

Mr. C. Firmin Cuthbert (Gloucester)

said that he had recently visited Dr. Gravesen at Vejle-fjord Sanatorium where he had had an opportunity of seeing a number of cases which had been treated by thoracoplasty. It was very advisable to divide the ribs quite close to the transverse processes of the vertebræ and without any projecting angle, in order that the movements of the scapula or the freedom in raising the arm above the head should not be impeded.

Dr. Gravesen made use of a large pad, weighing twelve pounds, to place above and below the clavicle, which assisted in compressing the structures over the lung and prevented movement of the side operated upon. It seemed that the left side was the more favourable for operation, as the heart stood the pressure better on this side; whereas on the right, the soft side of the heart suffered more from the effects of the pressure. The post-operative scoliosis always deviated to the operated side, and this might be considered favourable, as it helped to compress the lung. If it was necessary to hide any deformity, a pad and belt could be worn. Professor Bull had laid great stress upon the absolute co-operation between surgeon and physician; he (Mr. Cuthbert) would also include the intimate association of the radiologist, for there were many cases of apparent unilateral disease in which no physical signs were revealed in the other lung, but in which the X-ray plate revealed a considerable amount of active disease in what was thought to be a sound lung. Without doubt many cases would be benefited by a pneumothorax, and the question arose as to when this should be recommended in the event of hæmoptysis. Often the first indication of disease was a sudden attack of hæmorrhage. The patient was put to bed, carefully treated for a number of weeks, and then it was discovered that there was a large area of disease from the rapid invasion of tubercle in the fertile ground of disintegrated blood-clot. Although it was held that in the acute stage of hæmoptysis it was not good to do a pneumothorax on account of the liability to aspiration of blood, it could be done with safety when the bleeding was reduced, but continued in small quantities, and it would thereby control the free dissemination of tubercle in the old remains of blood-clot, and forestall the formation of adhesions which had such an adverse effect in the future, in preventing the compression of the lung. Tuberculous joints, by being immobilized, were given opportunities for the formation of granulation tissue, and it was the same in lung compression. As Professor Bull and others had said, it was only in those cases in which formation of adhesions prevented the pneumothorax from producing collapse of the lung that the severe measures of thoracoplasty should be undertaken; yet due care must be taken not to wait till the patient's condition was exhausted by the result of septic infection. In one special case, in which the lower part of the lung could not be collapsed on account of adhesions to the diaphragm, there was no difficulty in doing a complete pneumothorax, after the phrenic nerve had been divided in the neck (under local infiltration of novocaine) as it lay on the scalenus anticus muscle. Bell-sounds in the chest after pneumothorax were not uncommon, but apparently they were not due to communication with the lung.

Mr. Cyril Nitch (Chairman)

tendered the thanks of the two Sections to all the speakers who had participated in this discussion and thrown light on the subject. Professor Bull, Dr. Vere Pearson and Mr. Morriston Davies had travelled considerable distances to be present, and the Sections felt very grateful to them for the time and trouble they had expended. Thanks were also due to Dr. Stanley Melville for the beautiful X-ray films he had placed on exhibition.

He had had no personal experience in this particular field: he came to learn, and he had learnt much; and it occurred to him that the worker in this domain should be an individual with the inclinations of the physician and the subtle ways of the surgeon. As Professor Bull had pointed out, it required the close co-operation of physician and surgeon.

The trend of opinion seemed to be that an artificial pneumothorax was the first essential, and that, following that, thoracoplasty was the best operation, with division of the ribs at their angles.

He was rather surprised that Mr. Morriston Davies was not in favour of thoracoscopy; but now that he had seen the beautiful pleurograms of Mr. Tudor Edwards, perhaps he would change his mind. He believed Mr. Morriston Davies divided the bands with a tenotome, with the pleural cavity full of nitrogen. Was that under pressure? (Mr. Davies: Yes, under sufficient pressure to keep the band taut.) Had he ever met with cases of gas embolism following the operation? He said the bands sometimes bled. (Mr. Davies: No.) In urological work deaths from air embolism had followed aero-distension of the urethra from air under pressure entering a minute vein which had been injured by the instrument. He considered that gas embolism was a real danger and a serious objection to pneumolysis under nitrogen distension.

Professor BULL (in reply)

said he did not feel he had much to answer in regard to the debate, but hoped to be favoured with personal connexions with medical Englishmen; and if his hearers and their colleagues in this country felt they could learn anything in Norway, he need scarcely assure them they would be very welcome in Christiania.

Sections of Medicine, Neurology, Obstetrics, Psychiatry and Surgery.

DISCUSSION ON POST-OPERATIVE AND PUERPERAL MENTAL DISORDER.

Chairman—Dr. T. B. HYSLOP (President of the Section of Psychiatry).

Dr. T. B. HYSLOP (Chairman)

said the purpose of the meeting was to discuss some problems which, he thought, fittingly ought to be discussed under this Section of Psychiatry, and there were present Members of other Sections, who would, he hoped, throw light on the questions concerned.

He did not think there would be much difference of opinion as to the importance of heredity in the rôle of the production of mental disorders which were actively caused by operative measures, or by such a condition as the puerperium. With regard to the mental factors of causation, probably all were agreed from the first that anxiety, caused by such a circumstance as illegitimacy, was sufficient in itself to produce a certain mental condition and to precipitate mental disorder.

From the bodily point of view, he hoped the Members would hear much. The effects of a severe labour, of the suppression of the lochia, &c., were well known, and also the influence of previous attacks, so much so that it had become almost a saying among alienists that when a woman had developed a habit of suffering from puerperal mania or melancholia, it was best for her to wait two or three years before becoming pregnant again, so that the influence of suggestion from the previous attack might have a chance of being obliterated. It would be well to hear an expression of opinion as to whether the mental disturbance was more prone to occur in the primiparous condition, or the opposite. An old Scottish saying, which he well remembered, was "Look out for the brown-eyed puerperal." He had looked out for that type, and it was the fact, in his experience, that a large proportion of those who suffered from puerperal insanity were brown-eyed.

Alienists who practised in asylums had come to note that there was now a comparative absence of acute febrile puerperal conditions of the septicæmic type which used to be seen in former days. Whether that was due to some subtle change in environment, or chiefly to more enlightened methods of

dealing with the puerperium, was for Members to determine; he hoped there would be an expression of opinion about it.

Two prominent types were now recognized: the acute, sub-maniacal type, sometimes going on to acute mania, and the stuporose type. As a sub-section there was the type in which there were hallucinatory phenomena. Commonly an attack of puerperal insanity commenced during the night hours, and it seemed to be the outcome of a dream-state, the continuation of the dream taking the form of terrifying hallucinations or illusions. In this condition the woman might commit infanticide, bringing the cases into the medico-legal domain. In this question arose one of the most painful situations: that of a mother who, while puerperally homicidal, killed her child, and who subsequently improved so much that the mental symptoms disappeared, she being adjudged "Guilty, but Insane," and ordered to Broadmoor to await His Majesty's pleasure. He would like to hear some opinions on that; his own views were not in harmony with those expressed in the Law Courts at the present day.

He would also like to hear of types of perversions, also views as to the duration of these puerperal manias and melancholias. Those practising in asylums were accustomed to recognize six weeks' time, three months' time, five months' time, &c., as fitting certain types, and did not regard cases of even lengthy duration as necessarily incurable.

In this discussion it would be well to keep strictly to the clinical and pathological phenomena, and he called upon Sir Charters Symonds to open the discussion.

Sir CHARTERS SYMONDS

said that speaking from a forty years' personal experience in surgery he could recall only four cases in which mental disturbance resulted as a direct effect of operation. In all four, the conditions preceding the operation, and the mental disturbance, were different.

The first case was that of a man a little over 60 years of age, a vigorous and robust person, who had had intestinal obstruction for a long time. At that date, 1883, patients with this condition were allowed to go for a long time before the surgeon was called in. The operation, lumbar colotomy, itself went very well, but there was an enormous evacuation of material from the bowel, as was usual in these long-standing cases. Possibly there was a condition of toxæmia which determined the attack. A few days after the operation he showed mental disturbance, and was removed to the strong room, where he had to be kept strapped in bed. In respect of his talkativeness, extreme restlessness and refusal of food, the condition resembled delirium tremens. He was nursed by one of the old-fashioned nurses, who shared the speaker's opinion that the man would get well if he could be fed. He recovered in six weeks and died of his disease, which was malignant, eighteen months later, remaining well mentally.

Would it be considered that the cause of the mental symptoms in this case was toxæmia? There was no rise of temperature, no general bodily disturbance. In such cases he did not think it was necessary to send the patients to an asylum, as probably the mental illness would be of short duration.

In the second case the antecedent conditions were interesting.

The patient, a girl, aged 20, had advanced appendicitis, with a good deal of suppuration. The next day she showed symptoms of mental disturbance, chiefly in the form of singing and mild delusions. She remained in that condition eight or nine weeks. She was not noisy, nor inclined to get out of bed, but was difficult to feed. She was of the emotional type, given to phantasies and dreams, and she used to write stories—

altogether not a very stable type. There was rise of temperature for some time. Her mother was a sound, sensible woman; her father was dead.

The third case also presented a very interesting problem.

This patient was a highly intellectual man, aged 76. He was seen by the speaker on the fourth day of an attack of appendicitis, and had a localized swelling; he was frequently ejecting a brown fluid. At 5 p.m. he was given a large quantity of water, which he rejected with much brown fluid with the aid of his finger applied to the back of his throat. He was directed to repeat that process at 9 p.m., as operation, though urged as immediate, was not so considered by the physicians in charge. A pint of brown fluid was rejected, and balanced on the edge of the basin was a feather, with which he had stimulated the ejection. After eight days the patient's mental balance was upset; he was excited and refused his food, and there was great difficulty in nursing him. As he had a loose sphincter, he could not be given saline, and he lived on claret and water—his own choice. A very distinguished physician was in charge, and a local practitioner was in attendance. They were both of the opinion that the condition was senile, but the speaker was convinced it was a post-operative mental disturbance, and that if the patient could be kept going for four or six weeks, he would get well. The medical practitioner was really a handicap, but he became ill and could not attend the case; Sir Charters then secured a younger man who shared his views as to the possibility of recovery. At the end of about three weeks the patient became so thin and the skin so dry—his knees being almost excoriated from the constant rubbing against the bed clothes—that it seemed he must die unless some fluid could be got into him, and he (Sir Charters Symonds) therefore put a pint of saline into each axilla. Following that, he had a fairly good night, and next morning he was better and gradually got well, with complete restoration of his former intellectuality. During the whole time there was no increase of temperature. It would be interesting to know why he broke down; he was not a senile type, and was a very sound kind of person. He lived and worked for some years after.

The next was a prostate case, and he would like to ask his surgical colleagues what was their experience with these cases. Though he had operated on the prostate many times, this was the only case of the kind he had had in which there was mental disturbance following the operation. He thought it would probably occur here, because the man was an excitable Jew, and he had had a good deal of trouble with his prostate and had become difficult to control. He (Sir Charters) warned the relatives that although it was all that could be done for the man, mental disturbance must be expected. He had a severe mental attack, and had to be removed from the nursing home, as he was too noisy.

He had permission to say that in Lambeth Hospital, where Dr. Stebbing did many of these prostate operations, there had been no example of post-operative mania; some of the patients on admission were fidgety and difficult to manage, these symptoms disappearing after operation—just the reverse of what might be expected.

He would like to hear about the allied condition, delirium tremens, which came on after injuries. In his (the speaker's) ward at Guy's, there were two or three cases of delirium tremens every year in men who had met with a simple fracture of the tibia and fibula. Occasionally one could prevent the onset of the delirium by watching the patient carefully, and giving a heavy dose of paraldehyde to induce sleep. These were men not given to bouts of drinking, though they undoubtedly took more than was good for them, and they remained well and at work until the accident, and then the delirium tremens developed. He suggested that there was probably some relationship between these cases and the post-operative kind, as both followed injury. When the mental symptoms followed acute illness, e.g., pneumonia, he thought there was no necessity to remove the patient to a mental hospital, and that

whenever possible such patients should be treated at home. In one instance, notwithstanding strong pressure to the contrary, this course had been followed with success. His experience was that there was no need for the removal of these patients, and that within six weeks recovery might be expected. It was interesting to hear the Chairman mention of six weeks in the puerperal cases.

DR. JAMES S. COLLIER.

On entering into this discussion upon the mental disorders which may follow operative procedures and the puerperal state I am impressed chiefly with my own ignorance of this subject, for in the experience of a neurologist and general physician the events which we are considering this evening are of rare occurrence, and I must necessarily confine any remarks to a somewhat small clinical experience of these rare happenings, the rarity of which alone has perhaps led to my not reflecting upon them very deeply. I have had a very large experience of operations upon the brain and have repeatedly seen every accessible region of the brain destroyed in the removal of tumours. Yet, though mental reduction has been a common result of brain destruction, mental aberration has been conspicuous by its infrequent occurrence, and when it has occurred it seems to have little or no relation to the region of the brain involved. Lesions of the brain seem usually to be associated with mental reduction and only very rarely with mental aberration, general paralysis of course excepted. Even with lesions of the pre-frontal lobes mental symptoms are for the most part entirely absent, or are not exteriorized so as to give us much help in local diagnosis. The very striking and common condition of post-concussive insanity seems never to occur from operations upon the brain, even though the brain may have been severely handled or pressed upon during the operation, and therefore I am sure that Meyer's theory that post-concussive insanity is the result of traumatic œdema of the brain cannot be correct, for œdema of the brain is seen in its highest known degrees in conditions of cerebral tumour, and no condition similar to post-concussive insanity has ever resulted under my observation.

I can only recall four cases in which an operation upon the brain was followed by insanity. Three of these cases were almost identical. They were cases of tumour of considerable size situated in the central region of one hemisphere and the tumour was extirpated widely, part of the wall of the lateral ventricle being removed in two of the cases. The shock of the operation was profound in all, and mania of a violent type appeared early on the third day after the operation and persisted until death, which occurred within a fortnight in each case. In each of these cases there was slight pyrexia. In none of them were there antecedents of family or personal insanity, of alcohol or of syphilis, neither had any sepsis occurred, nor was there any obvious cause for death found at autopsy.

The fourth case is interesting from the fact there was recovery from the condition of insanity. It was a case of a large and long standing tumour of the right occipito-temporal region which was decompressed widely by Mr. Sargent. No attempt was made to remove the tumour on account of its deep position. Hedonal was used as the anæsthetic and there was the usual long lasting unconsciousness which follows the administration of that drug. There was some shifting of the tumour-pressure after the decompression, with the result that a left hemiplegia developed which took some weeks to pass away. A week after the operation the patient developed delusions and became suspicious, erotic and dirty, and as his physical condition improved his mental aberration

got worse; and he became so troublesome that he was placed under certificate and sent to Virginia Water, where in the course of six months his mental state recovered and he remained normal until his death some two years later from hæmorrhage into the growth.

In the first three of these cases it may be argued that the profound shock and the serious interference with the brain were the factors causing the mental disorder, but if so why are these events so rare in like circumstances? In 1,985 cases of injury to the brain in warfare only thirteen cases of insanity were noted, and Stolper collected another series of 981 serious head injuries with insanity following on twelve occasions only.

In the fourth case which I mentioned there was recovery from the insanity, notwithstanding that the cerebral lesion remained and increased. It seems, then, that operations upon the brain and serious injuries to the brain do not give rise to insanity much more frequently than do operations and injuries in other parts of the anatomy.

I have seen three cases in which removal of the prostate was followed by melancholia, which became marked at the period when complete relief and comfort had been obtained. In none of these cases was any renal inadequacy or sepsis a feature. One of these patients recovered, another remains melancholic, and the third rapidly became asthenic and lethargic, and died, nor was any definite explanation forthcoming from the autopsy as to the real cause of his death.

In those cases of operations upon the pituitary gland, the thyroid gland and the suprarenal gland which have occurred under my observation, I have not met with any mental disturbances, but I have seen them in connexion both with hepatic and renal operations where there has been obvious sepsis with severe interference with hepatic or renal function, and such cases have always ended fatally.

Most of the cases of post-operative insanity which I have seen resemble those which may occur after many and varied medical events of illness. For example, two straightforward cases of lobar pneumonia have recently been under my care, the patients being middle-aged subjects in whom, after the crisis had occurred, a very marked but entirely transient condition of delusional insanity developed and persisted for some weeks.

I have difficulty in making any generalizations from cases of post-operative insanity. Heredity of insanity has not been found in any of the cases which I have seen, nor has any recurrence of insanity occurred to my knowledge in those cases which have recovered. Mr. Clinton Dent recorded several cases in which transient insanity had occurred after his operations and in which he had subsequently performed other operations without any mental disturbance following. I do not think that anxiety and fear are factors, for I have seen the condition occur in the most stolid and unemotional of patients. The severity of the operation does not seem to count, since insanity has followed the most trivial operations. Nor does the anæsthetic appear to be important, for the condition which we are discussing has followed operation under every variety of anæsthesia and has occurred with operations where no anæsthetic at all has been employed. The cases occurring in children at an age when transient insanity is very uncommon are striking and recovery from the insanity is said always to take place. I saw one such case some years ago and the outlook as regards mental stability was considered grave, but this patient has since remained in every way normal.

As regards their actual cause and origin these cases seem to be as intangib e as do cases of non-hereditary insanity in general.

Mr. Aleck Bourne.

Ætiology and Prognosis of Puerperal Insanity.

It is well known that those morbid conditions which lie on the border line of two specialities of medicine, and may therefore be claimed by both groups of workers, are in danger of neglect because each repudiates the responsibility of special study.

In other cases we find that any given disease may be closely observed and viewed from the different aspects of two special branches of medicine, but there is lacking sufficient coördination between the two to present the condition as one undivided whole. For example, the affections of the thyroid gland are of interest both to the physician and the gynæcologist, but each is apt to regard the condition from his own point of view only.

Our subject this evening, the ætiology and prognosis of puerperal insanity, is another instance of the tendency for incomplete study or neglect of a point of contact between two groups of special workers. By the analysis of about sixty cases of puerperal insanity I have made an attempt to define the obstetrical relationships in both ætiology and prognosis.

In addition to a few cases of my own I have collected the notes of others from Queen Charlotte's Hospital and Paddington Hospital, and I am indebted to the resident medical officers of St. Thomas's and the General Lying-in and the City of London Lying-in Hospitals for sending me further reports.

The chief difficulty in producing any account of the subject has been the inadequacy of the clinical notes. Whilst these records have frequently supplied detailed descriptions of the labours, there have been only the scantiest details of the insanity or any factors of ætiological importance. An additional difficulty in analysing the records is that these patients have been quickly transferred from the lying-in hospital to an infirmary at the earliest signs of mental disturbance. It has been necessary to trace nearly every case to the infirmary, whence again, after a short stay, they have been moved to various asylums.

By following them up to the asylum it has been possible to obtain in a large number of cases a fairly complete account of their subsequent history, while in a few cases I have succeeded in tracing the patients to their homes after so long a period as twelve years.

Ætiology.

Sufficient ætiological details have been forthcoming in sixty-one cases. In few instances have there been furnished full accounts of antecedent conditions, but it is clear that physical puerperal diseases have played a large part as exciting causes. How far they have been only exciting causes, operating upon patients already predisposed to mental disease either by congenital feeble-mindedness or by some hereditary taint, it is impossible to be certain, but from my survey of the cases I have the impression that as a rule physical disease has been a sufficient factor even in the absence of any tendency towards insanity. This is not true of all cases, however, as I have notes of patients who have indicated a mental instability by such histories as a mental breakdown at puberty, and repeated insanity with successive confinements; but it is true in general that those patients with serious physical disease do not appear to have had a bad family or individual history. In some cases the notes have been sufficiently detailed to make it clear that no obvious cause could be traced. In nine of the sixty-one patients no cause of any kind of

insanity was apparent. Five were single and four were married. It is possible that pregnancy in the single state may have induced a constant and severe anxiety which caused mental exhaustion and finally a breakdown at the stress of labour. This view gains support from the fact that the single patients amounted to so high a figure as 36 per cent. of the whole. It might be argued that mentally feeble single women are more liable to become pregnant than those of sound mental health, but that is not borne out by the figures. For example, of twelve patients with previous mental trouble or a bad family history, only one was single. We may suggest, therefore, that the worry of an illegitimate pregnancy may have an important bearing on the incidence of puerperal insanity in single women.

The chief organic disease associated with insanity is one of the many forms of puerperal sepsis. Uterine infection, with or without blood infection, accounted for eighteen out of sixty-one or 27 per cent., and of these eighteen thirteen cases were serious, some of them proving fatal. High fever from any cause is liable to provoke a certain amount of mental aberration, but it quickly passes with the fall of temperature. In insanity associated with puerperal sepsis we find that the mental trouble lasts many months after the fever has subsided.

Five of the eighteen cases were described as being only of slight degree and it is doubtful how far it was an exciting cause. There were three cases of severe and persistent pyelitis, while rheumatic infection and cardiac disease were noted in one case, and severe influenza in another.

After uterine infection the next most important organic disease was eclampsia. Of the sixty-one confinements there were nine cases of eclampsia or 13·5 per cent., in all of which there is a note that the women were married. It is well known how eclampsia causes mental symptoms, usually in the direction of coma during the acute stage, sometimes accompanied by violent restlessness. In one of my own cases, as the coma passed off the restlessness increased, and the patient had to be controlled by four or five nurses until she could be finally removed from Queen Charlotte's Hospital in a condition of violent mania. Another of my eclamptic patients passed from her state of coma in which there was no restlessness into a condition of stuporose melancholia with an aversion for the baby, from which she recovered in two or three weeks.

The character of the labour, that is, whether it were normal or difficult and complicated, appeared to have no ætiological significance. Of sixty-five confinements of which details were given, there were eight cases of difficult labour (12 per cent.) with various conditions, including severe hæmorrhage, manual dilatation of the cervix and forceps extraction. Several of these patients appear in the category of infection, to which the insanity was probably due.

Primigravidæ formed a much larger proportion of the total than multiparæ, namely 59 per cent., while second gravidæ formed 14 per cent., third gravidæ 8 per cent., and those confined for the fourth or more times accounted for 19 per cent.

Passing now to the influence of an unstable mental equilibrium and a bad family history, there are eighteen, or 27 per cent. Thus of all cases more than one quarter give signs of a predisposition to mental disease. If the notes had been more detailed, it is probable that this figure would have been higher.

Four out of the eighteen were epileptic, of whom three were single women. In four there was a history of previous sojourns in a mental hospital, while five had suffered puerperal insanity after previous confinements, and five came of a stock in which insanity had appeared in former years. There were one or two

patients who had suffered from great anxiety during pregnancy, one on account of the development of phthisis, and one whose husband was reported missing in the war. It is probable, as perhaps in the case of illegitimate pregnancy, that a severe anxiety had contributed to the mental disease after the confinement. A third patient, whom I saw, had had previous mental trouble for a short time at puberty, and during her second pregnancy had undergone a great deal of constant household trouble which caused her to give way to fits of temper. Her first pregnancy and confinement had been uneventful. I believe this patient illustrated the effect of anxiety during pregnancy on one who had previously shown symptoms of mental instability.

To sum up, we can say that puerperal organic disease, particularly sepsis and eclampsia, is capable of provoking insanity in patients who have not hitherto shown either in themselves or their families any evidence of mental weakness. In a few cases insanity supervenes without any obvious cause, though I believe that in some a closer examination would reveal signs of a predisposition. Pregnancy and labour obviously throw a real strain on the mental constitution of the patient as well as the physical, inasmuch as in 27 per cent. of the cases it provoked the onset of insanity in those showing a mental taint.

Time and Manner of Onset.

Of sixty-four cases four developed insanity during the latter part of pregnancy. Of these four patients one was feeble-minded, a second showed no physical disease. Both were melancholiac and stuporose and are still in mental hospitals. A third patient became insane during pregnancy while some obscure fever with meningeal symptoms developed. After labour she grew worse, but was declared not to have true meningitis. She died within a few weeks. The fourth patient thought she had phthisis, which caused her great anxiety and rendered her melancholiac, but she recovered her mental equilibrium in two months after delivery.

Six patients became mentally affected during labour, of whom some were eclamptic, thirty-nine cases showed the first symptoms during the first week, and only eighteen were first affected during the second week. In three cases the patients became insane from three to six weeks after delivery, one of whom had a collateral history and has been in an asylum twice since her confinement. The other two patients suffered from prolonged and severe sepsis, one of whom died.

The incidence of insanity was equally common on the third to the seventh days, while only one case is reported on the second day. The manner of onset was very similar in the great majority. There had usually been some nights of sleeplessness after which the patient was described as being "peculiar," inasmuch as she talked with a little incoherence, which showed some deep-seated anxiety. From this indefinite state she quickly developed more obvious symptoms such as aversion for food and the baby, and delusions that she was being poisoned or given chloroform, or that the baby was not her own. Becoming more and more agitated and restless, by the second day she was trying to get out of bed, exposing herself or making spiteful attacks on nurses. Most of the cases were maniacal within three days, constantly screaming and throwing their limbs about. A lesser number became depressed and stuporose, or morbidly religious, with intervals of weeping and hymn-singing.

Results.

Sufficient details of the after-histories have been obtained to show a mortality of thirteen patients out of fifty-eight (22 per cent.) Of the thirteen fatal cases six died of the effects of infection, while two died by self-destruction, jumping out of the window, two died of cardiac failure, one after eighteen months in an asylum, while the cause of death of the remainder was not given.

It is clear, therefore, that puerperal insanity is a serious indication of the severity of the organic disease which causes it. Since the compilation of these figures I have had another case of long-standing streptococcal pyelitis in which the patient became insane and died soon afterwards.

I have been able to trace thirty-six patients who survived. The ultimate result is described as "recovered," "relieved," or "*in statu quo*."

Of the thirty-six patients, twenty-eight (78 per cent.) have been discharged from asylums or infirmaries after a varying time as "recovered," while four were "relieved." Occasionally a patient described as "relieved" has been readmitted to the asylum for a further attack of insanity. Four patients are still in asylums in a chronic condition of insanity.

Comparing the cause and variety of insanity with the ultimate result, we find that six out of seven patients in whom there was no obvious ætiological factor, recovered, and four others of whom nothing was stated also recovered. The one patient, who is in a persistent stuporose amnesic condition, was a married woman who developed melancholia during pregnancy and was delivered of her first child only eighteen months ago. There was no organic disease discovered either in the infirmary or asylum.

There were thirteen cases of uterine infection traced, but of these six died and seven recovered. Three of these recovered mentally after nineteen, eighteen, and eight months respectively, while the other four patients were discharged after only a few weeks.

Six patients with eclampsia who have been traced have all recovered, which is a result, I believe, in accordance with everybody's experience of eclamptic insanity. The majority of these recovered in a few weeks, while two were in the asylum for six and nine months respectively.

I have been able to trace only nine patients who had had previous attacks of insanity or a bad family history; of these three recovered, three were relieved, and three are still confined to the asylum.

Those who are only relieved, or *in statu quo*, are described as "feeble-minded" (one), epilepsy (one), one who had had an attack at puberty, two who had previously sojourned in asylums, and one with a collateral history of insanity.

The average time before recovery varied from two weeks to three years, the average being six and a half months. The eclamptics, as already stated, tended to rapid recovery, but apart from this disease there is little correlation between cause and length of time in the asylum.

Those patients who remained insane for the longest period, that is for about a year or more, were chiefly those either with no obvious ætiological factors, or only very slight conditions, such as mild pyrexia during the puerperium, while those with a hereditary factor who recovered became normal in less than six months. The details of the variety of insanity were not sufficiently precise to yield any basis for formulating a prognosis. Those described as having mania, melancholia or acute confusional insanity, all varied within wide limits in the duration of the disease and ultimate result.

Summing up our evidence of the prognosis of puerperal insanity, it is possible to say that it first of all depends upon the severity of the organic disease. Half of the patients in the fatal cases died of some variety of infection, while of those cases of infection of which later details are available, half the patients died.

Eclampsia, on the other hand, tends to produce only a brief period of insanity.

The majority, that is 77 per cent. of the survivors, recovered their mental health, though it is probable that many of these might again become insane should they be subjected to another stress such as pregnancy and labour. It is impossible to give any figures of the liability to repetition of puerperal insanity from my notes, but from letters received from patients' husbands and asylum reports, it seems clear that if a patient, having had previous attacks or a collateral history of insanity, becomes pregnant subsequently, she will be prone to become again insane, as happened in five of these cases, but should insanity be only a symptom of a serious organic disease, such as puerperal sepsis or eclampsia, the probability is that it will not recur in a future puerperium.

It has only been possible to produce the few facts I have given you by the kindness and courtesy of the medical superintendents of the infirmaries and asylums. In every instance I have had prompt and detailed replies to my letters of inquiry, which have involved searching infirmary and asylum records for as much as twenty years. I should like to mention particularly Dr. Thane Taylor of the Holborn Institution, Dr. A. H. Bayly of the Lambeth Hospital, Dr. Bendle at Paddington Hospital, Dr. Basil Hood of the Marylebone Hospital, and Dr. Thackray of St. Pancras Hospital. Also Dr. Littlejohn of the Manor Asylum, Epsom, Dr. Beresford of Tooting Bec, Dr. Martin of Long Grove, Epsom, Dr. Porter Phillips of Bethlem Royal Hospital, Dr. Barham of Claybury, Dr. Gilfillan of Colney Hatch, Dr. N. Roberts of Horton, Dr. Spark of Banstead and Dr. Daniel of Hanwell Mental Hospital.

Sir MAURICE CRAIG

said that Sir Charters Symonds spoke of the fewness of the cases of mental disorder following operations; but there was a danger that the surgeon had not the means of judging as to this, as he did not know of cases which broke down mentally after his work had ceased. The mental trouble might not occur until weeks or months after the operation. Only that day he had seen two patients who dated their nervous symptoms from an appendix operation, one five years ago, the other seven; both had appendicular abscesses. His clinical experience had made it clear that people broke down from various causes, no one feature was constant, either in the surgical or the puerperal cases.

He agreed that the influence and importance of heredity could be overstated, but he agreed with the Chairman that there was something in a person's inheritance which helped the tendency to break down mentally. It might be a question of hypersensitivity, which was more important than some would allow. A certain proportion broke down after operation because of the emotional condition following on anxiety. One speaker referred to a patient being of stolid temperament, but without a careful inquiry it should not be concluded that a person was unemotional; the apparent stolidity might be due to the person holding himself in, and he might be thus running certain

risks. Even a child did not talk about what was most important to him ; he often repressed.

He attached great importance to the factor of sleep. Sir Charters Symonds had stated that he had been able to ward off a threatened mental attack by securing sleep to the patient. He (Sir Maurice Craig) was certain that could be done in some cases, but not in all. In early stages of breaking down after delirium, by securing seventeen to twenty hours sleep, by means of a strong dose of paraldehyde, there was no further mental trouble. It was, in his view, very important to combat sleeplessness after operation. Some seemed to consider that the patient must not expect to sleep immediately after operation. Some could put up with that loss of sleep, but others could not. Recent physiological work confirmed the great importance of sufficient sleep.

He was also sure that alteration of blood-pressure was important. At Guy's Hospital he had seen three patients who developed delirious maniacal conditions within twenty-four hours after the removal of a large abdominal tumour ; it might be that a sudden lowering of blood-pressure accounted in some way for the mental condition developing. The mental deterioration was so rapid in some cases that there must be some definite vascular cause to bring it about, particularly where there was no sepsis. It might also account for certain of the puerperal cases. He believed a large number of the puerperal cases might be prevented ; it was very important in a person whose blood-pressure was low to see that the blood-pressure did not undergo a further rapid fall.

It was interesting to hear Sir Charters Symonds speak of the case in which he gave a saline infusion. He (the speaker) had seen cases in which delirium had already been present, and increase of the blood-pressure brought about at once had effected great improvement, but the measure must not be delayed.

In certain types of persons it was very risky to do operations on the nose and ears ; if they were hypersensitive they might, after the operation, pass into a condition of nervous irritability.

In a general way it could be said that in order to avoid post-operative mental trouble it was desirable before operation to investigate as thoroughly as possible the patient's nervous make-up.

In the first case related by Sir Charters Symonds, there was a large mass of fæces, and it might have been a toxic case ; some such cases were of very short duration. And some persons had a mental reaction to protein poisoning. Some people, as soon as they had a collection of pus, became definitely mentally afflicted, perhaps only for a time, and they broke down again as the pus re-collected.

Mr. H. J. F. Simson

said that from Mr. Bourne's contribution it could be gathered, from the obstetric point of view, that there were two classes of cases which gave rise to puerperal insanity. The first was the preventable form, due to sepsis. By far the largest number of Mr. Bourne's cases were in hospitals and infirmaries, and they had probably been infected in the district, and entered the hospital in an infected state. In private practice sepsis was an entirely preventable disease, i.e., puerperal infection, and cases of it ought, by proper institutional treatment, to be non-existent in the future. Post-partum insanity due to puerperal sepsis was therefore a preventable disease.

The important class, however, from the general practice point of view, was that in which there was a hereditary tendency. From the practical obstetrical point of view there was always the question whether these women should be allowed to become pregnant, and he would like to hear evidence on that point: whether it was justifiable, because a woman had had puerperal insanity in a previous confinement, to forbid future children, and, if so, whether the uterus should be emptied. He was sometimes asked to see a patient four months pregnant who had been told that she must never again become pregnant. That was a point which ought to be very carefully discussed, not only by the doctor, but with the husband. It was a difficult matter upon which to advise, because there was a psychology attached to the mentally-unstable woman which, in itself, tended to produce a condition that might give rise to puerperal insanity. And if an obstetrician were asked to empty the uterus of a woman four months pregnant, on account of mental symptoms, it might be his duty to say he could only do that by abdominal section, and that he would do that section and divide the Fallopian tubes at the same time. But to be asked to procure abortion in a woman of that type was, he thought, a very serious matter for a medical man.

Sir Maurice Craig spoke of the importance of sleep, and he (the speaker) agreed that in cases of mental instability when the woman was parturient, one must do all possible to calm her mind and procure sleep for her. There were two occasions during labour when sleep was particularly necessary. One was in the first stage. There was no doubt that the mentally unstable woman suffered more easily from shock than did the normal woman; so that what would be the normal first stage in an ordinary woman would be a severe stage in one who was mentally unstable. Hence the necessity for procuring calm and sleep in the first stage of labour, for it would mean a likelihood of a quicker recovery. At the present day the best means of treatment in the normal woman for producing a quiet first stage was the administration of hyoscine and morphia. What effect, therefore, from the neurological point of view, would those drugs have on such an unstable patient? He well remembered one case in which a woman became mentally unsound. She had a definite neurotic family history, and hyoscine had been given during the first stage of labour. The result as far as her friends were concerned was that the injection of hyoscine produced mania, from which she never recovered. And the condition produced by scopolamine narcosis was very similar to slight puerperal mania. If one used hyoscine in those cases, it should be given in large doses, so as to get the patient well under its influence, and carried beyond the ordinary "amnesic" dose given for an ordinary confinement. But, better than that, he would avoid the use of hyoscine in these cases, and rely on bromide and paraldehyde to procure the necessary sleep. The patient should be kept under the influence of the bromide, or other narcotic, for at least forty-eight hours after the confinement, so as to get well over the shock of the actual delivery.

Dr. Carswell

said that the number of puerperal insanity cases had now greatly diminished; he had rarely seen true infection cases occurring in maternity hospitals; was that because there was now a reduction of risks from sepsis? He did not think it was solely because there were now fewer risks in obstetric practice, but a higher standard was now maintained in the whole care of the patient, particularly as regards quiet and sleep during the first few days after the birth.

Dr. CUTHBERT LOCKYER (President of the Section of Obstetrics and Gynæcology)

said that the great point Mr. Bourne brought out in his contribution was the importance of puerperal infective lesions as an exciting cause of insanity, and he had shown how grave was the prognosis in such cases. In the sixty-one cases he recorded, the aggregate mortality was 22 per cent.; whereas in the cases in which the exciting cause was puerperal sepsis or puerperal infective disease, the mortality was nearly 50 per cent. He thought Mr. Bourne attributed that high mortality largely to the inflammatory or infective process itself.

That suggested to him (the speaker) a practical point. Cases of puerperal insanity were sent to asylums to be looked after by the alienist. He suggested that as the woman went into an asylum to be treated for insanity, she was still also a puerperal case; therefore, would it not be well to follow Mr. Bourne's advice and collaborate, have a skilled obstetric physician attached to the staffs of mental hospitals and asylums, so that the responsibility of looking after these cases might be shared by the obstetrician and by the alienist? He believed that idea had been followed out at the Maudsley Hospital; Sir Frederick Mott gave him that impression. It would be, he (the speaker) thought, to the advantage of the community if that idea were put into practice more universally. It was known that eclamptics got well; but what interested him was the large number of primigravidæ who suffered from puerperal insanity. Therefore it looked as if the emotional side sufficed, in itself, to account for this temporary confusional mental disturbance. He would like to hear the Chairman's (Dr. Hyslop's) view on the idea of collaboration between obstetric physicians and psychiatrists in the asylums of this country.

Mr. ALBERT CARLESS

said it was now over thirty years since he commenced operating, and his experience coincided with that of Sir Charters Symonds in that he had seen very few instances of post-operative mental disorder.

He would wish to add his testimony to the opinion expressed by Sir Maurice Craig as to the stress and strain of operations. He (the speaker) thought there would be definitely fewer instances of nervous breakdown after operations if surgeons were not in such a hurry to get their patients out of nursing homes and hospitals. He had himself recently undergone the experience of having his own appendix removed, and he was sure his convalescence was much hampered and delayed by his being obliged to leave the nursing home fifteen days after the operation. His own practice had always been to detain the patients three weeks at least after an abdominal operation, and he had no reason to believe it was wrong. The practice of some surgeons of getting the patient up in a week or eight days must prejudice their ultimate recovery.

The most vivid case of insanity associated with surgery he had seen was one in which the operation had never been performed by the surgeon. He did not perform the operation, but the patient did. During the war he was called down into the country to perform gastro-enterostomy on a private case. Some military duty caused a postponement of the operation for twenty-four hours. The patient worried much about it and had a sleepless night, unassisted by a sleeping draught. Being a nervous man, this preyed on his

mind and upset his equilibrium, so that in the morning he proceeded to cut his throat, and the speaker took his fee for stitching up his throat instead of connecting the stomach to the small intestine. That was a preventable accident, and emphasized the point that a surgeon needed to consider the individual and not merely the local condition, and until surgeons studied more the character and mentality of their patients and took a larger view of the whole matter, such occurrences as were now being discussed would persist. In reference to Dr. Lockyer's proposal as to putting an obstetric physician on to the staff of the mental hospital, he would suggest that a mental specialist might be added to the staff of an obstetric hospital with advantage.

Dr. Hyslop (Chairman),

in concluding the discussion, said it would be agreed that it had been widely varied, and of great interest. It must be recognized that the patient might be what might be termed a saturated solution of nervous instability, and a small incidental factor was enough to set light to a morbid strain of thought and a definite psychosis. The severity of the operation did not matter, because such a small matter as the extraction of a tooth might produce a psychosis in such people. Clinically, one had often been asked whether a patient was sufficiently well to have a tooth out, or whether there was any danger in it.

One side which had not been discussed referred to the beneficial effects of operation on insane patients—general operations, not necessarily those on the brain or nervous system. Those who had asylum experience knew many examples of strangely divergent bodily affections which had been no part of insanity, but by the treatment of which enormous benefit to the mental state had been effected.

With regard to hyoscine, he was rather prejudiced, as he had been in many asylums where the hyoscine method of producing artificial sleepiness was used only on definitely incurable cases. One would not give it except in cases of urgent need, and not to a patient who seemed to have a chance of recovery. He did not use hyoscine once in two years, and even then he did not give more than one or two doses. It enabled one to knock the patient over, but the difficulty of recovery afterwards was very great. One also knew what was the effect of administering morphia in mental cases.

He agreed that the more co-operation there was in regard to the diagnosis of these conditions the better, and the more was the profession likely to receive enlightenment. Hospitals which were in close proximity to teaching centres could readily obtain the services of gynæcologists; for instance, Bethlem Hospital in London, Morningside Asylum, Edinburgh, Gartnavel and Cheadle: but the matter was different in hospitals situate far away from centres. Visits of gynæcologists under these circumstances would be very expensive, and it became a matter of practical economics as to whether the maintenance rate could cover this. Throughout the continent of Europe and in America the practice was gradually spreading of having gynæcologists associated with the large mental hospitals. But if a start was made with one specialist, where was it to end? However, it was certain that such discussions as this must produce good results, even though the present debate had been rather more diffuse than he had hoped, perhaps because it had not been opened by a special paper.

Section of Medicine, Section of Pathology, and Section of Therapeutics and Pharmacology.

JOINT MEETING.

Chairman—Professor A. J. CLARK, M.D. (President of the Section of Therapeutics and Pharmacology).

DISCUSSION ON "THE USES AND LIMITS OF VACCINE THERAPY."

Sir ALMROTH WRIGHT[1]

said he would begin by drawing attention to the logical basis which must underlie the consideration of vaccine therapy. Conclusions could be reached as the result of two processes—namely, (1) direct inference, (2) indirect inference. The former process involved deductions from precisely similar cases, and was not possible in vaccine therapy, because, in addition to the simple question, "Will a definite dose of a standardized vaccine do good?" it was necessary to have a complete immunological diagnosis of the patient, including the exact nature and extent of the infection, and the degree of response of the patient. These were variable, and so direct comparison and direct inferences were not possible. The process of indirect inference was not generally popular, being described derogatorily as theorizing, and yet in the past it was through this process that knowledge had been gained—as, for instance, in the case of those who had first sailed round the world, or of Ambroise Paré, who deduced the method of stopping bleeding by ligature, which was an improvement on the cautery. So Pasteur and Einstein were compelled to demonstrate the truth of their theories, which had resulted from indirect inference, before these could win general acceptance. In the search for the verdict as to whether vaccine therapy would do good, attention should be paid to indirect rather than to direct inferences in the first place. Sir Almroth Wright proceeded to discuss some facts bearing on immunization, the deductions to be drawn, and the theories which were concerned. In the matter of dosage, although no precise details of comparable cases were available, yet two facts had an important bearing on the question. A healthy man would give a certain response to the injection of a large dose of bacteria. An unhealthy man, with these bacteria already in his system, gave a like response to a much smaller injection of bacteria. The deduction to be drawn was that an infected man needed a smaller dose of vaccine than would be inferred from the case of a healthy man, and hence it was necessary to determine the dimensions of his infection in order that the vaccine dose might be proportionate. Moreover, if the

[1] This report has not been revised by Sir Almroth Wright.

dimensions of the infection were increasing, it was clear that a time would come when the bacterial dose already in the system of the patient would become too great to elicit a natural response. In such a case the addition of protective substances from outside became necessary—that is, passive rather than active immunization. Passive immunization had been long familiar as the basis of serum therapy, but was not yet perfect, since standardization of the protective substances present in any serum had not yet been accomplished, though good progress in this direction was being made. The newer process of immuno-transfusion had definite limitations, since the amount of protective substances present in that portion of the blood of the donor which was available for the recipient was obviously limited. The truth of the old idea that antibodies were specific had always been doubted by the speaker. Laboratory experiments were now indicating that inoculation with one species of microbe protected against others, and it began to appear likely that any one would protect against any other. This was a hard doctrine for some to accept. In the case of immuno-transfusion there was no need to wait for more than a few seconds after inoculating the donor before collecting his blood; antibodies were already present in it. These new substances could not be produced in this short time *de novo*, so they must have been already present somewhere, and have been freed by the injection. The fact that about ten days were required for the production of specific protective substances indicated the existence of more than one class of these antibodies. In prophylaxis these specific antibodies were more important than the non-specific, but in treatment the reverse was the case. The crisis of pneumonia was perhaps produced by an accumulation of microbes reaching a critical dose and causing active immunization; it was conceivable, therefore, that an attack of pneumonia might be cut short by giving a vaccine injection on the first or second day. The case of tuberculosis treatment had been very difficult hitherto, but it had been found out recently that it was possible to secure growth of these bacilli in the blood. The removal of white corpuscles accelerated such growth, whilst addition of white corpuscles retarded it. The addition of tuberculin impeded growth also. Septicæmic blood had reduced bactericidal power, and the analogy probably held good in tuberculosis. Yet in chronic tuberculosis the blood developed a greatly increased immunizing power, and the question arose whether the treatment by vaccine injection in such cases was advisable, since the process seemed already to have been pushed beyond the limits of response. Perhaps in such cases the right treatment was the introduction of protective substances into the foci of infection. In conclusion, Sir Almroth Wright doubted the value of statistics as a means of judging the results of treatment by vaccine therapy. They were too vague, and omitted any consideration of such important details as the variations in different patients and in their treatment, nor were there any definite criteria of results, other than very general results, which did not clash with each other. He cited the typhoid statistics collected during the South African campaign and the recent war as an illustration of the fact that statistics when conscientiously elaborated, were valueless as criteria of results of treatment. Experimental results, rather than statistical evidence, determined the employment of any particular treatment, and conclusions were drawn from individual cases and laboratory discoveries. The demand for statistics was bad, especially when coming from an intellectual profession.

Sir WILLIAM LEISHMAN,

after commenting on Sir Almroth Wright's remarks on the statistics of typhoid inoculation in the war, said he did not think that any reasonable person could be left under a shadow of doubt as to the enormous debt the State owed to typhoid inoculation, and to Sir Almroth himself.

Although the title of the discussion appeared to exclude a consideration of the prophylactic employment of vaccines, he pointed out that the therapeutic use of vaccines owed much to the knowledge obtained in connexion with preventive vaccines and, further, that the latter had often a very definite therapeutic value in profoundly modifying the severity of an attack where the immunity had fallen short of complete protection. He instanced the mild nature of attacks of the enteric fevers in the inoculated and the rarity and comparative mildness of the secondary pulmonary complications of influenza in men who had received mixed influenza vaccine containing, in addition to Pfeiffer's organism, immunizing doses of streptococci and pneumococci. On account of this he was still in favour of the employment of the latter vaccine in times of influenza prevalence, in spite of the fact that the weight of evidence now pointed to a filterable virus as the cause.

As to vaccine therapy in septicæmias, he mentioned his experiences in France, where he had expected from it, in the case of enteric fevers, better results than were achieved. A careful examination of a series of cases in uninoculated men at one of the base isolation hospitals by Captain Whittington, showed no demonstrable benefit, but it might be pointed out that this was against the bulk of the recorded experience in other countries.

Although he could not lay claim to much recent first hand experience, he thought it might be of some interest if he recorded his personal views on some points connected with vaccine therapy with regard to which there was lack of unanimity.

Of modifications in the method of preparing bacterial vaccines there was no end, and high claims had been made in succession for the use of sensitized vaccines, sero-vaccines, lipo-vaccines, de-toxicated vaccines, diaplyte vaccines and others. All modifications had the same end in view, the obtaining of the active immunizing principle of the bacillus in an undamaged and stable form, and as free as possible from toxic or other elements which played no useful part in immunization. While admitting freely that great improvements might be possible, he did not himself feel convinced that any of those methods he had mentioned presented any striking advantages over the older methods of simple sterilization by heat, or perhaps by an antiseptic agent.

The subcutaneous channel of introduction he still considered preferable, though perhaps Besredka's brilliant researches on the oral method might give results in practice superior to those obtained by the older method; so far, he did not consider the published results on the prevention side an adequate demonstration of such superiority.

He was not a convinced advocate of the employment of autogenous vaccines. Apart from the consequent delay and the hurried nature of their preparation and standardization, he had observed little evidence of their pronounced superiority as compared with the use of a stock vaccine composed of a strain or strains which had been fully tested and which had been deliberately chosen for their superior antigenic properties.

In conclusion, he urged that, in assessing the results of vaccine therapy,

and especially its failures, weight should be attached to the extremely unfavourable conditions under which, from the bacteriologist's standpoint, it was often carried out. The latter had frequently no certainty that the material which was sent him had been collected under proper conditions, while, if long in transit, or perhaps exposed to undue heat or cold, sensitive organisms might have died out, and thus some hardier saprophytic organism which survived might be made into a vaccine. Such a vaccine might be used, and was probably not infrequently used, although the organism in question had nothing whatever to do with the patient's ailment. Again, the lack of sufficient information about the cases, and of the reactions and results given by the vaccine when used, were very serious handicaps to the bacteriologist who was asked to prepare it and to advise upon the size and frequency of dosage. He pleaded for a much more frequent and intimate association between physician, patient and bacteriologist.

Dr. William Gordon.

The point which I wish to bring forward is that the usual dose of streptococcal vaccine is excessive, at least in acute cases, and is sometimes dangerous. I shall only refer to five cases. All were acute. In all an autogenous vaccine was used.

The first was that of a man with streptococcal septicæmia. He was given a $\frac{1}{4}$-million dose at first and no bad effects followed. After forty-eight hours a $\frac{1}{2}$-million dose was given and, about four hours after it, he had a syncopal attack from which he was brought round with the utmost difficulty.

The second was that of a medical friend under treatment for a streptococcal infection of the lung. A dose of a $\frac{1}{4}$ million caused no bad effect. But, warned by what happened in the first case, I waited seventy-two hours, and then simply repeated the $\frac{1}{4}$ million. About four hours after it he had an alarming attack of syncope. I naturally stopped the vaccine. But, as, after a few days, although recovered from his syncope, he was obviously going downhill, the temperature ranging from 100° to 102°, I put it to him whether he would risk a much smaller dose, as the other doses had lowered his fever. He agreed, and I gave him 40,000. The temperature came down slightly and there was no bad symptom. I repeated this dose every four days, with the same result. After about two weeks, the temperature became normal and he recovered.

The third case was that of a lady with streptococcal dermatitis of the left leg, with threat of peritonitis. A $\frac{1}{4}$-million dose caused a good deal of malaise, so doses of 40,000 were given every four days with excellent results.

The fourth case was that of a girl, whose leg was similarly affected, but who had actual peritonitis and pneumonia. Here also after an injection of a $\frac{1}{4}$ million, which produced much malaise, doses of 40,000 every four days gradually brought about her recovery.

The last case was that of a doctor with streptococcal pleurisy and high swinging temperature. 40,000 every four days quickly brought it down and he is now well.

I know that larger doses than any I gave are in common use, and I have not noticed that such small doses as 40,000 are being given. Therefore, I hoped you would consider me justified in bringing these facts to your notice.

Dr. A. P. BEDDARD.

I propose to give so far as I can a summary of what I think I have seen or have not seen as a result of the use of vaccines. Unless otherwise stated, when speaking of a vaccine I am referring to an ordinary non-sensitized killed autogenous vaccine given by subcutaneous injection.

The two main uses of vaccines have been (*A*) prophylactic, (*B*) curative treatment of infected patients.

(*A*) *Prophylactic.*—The best known example is that against typhoid. I do not propose to say anything about it here; its value has been proved: I have no personal experience worth mentioning. In England the chief prophylactic use of vaccines is obviously in catarrhal condition of the respiratory tract.

It is not easy to summarize results and impressions. One sees good results; one sees complete failures. In some cases the acquired resistance, if any, appears to last but a short time: in other cases it appears to last for many months or even years.

Can one go further than this in attempting to sort out these cases?

My impression is that the more nearly the patient is really a chronic carrier of the infection the more difficult is it to get any good result at all.

(1) Take, on the one hand, an otherwise healthy person who every winter gets several bad colds, from each of which he recovers completely. In such a case four or more large injections of a mixed stock catarrhal vaccine given in September at intervals of ten to twelve days may produce an excellent result.

In dealing with these patients it is obviously necessary to remember two things: (*a*) That they may meet the infection directly after the injections. The danger of this is obvious. (*b*) That relying on their added immunity they often take considerable risks afterwards.

(2) At the other extreme one meets with patients who get one feverish attack after another, it may be throughout the year. In between these attacks they may be constitutionally well but they are never free from symptoms of catarrh. They are in fact, more or less chronic carriers. In them the method of giving a few large injections (*a*) not only yields generally no good results but is (*b*) unsafe.

The impression of these cases is that: (*a*) It is necessary to begin with quite small doses, in fact, a curative line of treatment before one attempts large prophylactic doses. (*b*) A mixed autogenous vaccine is superior to a stock vaccine. In passing, it may be remarked that it is necessary to ascertain the bacteriology of both nostrils, pharynx and sputum. (*c*) The longer the patient has been in the condition of a chronic carrier the more difficult is it to give him any added resistance. (*d*) In order to get a really good result it is necessary to free them from attacks for about four years on end, otherwise they will promptly relapse. (*e*) That in any given one of these cases it is impossible to *guarantee* anything from vaccine therapy.

Another prophylactic use of vaccines is the attempt to prevent a blood infection as the result of surgical interference with some infected area, e.g., a child has recently had acute endocarditis or acute nephritis as a result of tonsillitis. The tonsils are pitted and septic, and have to be enucleated. This cannot be done without producing a rather sore septic throat, the risk of blood infection and a fresh attack of endocarditis or nephritis, which may be serious. It is not easy to say of how much value prophylactic vaccination is in such a case; one's impression is that it must be of value. The main objection is that it will defer the operation by about three months, because it would take that time to work up the necessary resistance.

(*B*) *The curative use of vaccines* covers a far greater variety of cases which might theoretically be treated by this method.

It is manifestly impossible to consider every kind of case. From the point of view of this treatment one can distinguish:—

(I) Acute cases with high pyrexia and constitutional disturbance.

(II) Less acute cases of acute infection and chronic cases, with, of course, every conceivable grade in between the two extremes.

(I) As regards acute cases with considerable constitutional symptoms such as pneumonia one imagines that most people agree that (*a*) during the acute septicæmic stage vaccines as ordinarily given are not only useless but dangerous; (*b*) that the more acute the symptoms the more dangerous are such vaccines. In such cases vaccines might be tried at two stages (*a*) before constitutional symptoms were severe; (*b*) when they have begun to pass off; (*a*) before the symptoms are severe in an attempt to *abort* the acute attack, e.g., in pneumonia one has seen a good many cases in which 25 to 50 m. stock pneumococcal vaccines have been injected subcutaneously in the first forty-eight hours. I have never seen obvious good done and am not prepared to say that harm has not been done. But that it is possible to abort such infections I have seen in one convincing case, that of the wife of a doctor who has had a series of attacks of streptococcal pneumonia ushered in by an easily recognizable train of symptoms. It has been found that these attacks can be stopped every time by at once injecting subcutaneously two hundred thousand of an autogenous streptococcal vaccine, followed in twelve hours by double the dose. It is possible that the good result in this case is due to the promptness with which an autogenous vaccine is given, and to the accuracy of the dose appropriate to the case being known beforehand; of the two probably the question of dose is the more important. (*b*) When the severe symptoms have more or less passed off. It might be asked, if the patient is recovering spontaneously why give a vaccine? There are occasions however when a vaccine appears to be of value, e.g., uncleared lobar pneumonia complicated or not by recurrent venous thrombosis, arthritis, fibrositis, &c., or a broncho-pneumonia in which the temperature will not quite settle down to normal and there is a tendency to minor relapses. In passing, one may note that patients in this stage are extremely sensitive to vaccines in that dosage and interval must be very carefully considered, otherwise harm is done.

If these vaccines as ordinarily given are not of frequent applicability to the treatment of the acute stages of infection one might imagine that they would be often valuable in the treatment of less acute cases and chronic cases. This, however, is hardly borne out by experience.

(II) *Less Acute Cases.*—In a majority of these the patient is curing himself and requires no assistance. There are obviously, however, low grade septicæmias, e.g., gonorrhœal rheumatism, rheumatoid arthritis, osteo-arthritis, recurrent venous thrombosis, fibrositis, neuritis, &c., in which vaccines are of value provided they are combined with adequate local treatment of the site of invasion—teeth, sinuses, tonsils, urethra, uterus—whatever the site be.

(III) In regard to *chronic cases* serious limitations to the value of vaccines must have struck everyone who has used them much. (*a*) The more chronic the case the more difficult or impossible is it to stir up the patient's body by a vaccine to kill out the infection. What has gone wrong is not obvious, but it is as if the patient's immunity apparatus were no longer capable of responding to the stimulus. To take a simple local disease, e.g., chronic

acne, many cases are quite unaffected by vaccines. Of chronic joints very often the same is true. An extreme example of the same thing is the chronic carrier. Recent carriers, e.g., of *Bacillus coli* pyelitis, where the original attack has been mild and where the immunity apparatus as it were has never been thoroughly stirred up, are as a rule readily curable by a vaccine. But who can be more incurable than a chronic carrier of *Bacillus coli* in the urinary tract?

(*b*) The second type of case which marks a serious limitation to the value of treatment by vaccines is where the infection is situated in one of the *tubes* of the body: (1) Alimentary canal; the respiratory tract to less extent. (2) Female generative tubes. (3) Urethra and urinary tract. If, added to the fact that the infection is in a tube, it is also chronic, cure by vaccine however long continued or skillfully given is unlikely. If, in addition, there is any mechanical factor of partial obstruction the attempt is foredoomed to failure until the mechanical factor has been removed. Take a straightforward infection of the guts with *Streptococcus longus;* it is extremely difficult to put an end to this by vaccination, however long continued. In streptococcic ulcerative colitis, in spite of vaccines, relapse is almost inevitable. In *Bacillus coli* infection of the urinary tract vaccines appear to have little effect in curing the attack or in preventing relapse. I have not personally seen any patient's urine sterilized by vaccines who had had the infection for much more than about two years on end. In passing one may note how useless vaccines are in the treatment of infections of the endocardium.

A word about the *different kinds of vaccine* just to raise the question:

(1) Personally, I have nearly always used autogenous vaccines when available; whether ever, and if so when, one should deliberately use a stock vaccine in preference to an autogenous vaccine, I do not pretend to know. Whether a stock vaccine should always be polyvalent, as used in gonorrhœa, I also do not know.

(2) *Sensitized vaccines* are certainly less toxic than ordinary vaccines as shown by the larger doses which the patient can stand. But whether they really offer any advantage commensurate with the greater trouble and expense which they entail, I do not know.

(3) *De-toxicated vaccines*, i.e., de-toxicated by chemical means and not by culture, produce, one imagines, an active non-specific immunity, comparable to that produced by the injection of milk or peptone. What the exact indications, if any, are for deliberately producing a non-specific in preference to a specific immunity, again I do not know.

Difficulties.—There are many difficulties in the use of vaccines. I shall only mention two:—

(1) *Hypersensitiveness of the patient to vaccines.*—Some patients one knows beforehand will probably be very sensitive, e.g., in uncleared pneumonia. But occasionally one meets with quite a chronic case, e.g., osteo-arthritis, in which an injection of as little as 2 m. of their vaccine produces not only a marked local reaction but great focal reaction, and even constitutional disturbance. Some ways out of the difficulty are: (*a*) use of a stock or sensitized vaccine instead of an ordinary autogenous vaccine. (*b*) By giving minute intravenous injections of their vaccine at intervals of a few minutes comparable to the method of desensitizing a patient to horse-serum. This is often successful. (*c*) Of the method of desensitizing a patient by the injection of increasing daily doses of peptone I have no experience.

(2) The second difficulty is where the infection is situated in certain organs, especially the eye, pancreas, kidney, myocardium. The difficulty is that any

dose of the vaccine above the subminimal is followed by such a severe but temporary focal reaction that one hesitates to go on producing increased, although temporary, reaction in these delicate structures.

Many of the failures ascribed to vaccine therapy are more or less avoidable.

(1) The full or correct bacteriology of the case may not have been ascertained. All too often one finds it assumed that because some pathogenic organism has been found in some part of the body that that bacillus is the infecting agent and the cause of the symptoms. Unless the case is obvious it may take a long or widespread bacteriological hunt to find the real infecting bacillus which may be recognized by serum reaction or agglutination tests.

Sometimes the trouble is all the other way, and it is the very wealth of bacilli found which make it difficult to select the true infecting agents.

(2) The dose interval may be wrong. Doses are, as a rule, selected by rule of thumb, experience of apparently similar cases and infections, and by watching the effects of each dose. In general, it is found that one is using smaller doses than formerly. Personally I always try to avoid constitutional disturbance when using vaccines curatively and look upon marked local or focal reactions as meaning that the dose given was a very full one.

In chronic cases although large doses can be given with apparent impunity, it always seems to me that a patient's immunity apparatus can be overstrained by the colossal doses over long periods which are sometimes given.

It is in the more acute cases that the size of the doses, and especially of the initial dose, is so difficult to guess. The modern methods which suggest that it is possible to fix the appropriate dose beforehand in an acute case are of very great help clinically.

(3) Too much is expected of the method: a few doses are expected to cure a chronic case. In chronic cases I am sure that this is a frequent cause of failure and disappointment. Whilst it is true that harm may be done by very large doses given over long periods, there is to my mind no justification for the dread of harm necessarily following, if more than about twelve doses of vaccine are given. It is possible to vaccinate a patient with a medium fixed dose for years without harm, e.g., a woman who had had recurrent attacks of bronchial asthma for years after being vaccinated curatively received a fixed dose of 200 m. of her streptococcus and Friedländer's bacillus every fortnight for four years with an interval of about three months every summer. That is nearly five years ago; she has not yet relapsed.

(4) Neglect of local treatment and of mechanical defects. A urethra may have to be treated, or teeth removed, or tonsils enucleated, or antra drained, &c., if a vaccine is to be of any value. But when all these avoidable causes of failure have been enumerated there still remains the fundamental fact that, clinically, there are a very great number a cases of infection by bacilli, either very acute or very chronic, in which some curative method of treatment is really required, and that treatment by vaccines as at present ordinarily carried out is inappropriate to the one class and most uncertain in the other.

If Wright's newer methods of tackling the acute cases fulfil their promise he will have pointed the way to yet another great advance in treatment associated with his name.

Dr. NORMAN MCCASKIE.

As I am in general practice and have no special knowledge of bacteriology, I suffer from the disabilities of the general practitioner in contributing to a a discussion of this kind. We cannot employ controls, and we cannot treat

one series of cases with vaccines and vaccines alone, and another series by other means. We see fewer cases of disease in individual patients than a physician attached to a hospital, and therefore our experience is limited. Our notes are not primarily designed as a record of a series of cases, and we have to trust to our impressions of any line of treatment rather than to statistics. Therefore all I can give you is the sum of my own personal impressions.

The use of vaccine falls into two categories: (I) The prophylactic; (II) the therapeutic.

(I) The most important *prophylatic vaccinations* are against small-pox and against typhoid and paratyphoid fever, and their value is unquestioned.

The chief use which I make of other prophylactic vaccines is against infective catarrhs, and this is the only one which I shall mention. I advise patients who are always catching colds, and those whose colds always end in some complication such as laryngitis or bronchitis, to try the effect of protective inoculation. After excluding any local disease in the throat, nose or in the accessory sinuses, I examine a specimen of the sputum or of the nasal mucopus. As there are generally several organisms present, any one of which may overgrow the others, it is important that the specimen should be incubated as soon as possible after it is obtained, to prevent the more delicate organisms from dying out, and for this reason the specimen is best collected in a bacteriological laboratory. Even with this precaution the harmful organism may be overgrown, as in the case of a septicæmia, in which the *Staphylococcus citreus* was recovered from the blood, but did not grow in the culture obtained from the pharynx.

If the culture shows a growth of the ordinary catarrhal organisms such as *Micrococcus catarrhalis*, pneumcocci, *Bacillus influenzæ*, streptococci and staphylococci, I give three injections of a mixed stock vaccine in graduated doses at a week's interval. There is rarely any unpleasant result, except a slight stiffness at the site of injection for a couple of days, and occasionally a slight focal reaction. I like to do this early in autumn before the annual catarrhal epidemic in October, and as the immunity does not seem to last for more than six months, I do it again in spring for patients who catch colds in summer. If a stock vaccine fails I try an autogenous vaccine, or add autogenous to stock vaccine, but I have not had such good results.

On the whole the results which I have obtained are satisfactory, though I do not find the explanation easy. How is it that the actual infection may give such a much shorter immunity than the inoculation?

In a few cases I have seen *complete* immunity; in others *partial*, i.e., colds clear up quickly without complications and fewer colds are caught. Some patients tell me that they have abortive colds; they feel all the symptoms of a cold beginning, but it clears up in twenty-four hours without developing further. Several dozen patients come to me every year because they are convinced that inoculation has been a success in their case. In others it is quite useless.

I have only once seen a case in which I think harm resulted, and that was in a child aged 9 years subject to bronchitis, who had her worst attack a week after her third inoculation, but this may have only been a coincidence.

In my experience the best results have been obtained in the first year, and inoculations in subsequent years have lost a little of their efficacy. I am convinced that it is helpful to a great many people and is well worth trying.

(II) *Therapeutic use of Vaccines.*—I have had little experience of the use of vaccines in *acute* infections, and though I have heard of successes I have

not experienced them myself. On the other hand I have used vaccines extensively in the treatment of *chronic* diseases, and the following is an enumeration of the results of such treatment:—

(1) In chronic infective arthritis, after the removal of any obvious source of infection, I have had successes, and my impression is that those cases especially in which a streptococcus can be recovered from the fæces, have been improved by a vaccine combined with other general and local treatment. In arthritis of oral origin one would expect good results from vaccines, because one often sees a focal reaction in the joints after the extraction of septic teeth, comparable to the reaction after a dose of vaccine. I must say, however, that I have been disappointed, though I remember one case in which a rapid and complete recovery occurred during the course of administration of a streptococcic vaccine after the removal of two septic crowns. The psychic element enters into the treatment of chronic arthritis, and the feeling of hope that something definite is being done to remove the cause of the disease is of value in all these weary and depressing cases. Vaccines from bronchial, antral, nasal and urinary infections have been disappointing in the treatment of arthritis so far as I have seen.

(2) In *Bacillus coli* infection of the urinary tract, especially after pyelitis, I use a vaccine when the acute stage is over, because I am advised by authority to do so, but the patient gets better independently of this, and I have not seen relapses occur more frequently in cases not treated by vaccines.

(3) In staphylococcic infections I used stock and autogenous vaccines for years and found that they generally cleared up the condition, but that there was a liability to recurrence. For some time past I have used manganese, and my impression is that the immediate results are equally good, and that the recurrences are fewer.

(4) In chronic bronchitis, my results have not been satisfactory, though in one or two cases, in which streptococci have been present, I have seen improvement.

(5) In pulmonary tuberculosis I have found no benefit result; and cases of surgical tuberculosis, so far as I have seen, have not been affected one way or the other. I have had no experience of Dreyer's vaccine, nor have I used de-toxicated vaccines in any of the infections referred to.

(6) In nasal infections, in gonorrhœa and in gonorrhœal complications, in colitis, in pyorrhœa—except in cases of marginal gingivitis, which get well with local treatment anyhow: in acne, except that the superadded staphylococcic infection is improved, I have not seen any good results that I could attribute to vaccine treatment.

In all these conditions, we have only our clinical impressions to guide us, because the opsonic index is not the criterion of value that we once thought it to be.

There are fashions in treatment, and few of us who have been in practice for over twenty years can look back with quite clear consciences on our attitude towards them. Vaccine therapy to-day is the fashion of the moment. It has captured the general public: our patients expect it, and reproach the man who does not employ it as being behind the times. This reacts on us, and there is hardly a bacterial infection in which we feel that we have exhausted the last means of treatment unless we make use of a vaccine. It is fatally easy—and very profitable—but it lends itself to exploitation. There will be a reaction, and a prejudice will arise against a method of treatment which, though yet in its infancy and hardly as yet of general application, holds great promise for the future.

The practitioners who have a culture of colon bacilli prepared from normal fæces, and inoculate their patients week after week, month after month, perhaps year after year, for every disease under the sun, may do so in good faith, as I hope, but they bring discredit on the whole question of vaccine therapy and incidentally on the good sense and intellectual honesty of our profession.

I am convinced of the *prophylactic* efficacy of vaccines, but to speak frankly I am disappointed by my own personal experience at the results of their *therapeutic* use. What strikes me is the uncertainty of their action. One vaccine is a success and another, made in precisely the same way, is a failure. It may be that there is something in the preparation of which we are ignorant: it may be that an ultramicroscopic virus, which is really the effective agent, is only sometimes included: it may be that there is some unknown factor which one day will be disclosed.

I may have been unfortunate, but I have not yet met with clear and unequivocal results, due to a vaccine and to a vaccine alone, at all comparable to the sure and definite action of quinine in malaria or of mercury in syphilis.

Dr. Richard Armstrong.

I have been interested during the past year in an experimental investigation of the immunity response of laboratory animals, particularly the rabbit, to the inoculation of sensitized and unsensitized, or as I prefer to describe it, "raw" type 2 pneumococcus vaccine.

A method of accurate estimation of the quantity of protective antibodies in the serum of immunized animals has been elaborated, the essential feature of which is the employment of mice in the estimation of the protective power of immune serums against known quantities of virulent pneumococcus culture.

There is no time and this is not the place to describe laboratory experiments in detail. I propose to ask you to take for granted the accuracy and reliability of the methods employed and to demonstrate their results to you. I have been concerned with the study of the gross immunity developed by an animal after treatment. This has been determined by quantitative estimation of all the antibodies present in samples of serum collected at intervals during and after immunization.

I have not confined myself to the study of a single antibody group such as agglutinins, complement-fixing bodies or opsonins. I believe, therefore, that my results have practical bearing on the problem of the use of sensitized vaccines. The main results of the investigation may be represented diagrammatically. (Charts were then thrown on the screen by the epidiascope and demonstrated.)

The degree of immunity induced in rabbits at successive intervals of time after the inoculation of single equal doses of raw and sensitized vaccine may be summarized briefly as follows:—

(1) Sensitized vaccine, inoculated intravenously, exerts, in virtue of its antibody charge, an immediate passive immunizing effect which becomes exhausted within twenty-four hours. A definite phase of subnormal immunity then becomes apparent, lasting more than twenty hours, to be succeeded by a slight rise in immunizing power between the second and third days after inoculation. In the course of the sixth day actively produced immunity develops with dramatic suddenness and reaches a high level.

(2) Sensitized vaccine inoculated subcutaneously—the route most commonly employed in clinical practice—also sheds its antibody charge, and

confers passive immunity. This effect is exercised slowly, and does not become fully apparent until after twenty-four hours have elapsed. After that immunity is maintained, at a level equal to that at first conferred by the intravenous route, for upwards of three days. A brief period of subnormal immunity is apparent on the fourth day; on the sixth day serum samples show high protective power, surpassing that of intravenous inoculation and equalling the high immunity which succeeds inoculation of raw vaccine.

(3) A prolonged period—upwards of three days—of subnormal immunity follows inoculation of raw vaccine intravenously. Protective bodies then appear suddenly in high concentration in the serum, rising to a maximum on the sixth day.

(4) The level of immunity in the serum of rabbits inoculated with sensitized vaccine rapidly declines after the sixth day, and is reduced to a low level by the twelfth day. On the other hand a high level of immunity persists till the twelfth day after inoculation with raw vaccine.

(5) The culmination of the immunizing effect of vaccines on the sixth day appears to afford a sufficient explanation of the occurrence of crisis in lobar pneumonia on this day.

It seems reasonable to conclude that, on inoculation, sensitized vaccine parts from the whole of its antibody charge, and afterwards functions in a manner similar to raw vaccine.

In another set of experiments rabbits were inoculated intravenously at intervals of forty-eight hours with five successive increasing doses respectively of sensitized and raw Type 2 pneumococcus vaccine.

Immunity in response to raw vaccine appears on the third to fourth day. It rapidly reaches a very high level attaining a maximum on the sixth day after the first dose of vaccine and thereafter persists at a high level. On the other hand, after administration of repeated increasing doses of sensitized vaccine, not only is the appearance of actively produced antibodies delayed until the eighth day or later but the degree of protection is greatly inferior to that which follows administration of raw vaccine. No passive immunity is apparent.

The result is unexpected and may perhaps be explained as being due to reciprocal neutralization of the passive immunity conferred by each dose of sensitized vaccine by the phase of sub-normal immunity which follows the preceding dose of vaccine. Hence the bacterial antigen exerts little or no stimulating effect on the animal and the production of active immunity is correspondingly delayed.

These observations, which have so far been confined to prophylaxis, suggest that the clinical use of repeated doses of sensitized pneumococcus vaccine is actually likely to produce deferment of the desired protection of the patient. The method of choice in acute pneumococcus infections would appear to be the administration of a single large subcutaneous dose of sensitized vaccine early in the disease, sensitized vaccine being preferable to raw vaccine in that the phase of sub-normal immunity appears shorter in the former.

So far from the subject of vaccine therapy being exhausted, I suggest that we are only approaching its outer limits and I believe that the disappointments which have at times attended administration of vaccines have been due to an incomplete understanding of the mechanism of immunity response.

Sections of
Medicine, Neurology, Ophthalmology and Otology.

JOINT MEETING.

DISCUSSION ON VERTIGO.

Chairman—Dr. ROBERT HUTCHISON (President of the Section of Medicine).

Sir HUMPHRY ROLLESTON, K.C.B.

Vertigo has recently been defined by Isaac H. Jones as "a subjective sensation of a disturbed relationship of one's body to surrounding objects in space." Sydney Scott [25], in his able critical review, following Hughlings Jackson, goes more deeply, though equally briefly, into the matter by describing vertigo as "a state of consciousness of the effect on motor centres of a want of harmony between afferent impressions." Such a definition may be taken to include disturbances of the whole vestibular system from its end organ in the labyrinth through Deiters' nucleus and the cerebellum to the cerebral cortex, however caused, whether by organic lesions or by toxins acting directly on nervous tissues or indirectly by inducing vascular spasm or dilatation.

The afferent impressions are in the order of their importance: (1) vestibular, (2) ocular, and (3) kinæsthetic; muscular, articular and visceral sense of movement.

There must, of course, be some reservations to any general rule that the presence of an aural lesion in a case of vertigo proves that this symptom is solely due to ear disease. For example, a tumour of the auditory nerve or of the cerebellum in a patient with chronic otitis would explain vertigo without any need to consider the influence of concomitant ear disease. It is conceivable that an aural lesion may remain inactive as regards the production of vertigo until some further factor, such as toxæmia, comes into play and either renders the cells and fibres of the vestibular apparatus more sensitive or merely alters the pressure in the semicircular canals. Gowers, who regarded 90 per cent. of the cases of vertigo as aural in origin, refers under the heading of pseud-aural vertigo to cases with slight evidence of labyrinthine disease, in which he supposes that a disturbance of function of the cortex set up by a trifling peripheral derangement of the semicircular canals, becomes gradually independent and purely central.

In introducing, from the physician's point of view, a discussion on disturbance of equilibration or vertigo, it is, in the first place, obvious that neuro-otology, or analysis by means of the new vestibular tests, enables a diagnosis

of organic lesions of the vestibular apparatus to be made, and thus practically narrows down the points to be discussed by a physician, as apart from the otologist and neurologist, to two main categories: (i) What evidence is there that the action of toxins, transient vascular changes, or functional disturbances of that part of the cerebral cortex, where the sensations concerned in equilibration normally meet in harmony, may cause vertigo? (ii) The ways in which general diseases and visceral disorders affect the vestibular apparatus.

(i) It is improbable that vestibular tests can be relied on to detect transient changes, toxic, vascular or functional in origin, of the vestibular apparatus. Hence, in a case of vertigo, in the intervals between the attacks of which the vestibular tests do not reveal any abnormality, it may be impossible to determine that a passing functional disorder of the cerebral cortex rather than of other parts of the vestibular apparatus is responsible. That toxic, vascular, or functional disturbance arising at the cortical end of the vestibular system may occur appears probable from consideration of epileptic auræ and borderland cases of epilepsy, namely, vertigo as an aura and the occurrence in an epileptic of vertigo without any fit. It is also suggested by the analogy of migraine and, as Hilton Fagge pointed out long ago, by the occasional alternation of migraine and vertigo. It might be urged that, just as disturbances of vision (muscæ volitantes, hemianopsia) may be due to toxic influence or to vascular spasm, perhaps affecting the cerebral cortex (though in Raynaud's disease the retinal vessels have been seen to be contracted [1]), so may a similar cerebral localization explain vertigo. As bearing on the question of determining the localization of such toxic action, it would be interesting, as Mr. Sydney Scott has pointed out to me, to hear if deaf mutes with complete destruction of both labyrinths experience vertigo when put under the influence of vertiginous drugs, such as quinine, salicylates, alcohol, tobacco, lead, and enterogenous poisons such as indol. A large number of poisons due to defective metabolism in diseases such as gout, nephritis, arteriosclerosis, intestinal stasis, infections, are commonly regarded as capable of causing vertigo; but at present, though admitting that they may act on the vestibular end organ or on its central connexions, it must often be impossible to say with certainty where the transient disturbances causing vertigo occur.

(ii) As the mere enumeration of the ways in which general disease and visceral disorders affect the vestibular apparatus at its periphery, in its path and connexions, or in its cortical area, would entail a long list, a few examples only will be mentioned.

It has naturally been suggested that *anaphylactic conditions* may affect the semicircular canals; Émile Sergent has recently grouped vertigo with asthma, epilepsy and paroxysmal tachycardia, as manifestations of humoral dyscrasias. Duke reports patients with evidence of hyper-sensitiveness, such as asthma and urticaria, in whom attacks of dizziness could be excited or made to disappear by taking or avoiding articles of food to which they were hyper-sensitive; epinephrin also removed the dizziness. Jones quotes a case which manifested vertigo from gastro-intestinal disturbance due to fish and eggs. Parkes Weber [30] mentions the possibility that angioneurotic manifestations in the labyrinth, by increasing the pressure of fluid in the relation to semicircular canals, might cause vertigo.

Vernet has suggested that *endocrine disorders* may induce vertigo by modifying the vasomotor conditions in the vestibular labyrinth, and has advocated the oral administration of adrenalin as a successful remedy for vertigo; but our knowledge is at present too imperfect to enable a judgment to be formed as to the reality of vertigo of endocrine origin.

It has been thought that ovarian disturbance may induce otospongiosis, more often spoken of as otosclerosis, and by affecting the semicircular canals cause vertigo (Dalché and Labernadie); according to Lermoyer [17] otospongiosis at the menopause is a local change depending on a general cause. Escat describes labyrinthine vertigo as an infrequent event in otospongiosis, which he considers as probably a result of ovarian disturbance, either an excessive or altered internal secretion, and suggests that pituitary extract, by controlling the ovary, might be beneficial. The cause of otosclerosis is obscure; that it is a manifestation of endocrine disorder has been often suggested, but this view has received scanty sympathy from Fraser. In his case MacGibbon thought that the bony change so narrowed the ductus endolymphaticus that sudden vasomotor changes associated with hay fever could not be compensated for, and so Ménière's syndrome resulted.

The gastro-intestinal origin of vertigo has been much discussed; Trousseau in his clinical lectures separated "vertigo a stomacho laeso" from "vertigo ab aura laesa," and this interpretation has had a popular vogue, except among those specially interested in vertigo. Bonnier [3] regarded these cases as due to a fall of pressure in the fluid in the vestibular labyrinth, and vasomotor disturbance in the vestibular labyrinth, due to a reflex from the abdominal cavity, has been thought to be the mechanism. The occurrence of vertigo in glaucoma (Dor), explained as due to the increased intra-ocular pressure, may be an analogous example of this reflex process. The gastric manifestations and vertigo are now widely regarded as concomitant effects of vestibular disturbance instead of standing in the relation of cause and effect. But that the latter view is far from extinct among us is shown by the experience, not, I gather, rare among otologists, that the initial attack of acute labyrinthitis is diagnosed and treated as gastric influenza. But because many cases of vestibular vertigo have been regarded as stomachic in origin, it does not follow that gastro-intestinal disturbance cannot be a primary cause of vertigo, either reflexly through the brain-stem or by generating poisons which act directly on the vestibular system. Similarly, because vestibular vertigo is often regarded as due to "biliousness" or "liver," it does not follow that hepatic insufficiency and the resulting failure to stop or neutralize enterogenous poisons, such as indol, may not play a part in inducing a toxic vertigo by the action of poisons on some part of the vestibular system. In two cases of headache and vertigo Holmes found that these symptoms were associated with an excess of uric acid in the blood and disappeared when "uricacidæmia" was reduced. Lermoyer has argued in favour of vertigo due to local spasm of the vestibular artery in gouty, neuro-arthritic subjects whose vestibular labyrinths are rendered sensitive by poisons of alimentary origin, and suggests that the intermittent character of a vertigo points to a vasomotor nature; he also quotes a case of Raynaud's disease in which he considers that deafness was due to spasm of the cochlear branch and vertigo to spasm of the vestibular branch of the internal auditory artery.

The association of arterial disease with vertigo may be explained in several ways; syphilis, which may cause both endarteritis obliterans and disease of the semicircular canals, may perhaps be left aside. High blood-pressure, which so commonly precedes and causes arteriosclerosis, is associated with increased pressure of the cerebro-spinal fluid and of the intralabyrinthine fluid, and great variations may occur in the systolic blood-pressure, and so presumably in the intralabyrinthine fluids. Arteriosclerotic vessels appear to be more prone than healthy arteries to spasm, and vertigo has been

explained by spasm of the vestibular branch of the internal auditory artery, or of the cerebellar, pontine, or cerebral arteries. In cerebral arteriosclerosis, usually, I believe, in advanced cases, temporary attacks of paralysis or aphasia occur, and it is reasonable to suppose that similar attacks of vertigo may be due to transient cortical anæmia depending on vascular spasm. Attention may perhaps be directed to a result of intracranial arteriosclerosis, namely Bonnier's syndrome of Deiters' nucleus consisting of vertigo, oculo-motor disorders, trigeminal pain, and symptoms due to implication of adjacent nuclei, thirst, polyuria, and vasomotor disturbance. Nattan-Larrier and Maillard described a case under the title "maladie de Bonnier." An interesting point in this connexion is the occurrence of vertigo in Stokes-Adams' syndrome, and its absence in simple heart-block; Huchard, writing before the recognition of the causal factor of disease of the bundle of Kent-His, regarded Stokes-Adams' syndrome as due to changes in the vagal nucleus depending on cardiobulbar arteriosclerosis; now although he was wrong about the cause of the bradycardia, the added vertigo, while like the unconsciousness due to anæmia, may still be furthered by arteriosclerosis of the arteries supplying Deiters' nucleus.

In cases of arteriosclerosis combined with renal disease vertigo may be explained in several ways: in the semicircular canals, the factors may be increased exudation of fluid corresponding to œdema elsewhere, changes in the vestibular nerve comparable with those in the optic nerve, or the action of uræmic poisons; in the brain-stem, cerebellum and cortex, uræmia.

Embolism of the vessels of the labyrinth, such as may conceivably occur in malignant endocarditis, may so modify the conditions in the semicircular canals as to cause vertigo. It is possible that the giddiness of caisson disease is due to embolism of bubbles of nitrogen in the vessels of the labyrinth (Graef), though of course the larger area of the cerebral cortex might be similarly affected.

Laryngeal vertigo has been explained in various ways: as a form of epilepsy (Gray), as a result of impulses spreading from the vagus nucleus to that of the vestibular nerve, or as a result of the sudden increase of pressure in or even small extravasations into the semicircular canals, secondary to the violent expiratory efforts. Bonnier [2] raises in this connexion the analogy of subconjunctival hæmorrhages in whooping-cough.

Blood Diseases.—Both anæmia and erythræmia (Weber [31]) may, though not constantly, cause giddiness; here again arises the question on what part of the vestibular apparatus the altered character of the blood exerts its influence? In anæmia it might at first sight appear probable, from the not infrequent occurrence of syncope, that the semicircular canals are not specially, and that the medullary centres and the cortex probably are, affected; but it should be remembered that post-hæmorrhagic anæmia may eventually lead to degenerative changes in the retina, and hence, as far as analogy is a guide, the possibility that the end organs of the vestibular nerves suffer in anæmia should not be lost sight of. In erythræmia it is probable that plethora would affect the vestibular labyrinth more forcibly than the central nervous system.

Leukæmia.—Infiltration of the semicircular canals with resulting hæmorrhage is a well recognized cause of Ménière's syndrome. In Parkes Weber's [30] case, the necropsy which was performed six months after the onset of Ménière's symptoms, there was new bone formation more or less filling up the semicircular canals; this was also noted in Lannois and Politzer's cases. In Sir F. W. Mott's case there was no new bone, but the existing bone was rare-

fied by leukæmic infiltration and softened, the spaces for the endolymph and perilymph being occupied by recently escaped leukæmic blood. Hæmorrhage into the semicircular canals is almost unknown except in leukæmia (Bruce and Fraser) and even then it is rare, for among Weber's [30] nine collected cases of leukæmia with Ménière's symptoms, four only were proved to have hæmorrhage into the semicircular canals. This extreme rarity has suggested that Ménière's original case in a young girl who had an acute onset after nocturnal exposure to cold on the outside of a coach while menstruating and died on the fifth day, was really one of leukæmia. There is, however, nothing in Prosper Ménière's description to support this or indeed McBride's view that it was part of cerebro-spinal fever. Sydney Scott [26] interprets the case as one of infective streptococcic labyrinthitis with intense hyperæmia of the semicircular canals resembling blood-clot to the naked eye.

The question of hysterical vertigo and of ilingophobia (ἴλιγγος = vertigo) will be dealt with by Dr. Gordon Holmes; but in conclusion, more as a matter of historical interest, though it has been suggested that it was neurotic, I may perhaps briefly refer to *paralysing vertigo* described as occurring in small epidemics around Geneva in the 'eighties and subsequent years of the last century by Gerlier. Dr. Greene Cumston, as the result of inquiries kindly made from Professor Roch of Geneva, informs me that a few cases continued to occur up to 1901, but that since then the disease had not been seen. It appeared in the summer and vanished with the onset of cold weather, and was confined to men who were agricultural labourers or shepherds. Among the superstitious sufferers, who ascribed it to witchcraft, it was known under the expressive name of "le tourniquet" (turnstile). It was at first thought by Gerlier to be due to a "miasma," as a mildew was noticed to be contemporary, and later—though this aroused opposition and the suggestion of hysteria (Ladame)—to infection in stables; in support of this infective origin the occurrence of the symptoms in female cats which were attacked in winter was urged. Miura described the occurrence in the Northern Provinces of Japan of an endemic disease, Kubisagari, as epidemic in the summer, which he identified as Gerlier's disease and compared with myasthenia gravis, although vertigo is not a feature of that disease. The only ætiological factor noticed was its connexion with stables, thus supporting Gerlier's contention. In addition to vertigo there are other symptoms, namely, severe pain in the neck, transient loss of muscular power (the head dropping on to the chest), and ocular manifestations (ptosis, dimness of vision; Gerlier stated that strabismus was absent, but that vertigo was very often accompanied by diplopia). The disease was never fatal either in Switzerland or Japan.

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Dr. GORDON HOLMES.

VERTIGO FROM THE NEUROLOGICAL STANDPOINT.

I have been asked to approach this subject from the point of view of a neurologist, by which it is meant, I assume, that I should deal with those forms of vertigo arising from or associated with disease of the central nervous system, rather than with those varieties due to disturbances of the functions of the special sense organs that take part in the maintenance of equilibrium, especially the labyrinths and the eyes.

Vertigo is such a complex symptom that the interpretation of its manifestations frequently presents considerable difficulties to the clinician. The first and most common difficulty is to ascertain definitely what is the condition to which the patient applies the term "vertigo," "giddiness," or "dizziness."

This is partly due to the fact that vertigo consists of subjective symptoms which in each case require psychological analysis, and such analysis is often impossible owing to the transient confusion and inattention of the patient to his sensations during an attack, and partly due to the inadequacy of the language of the ordinary untrained observer to describe properly such abnormal and pathological sensations.

The second difficulty that the neurologist encounters is to exclude with certainty peripheral factors, especially slight degrees of labyrinthine disturbances that may excite vertigo directly, or indirectly by producing an instability of the central mechanisms concerned in equilibrium. It is unnecessary to insist here how difficult it often is to detect slight, yet clinically important, labyrinthine disease.

As the time allotted to me is short, I will probably serve the purpose of your invitation best by attempting to classify the various forms of vertigo met with in the diseases of the central nervous system. I am conscious that the classification I propose is incomplete and in many ways unsatisfactory; all I can claim for it is that I have found it of practical value.

VERTIGO DUE TO LOCAL CEREBRAL LESIONS.

Vertigo is a common symptom in local lesions of both the brain-stem and the fore-brain.

(1) We will consider first that met with in disease in the posterior fossa of the skull.

In the first place it may arise from local lesions involving the intracranial portion of the vestibular nerve, that is, either the nerve itself on the side of the pons, its intermedullary portion, or its terminal nuclei.

In the second place, vertigo may result from disease of those portions of the central nervous system which are intimately, though indirectly, connected with the labyrinths, as parts of the medulla oblongata, the cerebellum and the mid-brain.

Lesions in any of the structures can produce vertigo, and frequently do. We are familiar with the vertiginous attacks which often are among the most prominent symptoms of disease of the vestibular nerve in its intracranial course, whether the nerve be involved by a tumour, as an ordinary eighth nerve neuroma, by meningitis, by gummatous infiltration, or by the pressure of an arteriosclerotic vessel. I wish to emphasize the latter as a not infrequent cause, as I have seen cases in which acute attacks of vertigo seemed to be best explained by the compression of the intracranial part of one vestibular nerve by a tortuous or dilated arteriosclerotic vessel. In one of these there were other evidences of an intracranial aneurysm. In these cases there is usually a sensation of rotation of self from the healthy side towards the side of the lesion, and of external objects towards the opposite side, but this is not such a definite rule as Grainger Stewart and I suggested many years ago. There is, as a rule, severe or complete loss of hearing in these cases, but the vertigo frequently persists after all hearing has disappeared, which is relatively rare in vertigo of aural origin. The reactions to vestibular stimulation on the affected side are usually absent or modified too.

Intra-medullary disease involving the terminations or the central nuclei of the vestibular fibres is a less common cause of vertigo, but acute vascular lesions of the pons and medulla are often heralded by intense attacks of giddiness. A patient with thrombosis of the posterior-inferior cerebellar artery, for instance, almost invariably gives a history that, without warning, he became suddenly intensely giddy, with violent movement of self and external objects, vomiting, cardiac and vasomotor disturbances and intense prostration. Encephalitis of these parts may produce similar symptoms.

Lesions of those portions of the pons, mid-brain and cerebellum which are intimately connected with the vestibular nuclei give rise to vertigo of the same type. The frequency of vertigo in cerebellar disease is, in my experience, over-estimated in many monographs on this subject. I do not believe that the small cortical softenings of this organ which are occasionally seen in the post-mortem room, are often a cause of giddiness, but when they extend deeply into the white matter or to the central nuclei, and particularly when they involve the peduncles of the cerebellum, vertigo is very common. Tumours of these regions often give rise to vertigo too, but the attacks are usually less violent and stormy. It is, however, striking that many patients with tumours of the cerebellum never complain of giddiness, and in others there may be little or no sensation of movement of self or of external objects. In some of these cases of growth in the posterior fossa of the skull vertigo seems to be due to the irritant effect of the tumour, in others it is probably a result of the increase of pressure which the growth produces.

Vertigo also occurs in degenerative diseases of the cerebellum. Here it is probably secondary to the disturbances of equilibrium which these conditions produce, rather than to irritation or release of function.

(2) Vertigo occurs as a rule with local disease of the forebrain only when the cortex or subcortical white matter is involved. As far as I am aware there is no unequivocal evidence that it is ever associated with any form of disease of the basal ganglia or of the central white matter, except when this produces a rise of intracranial pressure or interferes with the circulation of the cortex.

It is sometimes assumed that there exists in the cerebral cortex a centre for equilibrium, to the disturbance of which vertigo may be attributed, but there is no real evidence of this; equilibrium is a much more complex function than can be assigned to any one centre. On the other hand lesions involving any portion of the cortex from which spasmodic phenomena, especially movements of the head and eyes, may be excited, may give rise to giddiness. There exist in the frontal lobe, for instance, centres for movements of the head and eyes to the opposite side, and if these are in any way irritated a discharge results which leads to clonic jerking of the head and eyes to the opposite side. Vertigo is usually associated with this, that is sensations of movement of self and of external objects accompany the actual movement and lead to a disturbance of equilibrium.

Similar spasmodic movements of the head and eyes may be excited from the occipital, temporal and parietal lobes, and irritating lesions involving these may in the same manner give rise to vertigo. During and since the war, for instance, I have found that vertigo was frequently complained of by men with gunshot wounds of the occipital lobes, who were subject to epileptiform seizures in which visual phenomena were prominent symptoms. In these attacks a clonic jerking of the head and eyes towards the direction to which the visual phenomena are referred usually occurs and this is the immediate cause of the vertigo.

In these affections the vertigo is not always associated with, or secondary to, convulsive manifestations and may occur independently of them. The probable explanation of this is that in these circumstances the disease disorganizes the functions of these parts of the brain to a sufficient degree to produce the subjective symptoms of vertigo, though not intense enough at the moment to excite convulsive phenomena.

All these local lesions of the brain, whether of the cerebral cortex or of regions more intimately connected with the vestibular nerves, give rise to a definite vertigo, that is a feeling of movement of self, either a sensation of rotation or displacement in any place of space, or of movement of external objects in relation to self, or oscillation or rotation on their own axis, or of both movement of self and of external objects, accompanied by emotional phenomena of unpleasant tone which may be sufficiently severe to lead to mental confusion or obfuscation, and often associated with visceral and vasomotor disturbances, as vomiting, palpitation, pallor and sweating. There are frequently objective signs too, such as loss of balance, nystagmus and reactional movements. Occasionally, however, the vertigo assumes a less definite form and is described by the patient as a dizziness, a momentary uncertainty of balance, or a swaying or other indefinite movement of objects seen.

Vertigo with Diffuse Cerebral Lesions.

The second class of cerebral disease in which vertigo is a common symptom includes diffuse affections and those which interfere with the blood supply of the brain or disturb its functional stability.

In the first of these classes I would include cerebral arteriosclerosis, anæmia, and the other affections of the cardio-vascular system to which Sir Humphry Rolleston has already referred. It is probable that in certain cases at least an increase of intracranial pressure acts in the same way; the immediate relief that often follows lumbar punctures suggests that the most

important effect of the pressure is an interference with the cerebral circulation.

How these different conditions produce vertigo has not been satisfactorily explained, but it seems to me probable that it is due to a lack or failure of the proper integration of the afferent impressions on the harmony on which our sense of balance depends. We know that in these conditions, especially cerebral anteriosclerosis and rise of intracranial pressure, there are evidences of defective integration in other spheres of cerebral activity, such as loss of memory, confusion and other manifestations. These symptoms are, in fact, frequently accompanied by, or alternate with, vertigo, or they may be permanent symptoms, the vertigo being merely episodal.

In the second sub-group I would include the effects of cerebral concussion, and the action of certain poisons, as alcohol, on the nervous system. The giddiness which is commonly present in neurasthenia may, perhaps, be included here too. All these conditions lead to a functional instability which, when pronounced, may produce or predispose to vertigo.

The vertigo associated with these diffuse affections of the brain is generally of a more indefinite type than that due to local lesions. The patient usually refers to it as "dizziness"; the illusion of movement of self is less definite, or its direction is not recognized, or there may be merely a feeling of unsteady equilibrium, unaccompanied by any apparent movement of external objects, but usually associated with transient mental confusion.

Vertigo as a Symptom in other Nervous Diseases.

In the third class I would place the vertigo which is a symptom or manifestation of other nervous diseases, particularly of epilepsy and migraine. In epilepsy it may occur as the warning or aura of an attack, or may be an equivalent replacing the attack. Vertiginous auræ are of course not uncommon in ordinary idiopathic epilepsy; they may consist of a sensation of rotation of self, or of external objects, or of both, or it may be merely giddiness which the patient cannot analyse or describe more fully. When these auræ precede minor attacks epilepsy may not be suspected, and the nature of the vertigo may not be consequently recognized. Some time ago I saw a woman who had been subject to sudden attacks of vertigo for several years. She stated that the giddiness was sometimes so intense that she lost consciousness for a moment, but she was not always aware that she did so. It was only later, after she had had a few unmistakable major epileptic seizures preceded by identical attacks of vertigo, that the true nature of the case was recognized.

Attacks of migraine are sometimes accompanied or replaced by sensations which the patient describes as giddiness; in some cases there are severe attacks of definite vertigo, in others there are merely feelings of unsteadiness or indefinite cephalic sensations, or apparent movement of external objects, alone or associated with headache and nausea.

I must finally refer to the question whether vertigo is ever merely a manifestation of hysteria. It is, in my opinion, never safe to regard true vertigo as an hysterical symptom. But the manifestations of hysteria are so protean that it is not surprising that patients occasionally complain of symptoms which, if not carefully investigated, may suggest vertigo. The other possibility is that if an hysterical patient has at any previous time suffered with vertigo the symptoms of the latter may be reproduced as hysterical manifestations.

I am conscious that this classification of vertigo resulting from disease of, or functional disturbance in, the central nervous system is incomplete and crude, and in certain respects unsatisfactory. I have, however, found it useful in investigating patients complaining of vertigo in whom aural disease can be excluded.

Perhaps in the present state of our knowledge a wholly satisfactory classification is impossible, for there is much truth in the epigrammatic saying of Gowers, that "vertigo is a symptom which is often more obtrusive than its cause."

Mr. Sydney Scott[1]

said he would consider the possible peripheral causes of vertigo. He showed a number of slides illustrating the normal anatomy of the labyrinth and of the middle ear, to remind those present of many well known facts which he thought were often forgotten or overlooked in considering the causes of vertigo.

Among the illustrations shown were those obtained from sections of the labyrinth and middle ear in infective otitis media. Streptococci were demonstrated disintegrating the membrana secundaria, and penetrating the labyrinth. Such destructive processes always caused the symptom of Ménière's disease. In fact he regarded Ménière's one fatal case, the only one in which an autopsy was obtained by Ménière, as being due to infection of the labyrinth. The post-mortem appearances to the naked eye resembled hæmorrhage into the semicircular canals, on account of the hypervascular state of the membranous lining in spreptococcal infections. Moreover similar cases, when untreated, generally terminated in lepto-meningitis, and elsewhere he had submitted that the fatal termination in Ménière's case was in fact due to that form known as meningitis serosa.

The onset of vertigo in the course of otitis media was therefore a symptom of the utmost practical importance, as it might be the call for immediate intervention on the part of the surgeon. But it would be wrong to assume that every case of vertigo in the course of middle-ear disease, whether acute or chronic, indicated invasion of the labyrinth. A normal labyrinth might be disturbed by processes taking place outside the labyrinth. Many were aware that vertigo and nystagmus occurred, not rarely in the earlier stages of acute otitis media, generally before the drum membrane had perforated spontaneously, or had been incised.

This distinction between labyrinthine disturbances due to destruction, and those due to extra-labyrinthine causes, could be ascertained by means of the various tests which were now in use. He referred particularly to the caloric tests. So long as the patient who was giddy yielded positive caloric tests, they had no right to diagnose disease within the membranous labyrinth. The surgeon could rarely have any justification for destroying the labyrinth, so long as positive caloric tests were obtainable.

The speaker next showed sections of the labyrinth in a case of bilateral auditory nerve tumour. Vertigo had not been a marked feature of this case, but in unilateral eighth nerve tumours, vertigo and staggering were consistently present, though these symptoms were not always as violent as might be expected.

Among patients subject to the most intense vertigo were those who had no

[1] Abstract of contribution to the Discussion.

signs of intracranial disease, or of labyrinthitis, or of middle-ear infection. Deafness and tinnitus were not by any means constant or permanent features of the complex.

He showed the different tone ranges from a number of different patients. In one group was a contracted auditory field, with a distinctly raised lower limit—a common condition in cases of ankylosis of the ossicular joints. When both ears were deaf to low tones, below, say 90 d.v. per sec., and the patient had signs of bilateral middle-ear deafness, he was not liable to be giddy like patients who had no deafness, or only unilateral middle-ear deafness. This agreed with the well-known fact, that patients with only one deaf ear were not ordinarily noticed to be deaf. But amongst these were many who were liable to be giddy. The dictum that such giddiness was likely to endure until the patient became deaf, was founded on the fact that "deafness" was a sign that *both ears* were more or less involved. By the time that there was bilateral deafness and marked raising of the low tone limit, which generally indicated that there was bilateral ankylosis of the ossicular joints, attacks of giddiness had usually ceased. What was the explanation of these well-known general observations?

The speaker recalled some experiments made years ago, and repeated by him, to ascertain the effect of changes of atmospheric pressure on the labyrinth pressure. His experiments confirmed Politzer's own results.

If, for convenience, the superior semicircular canal of the human temporal bone from the post-mortem room was opened and a capillary glass tube, containing a coloured fluid, was sealed into the canal, as a manometer, it served to show the effect of movements of the stapes in producing relatively to such a sensitive organ enormous changes in pressure in the labyrinth. This fact, he considered, many forgot or overlooked. Similar changes of pressure could be evoked by distending the tympanum and by compressing the drum membrane inwards. They had only to examine the drums of the ears in aviators and in caisson divers to see the maximum effects of air pressure—the invaginated drum membrane—increased by a choked Eustachian tube. The results of such observations all pointed to the fact that unilateral differences of pressure and sudden changes of different pressure in the middle ear, could be transferred to the labyrinth through the movements of the stapes. Undoubtedly this was an important factor associated with vertigo.

In cases of unilateral ankylosis of the stapes there was, of course, unilateral deafness. The cause of vertigo in such patients should always be sought for in the ear where there was no sign of ankylosis.

His experiences during the last sixteen years at Queen Square led the speaker to regard temporary disturbance of the peripheral termination of the vestibular nerve as a common cause of vertigo. Investigation and tests by caloric stimuli, and if necessary by rotation and by galvanism, could be relied upon to exclude any intralabyrinthine lesion. Galvanism was of value in differentiating medullary and extra-cerebellar lesions from labyrinthine lesions.

It was of vital importance to ascertain whether aural vertigo was due to labyrinthitis. But the commonest causes of aural vertigo were not due to labyrinth disease, but arose in connexion with Eustachian inefficiency. The high percentage of cases of post-nasal catarrh as a factor associated with the occurrence of temporary attacks of vertigo could have no other significance.

His experiences coincided with that of other observers, by whom this relationship between vertigo and choked Eustachian tubes had been noticed, namely, McBride and Grainger Stewart, Dundas-Grant and Mark Hovell; but he

would remind those present that the signs of obstruction had often disappeared with the cessation of symptoms and at the time of examination. After recovery from an attack, an abnormal state in the ear might no longer be observable. He had been fortunate enough to have opportunities of examining the ears during the height of attacks of vertigo, when the cause was recognizable, and again after attacks when the cause was often unrecognizable. He urged that in all cases the efficiency of the Eustachian tubes and state of the posterior nares and accessory sinuses of the nose, should receive particular attention, as well as the reaction of the labyrinth to caloric or rotation stimuli. He believed many of the obscure cases of Ménière's disease met with in clinical practice could be traced to one or other of these causes. Mr. Scott said he would not mention other rarer aural disturbances causing vertigo, as he left these to the subsequent discussion.

Mr. J. Herbert Fisher.

In venturing to make some introductory remarks on vertigo from the ophthalmological aspect, any attempt to lead you to believe that I had acquired a special knowledge of the subject from its literature would soon be exposed. Neither can I claim that I have an acquaintance with vertigo from any particular attention that I have given to the question in my clinical work. Such observations, therefore, as I shall bring before you may be crude, but they will be personal, and on each of these accounts may the better serve the purpose of promoting discussion and argument, the primary objects for which we are gathered.

That visual judgments and information obtained from different impulses generated in the extra-ocular muscles are elements which contribute to our equilibrium is a proposition which no one would deny. The relations which we appreciate objects to bear one to another in the world around us must be in the main determined by the visual images which they form upon the retinæ.

An image of the field of vision is formed on the retina; and the image of each object in the field is projected outwards along the secondary axis passing through the nodal point to its proper position in the field—this relation of objects to one another in space is called objective localization, or objective orientation.

Disorder of objective orientation may lead to metamorphopsia, polyopia and errors of judgment; I should say it is doubtful if it ever causes true vertigo, though when the latter condition is present from other causes it would undoubtedly add to the mental confusion.

The advantages of binocular over monocular vision are conspicuous. I have recently seen a man with one eye who, seated behind a hedge waiting for driven partridge, mistook a gnat at two or three yards for a partridge at fifty yards and barely restrained himself from shooting.

Making use of stereoscopic vision, as supplied by our two eyes, we are enabled to judge of the solidity of objects, and perhaps less accurately of their distance from us and of their size; the two latter factors are closely interwoven; a misjudgment of the one will lead to a wrong estimate of the other, and in estimating size, the influence of simultaneous contrast must not be forgotten. All these are purely visual functions, and none of them operate unless the retinal stimuli influence consciousness as highly organized visual perceptions. To this extent it appears to me, that the visual sense differs from deep and superficial tactile sense, from muscular sense and the guidance which

it supplies, and also, from that derived from the semicircular canals. Different impulses from these latter sources can, and no doubt do contribute to balance-maintenance without affecting consciousness, as is proved by physiological experiment in lower animals and clinical evidence in man; it is well that this should be so; did every stimulus derived from external sources affect consciousness we should be worthy of Bedlam, while if they did no more than provoke reflex movements, our life would be one of uninterrupted convulsions. By closing the eyelids we can at will abolish visual sensations. The other stimuli to which I have alluded are not under volitional control, and except during natural or artificial sleep, are at all times operative.

The statement has been made that unaccustomed visual sensations may, *per se*, be a cause of vertigo, e.g., that a sense of giddiness is produced by gazing at a waterfall: that it is possible to view such an object with complete immobility of the eyes seems to me improbable, and I should be inclined to believe that unperceived ocular movements play the important part in giving rise to the vertigo. An experiment by Professor Silvanus Thomson showing that the after-images of two discs, rotating in opposite directions side by side, are seen equally to be rotating in opposite directions is, however, difficult to explain on a muscular hypothesis. Disjunctive rotatory nystagmus has, however, been observed, and if in the experiment each eye had fixed an individual disc, an easy explanation offers itself.

The part, therefore, which the eyes play in the maintenance or otherwise of equilibrium, appears to me to depend in the main upon the functions of the internal and external ocular muscles. Though it is impossible entirely to dissociate these two groups, which for many purposes at least have an intimate physiological association, a few words on the muscles of accommodation and refraction errors may be appropriate. It is common knowledge that when a hypermetrope first wears his correction and moves about the world in convex glasses, he makes erroneous judgments; the ground appears too close to him, with some resulting disorder in walking shown by the exaggerated way in which he lifts his feet. If such a patient be presbyopic, his hypermetropic correction will so sharpen his view of distant objects, which formerly were to him somewhat indistinct in outline, that he judges such objects to be nearer to him than they really are, and than they formerly appeared to him to be. Such disturbances of judgment are, in themselves, not enough to produce any sense of vertigo. More attention has been centred upon astigmatic errors of refraction, more particularly when the axes of the correcting cylinders are at oblique angles; claims are made that the correction of 0·5 D., or even of 0·25 D. of astigmatism will eliminate the giddiness; my own experience lends little support to this, though I recall one patient with 0·5 D. astigmatism who complained of vertigo when he walked beside railings, though in an open space he experienced no such sensation, and was quite clear that his unusual symptom was quite cured by the 0·5 D. cylinder lenses which I ordered. In regard to this case, it has always seemed to me that if his cure was not entirely by means of suggestion, some weight might be attached to the fact that the cylinders would render the vertical and horizontal bars of the railings equally definite for him. It has to be remembered that patients use the word giddiness in a sense which is far from accurate or scientific; we need to be sure that they experience real vertigo in each case before we attribute a cure of giddiness to correction of refraction errors. Correction of astigmatism with oblique cylinders, however, undoubtedly disturbs ocular judgments, and we warn patients of the need to acclimatize themselves to such glasses; uncorrected astigmatism, conversely, would

tend proportionately to diminish the value of eyes in the maintenance of equilibrium.

In making the above remarks, I do not by any means wish to suggest that we should ignore refraction errors in dealing with cases of vertigo. Physiological working of all the eye muscles is desirable, as I propose to indicate later, if abnormal impulses are not to reach and throw out of harmony the equilibrium centres, which are receiving information from multiple sources.

Assuming that we have a correct sense of body balance, it will be necessary that our external eye muscles shall be accurately coördinated, if by the sense of sight we are to estimate our relationship to objects in the world outside us. By the muscular sense of our ocular muscles we learn, subconsciously, the position of our eyes in our heads, and their alignment in regard to objects around us. This is what is meant by subjective orientation, disorder of which function results in false judgment of our own position and a sense of giddiness or want of balance, which is not experienced as the result of the conscious processes which constitute false objective orientation. Disarrange the balance of our eye muscles, and we must draw deductions from false premisses, while if ophthalmoplegia externa be present, diplopia and false projection will complete our confusion.

The relative values of objective and subjective orientation is an interesting one. It is well known that occasionally a patient is met with, who, having developed paralysis of a muscle, e.g., the external rectus, in his better eye, yet prefers to use this eye and to exclude his non-paralysed eye of less visual acuity, to avoid diplopia. If this has been his habit for any length of time, testing will reveal that the patient is the subject of false projection when using the non-paralysed eye alone, and he walks unsteadily if attempting to guide himself with this eye. It is to be presumed that he has established a new sense of values for the function of his paralysed eye, and including such a position of his head as will call for little direct action on the part of the paralysed muscle. That such cases can exist would seem to suggest that the organ of vision plays a rather subsidiary part in the maintenance of equilibrium. It is to be presumed that in birds possessed only of panoramic vision, and in whom the movements of the eyes are extremely limited, the sense of equilibrium is highly developed. Primates alone are provided with full stereoscopic vision demanding a complete harmony of movement in the two eyes. In man, with one eye only available, no sense of vertigo is experienced, and it appears to be not difficult to learn to use one eye for guiding our movements with fair accuracy, even when its external musculature is in a state of disorder.

Short of ophthalmoplegia externa, such conditions as labyrinthine nystagmus, or the clonic spasm of compositor's nystagmus, cause us to see stationary objects as if they were in oscillatory movement; labyrinthine nystagmus, I understand, is supposed to be produced by the influences from the semicircular canals reaching Deiters' nucleus in the cerebellum, and, being thence reflected to the ocular muscles, jerking of the eyes results, and stationary objects appear to be in oscillation. It is reasonable to think that abnormal impulses from disordered ocular muscles may reach the same centre, and in the cerebellum may provide reflex irritation of the labyrinthine nerve centre and thus a sensation of vertigo. Such an explanation would at least be worthy of consideration in regard to the vertigo of insular sclerosis when nystagmus is present. Train-sickness and sea-sickness are in all probability partly due to the disturbance of ocular muscles which results from viewing

the moving waves or the apparently moving objects seen from the railway carriage window, and this disturbance may persist at least for some hours after the journey has been completed. After a bout of sea-sickness I can well remember the apparent undulating movement of the tablecloth, and the inclination to try to steady it by placing the hands upon it; this I conceive must have been originated through the eye muscles. The school-boy trick of bowing the head on top of a cricket stump and whirling round it six or eight times produces a sense of intense vertigo; this is no doubt due in the main to the semicircular canals, but the giddiness is more severe if the whirling is performed with the eyes open than if they are closed; disorder of ocular muscles is then induced and its effects are superadded to the labyrinthine vertigo.

Heterophoria, it appears to be generally agreed, is of little importance as regards our subject; of errors of this class vertical heterophoria is of more importance than lateral heterophoria.

Vertigo is a frequent symptom in disseminated sclerosis; apart from focal lesions in the cerebellum I have suggested above that the conditions of nystagmus may react reflexly on the labyrinthine nerve centre. When, as so often, ophthalmoplegia externa is also present the resulting diplopia will add further confusion to the visual judgments; I am not of my own experience aware that nystagmus in insular sclerosis gives rise to apparent oscillatory movements of objects viewed by the patient, though it is stated that this sometimes may occur. The transient diplopia of locomotor ataxy results in false orientation, but I imagine a true vertigo is unusual. In tabes the value of accurate visual judgments in maintenance of balance is well shown; when in this disease deficient information is conveyed from tactile and muscle sense in the lower limbs, the swaying of Rhomberg's test is inconspicuous unless the eyes are closed. Ocular judgments in this disease can, to a large extent, compensate for the disordered subconscious impressions. On the other hand any vertigo of purely ocular origin is relieved by closing the eyes, or if due to irregular action of the intra-ocular muscles by the use of a cycloplegic. Such tests may with advantage be made in cases of doubtful diagnosis.

It seems hardly necessary to elaborate the sensations of giddiness due to ophthalmoplegia externa; to disorientation and gross disorder of muscular sense impulses is necessarily added the confusion which double vision induces, and the apparent movement of stationary objects which may be recognized when a call is made upon a paretic muscle. With false projection and the disturbances of locomotion which it occasions we are all familiar. Where diplopia results from anchoring of one eye by cicatricial changes in its orbit, I am inclined to think the disorientation symptoms are less marked than in cases of extra-ocular paralysis.

When either an eye long blind from disease, or a seeing eye recently injured, has been excised, the patient experiences no sense of vertigo. The interesting question has in my hearing recently been raised and discussed whether or not the removal of a congenitally amblyopic eye, which may or may not have been strabismic, results in any disability to the patient's visual judgments. It is an interesting point, upon which further observations and information are desirable.

Under normal conditions harmonious conjugate movements of the two eyes are the only movements which we can voluntarily impart to them; even convergence needs the stimulus of the presence of an object at short range before we can effect this movement, and is in the main a reflex effort. It is only

when loss of coördination of movement occurs, as in various diseases, or in alcoholic or other poisoning, that the movements of the two eyes cease to agree with one another. Vertigo appears to me to be a conscious process; normal equilibrium is maintained subconsciously; but a sensation of giddiness can be experienced only as a higher mental perception. As far as eyes are concerned lack of harmony between our various sources of information in regard to our balance will produce vertigo only if we are consciously aware of the discrepancies. The two eyes of a man in a state of alcoholism may be out of true alignment, but this, as far as visual sense is concerned, would produce vertigo only if he perceived the results of their malposition; on the other hand, a man who goes to bed in a semi-drunken condition may, when lying down in the dark, experience a sense of vertigo; by switching on the light and opening his eyes, if the latter are working in coördination, he will be able by correct visual judgments to eliminate his discomfort, and is conscious that he has succeeded in doing so.

When we darken the bedroom and close the eyes to encourage sleep, all visual stimuli, whether conscious or subconscious, are excluded. A patient with lagophthalmos is not, so far as I am aware, disturbed in his sleep by subconscious visual stimuli, though there may be sufficient light to generate such; in sleep walkers the lower centres are sufficiently active to enable equilibrium to be maintained and complicated movements to be performed; the eyes may be open; the sleep walker in partial daylight is unconscious of any visual impression, and I should very much doubt if stimuli generated in the retinæ are contributing even subconsciously to his manœuvring capacity.

The eye movements necessary to watch either the falling water of a cascade or objects seen through the window of a moving railway carriage, or to perform the duties of a compositor, are objective and volitional; the nystagmus which they induce causes stationary objects to which the eyes are next turned to appear in jerky movement. On the other hand the movements of congenital nystagmus, due to defect in the eyes, are reflexly produced and unconscious; in this class of case no sensation of jerking in the outside world is ever experienced, either when the nystagmus is most active or, on the other hand, when it is in temporary abeyance as in some patients.

The above considerations may help to strengthen my suggestion that the sense of vision contributes to equilibrium only when it affects consciousness through the higher perception centres.

Equilibrium appears to me to be a subconscious process maintained by tactile and muscular sense, and provided for above all through the semicircular canals, the vestibular nerve and the cerebellar centres; to the total sum of muscular sense the muscles of the eyeball contribute their quota; disorder of the muscular and tactile elements of balance produce incoördination which may be sufficient to cause stumbling movements, or loss of equilibrium sufficient to produce a fall without originating any perception of vertigo. Disorders of the semicircular canals, vestibular nerve or cerebellum produce a conscious vertigo, but vertiginous movements may be produced experimentally through this route in a decerebrate animal. The highly developed special sense of vision can operate only as a higher mental perception; in this direction it can, if working normally, compensate for a lack of general muscular coördination or even control a vertigo, while, if acting abnormally, it aggravates a vertigo by the false information which it supplies. Disorder in the coördination of ocular muscles may, reflexly, influence primarily the cerebellum, and thence give rise

to a perceived vertigo of low intensity, while the resulting incapacity may be further aggravated by the conflicting visual judgments. Accurate visual impressions may act as an arbitrator when the stimuli derived from other sources necessary to equilibrium are inharmonious, and thereby abolish any sense of vertigo; apart from disorder of the ocular muscles, the sense of sight will not itself produce any appreciation of true giddiness. Vertigo is a higher perception and not the true converse of equilibrium.

Neither the sense of sight nor information from the ocular muscles is essential for the maintenance of equilibrium.

Normal visual impulses, normally projected on the sensorium, contribute to equilibrium and may be capable of controlling a vertigo generated by disorder of other mechanisms.

Unaccustomed impulses generated by a disordered retina will produce no sense of vertigo if they are normally transmitted to, and projected upon, the sensorium, though they may disorder visual judgments. Normal visual impulses, on the other hand, if abnormally transmitted to, and projected upon, the sensorium may produce a sense of vertigo, and *a fortiori* abnormal visual impulses.

Sir JAMES DUNDAS-GRANT, K.B.E.

said that one difficulty was that of diagnosis in cases of suspected Ménière's disease, cardiac syncope, laryngeal syncope, and petit mal. Small doses of quinine had so remarkable an effect in checking, sometimes putting an end to, these attacks of aural vertigo, that if small doses of quinine did not, in a unilateral case, cause diminution of the vertigo, it was probably not a semicircular canal condition. He thought the quinine equalized the influence of the two labyrinths by its sedative action on the sound labyrinth.

He agreed with Mr. Sydney Scott as to the part taken by the structures of the middle ear in so many of the cases which presented Ménière's symptoms. A middle-ear lesion alone might cause them, as in otosclerosis, where the stapes was fixed, or when the ossicles were fixed as a result of suppuration, or even when the relaxed membrane was pressed inwards by the external atmosphere, so as to prevent the bones from moving. Why did such a condition cause vertigo? In the normal state the movements of the stapes and also of the membrane of the round window allowed them to act as safety-valves, and there might be a sudden vascular increase of pressure in the internal ear, without its causing disturbance, because of the yielding of these two safety-valves. When they were fixed, that action was lost. He believed that to be the explanation of Mr. Scott's observation that he could check vertigo by compressing the carotids.

In some of the worst cases with which he (Sir James) had had to deal, the cause had been "cholesteatoma," the desquamative condition in the middle ear after suppuration having lasted a long time. The symptoms disappeared after an exit had been effected by ossiculectomy.

Recently experiments had been made concerning the action of the sympathetic nerve on the circulation in the labyrinth, and on the consequent changes in responsiveness of the labyrinth to caloric tests. Particulars of one of these experiments had appeared in a German periodical. Both the vertebral arteries were blocked up where they entered the skull, and one carotid canal was tied, so that the circle of Willis was only supplied by one carotid. The rapidity of response to the caloric test was then tried in the ear of the same side as the open carotid, and the time required to induce nystagmus noted. The sympathetic in the neck was then stimulated on that side. It caused

contraction of the blood-vessels, and a much longer time was required for nystagmus to be induced.

He (the speaker) had made the observation that the vertebral canals could be compressed digitally in the living subject, behind and a little below the level of the mastoids. In the normal subject it took distinctly longer to induce response to the caloric test when that pressure was so made. This confirmed what Sir Humphry Rolleston said as to the action of the sympathetic nerve.

He asked Fellows to note that the late Sir Victor Horsley accepted Mills' idea of the localization of a centre for the sense of equilibrium in the temporal lobe.

Concussion was a frequent cause of vertigo. Erichsen found that in railway spine due to concussion great relief was afforded by small doses of perchloride of mercury. He (Sir James) found the drug had the same effect in the vertigo caused by concussion, and therefore he recommended its use to those who had to deal with cases of vertigo from concussion.

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Section of Neurology.

President—Dr. JAMES S. COLLIER.

PRESIDENT'S ADDRESS.

The Pathogenesis of Cerebral Diplegia.

By JAMES S. COLLIER, M.D.

THE Pathogenesis of Cerebral Diplegia, the subject which I have chosen for this Presidential address, is a very ancient subject, for it is eighty-one years since Dr. Little, Senior Physician to the London Hospital and founder of the Royal Orthopædic Hospital, commenced his writings which culminated in a monograph based upon a personal experience of 200 cases. This monograph is so complete in clinical detail, so free from error either of observation or deduction, and so truthful that it has completely withstood the test of time to the present day. Even modern writers repeat "he left but few details that can be added to his clinical description of the disease, and we may justly regard this as one of the first monographs on modern neurology." No malady more appropriately continues to bear the name of its recorder than does "Little's disease." His truthfulness of description and accuracy of deduction have not by any means been followed by all of his successors who have written upon this subject, some of whom seemed to have failed lamentably both in observation and in accuracy of deduction. And yet their conclusions have gained such universal acceptance, as to have led to a serious impairment of the progress of knowledge upon the subject, and to such confusion of the issue, that despite the excellent work and splendid monographs which have been published by many writers, the student of the present day, if he has gained a fair working idea of the pathology of this disease, will not have obtained this knowledge from any text-book of neurology written in the English tongue, whether translated from a foreign tongue or no.

I can best introduce my subject to you, and build up a conception—though perhaps a personal one—of the causation of cerebral diplegia by briefly reviewing the more important contributions which have been made to this subject.

Although some of the clinical features of cerebral diplegia were described in the early days of the nineteenth century, by Andry, Delpech, Heine and others, under the name of "cerebral spastic paralysis," and although Joerg, in 1828, stated that "too early and unripe-born children may present a state of weakness and stiffness in the muscles persisting until puberty or later," yet it was Little in the *Lancet* of 1841 who first called attention to "The influence of abnormal parturition, difficult labour, premature birth, and asphyxia neonatorum, on the mental and physical condition of the child, especially in relation to deformities." He saw at once the difficulty of making such widely dissimilar physical

conditions as premature birth with precipitate delivery on the one hand, and prolonged labour with asphyxia on the other hand, responsible for one and the same pathological condition. He did not think that gross injury to the brain was the cause of any of the diplegic forms of infantile spastic paralysis, though he thought it produced some of the hemiplegic forms, for how could the precipitate birth of a premature child in a multiparous woman give rise to injury to the brain? He pinned his faith to asphyxia as the cause of the lesion of the nervous system. In all prolonged labour and in breech presentations the cause for the asphyxia is obvious. In precipitate labour and in the case of premature infants he pleads again for asphyxia as the essential cause. Pressure upon the umbilical cord even for a very short time is likely to have a much more serious effect upon the immature than upon the mature. The prematurely born child may not have sufficient vitality for respiration to be established soon enough, or its vessels may not be ready for the change of circulation which should occur at birth. He actually found the same capillary congestion and minute petechiæ in the brain, heart and serous surfaces of the premature and precipitantly born, such as are well known to occur in children dead after obvious asphyxia; and he argued that widely spread fine lesions of this nature, in which—to use his own words—"decarbonization produced devitalization of the brain" and in which petechial hæmorrhages played a part, were the cause of spastic paralysis, which he attributed to these various abnormalities of birth.

The value of Dr. Little's contribution may be stated thus: He first established the incontestable fact that diplegia is often associated with abnormalities of birth, at the same time stating "it is obvious that the majority of apparently stillborn children whose lives are saved by the attendant accoucheur, recover unharmed from that condition." He theorized the lesion as a widely spread fine lesion of the cerebral substance which could affect one part of the brain more than another. He showed that the less severe cases may progressively recover in the course of years, thereby proving that the slighter degrees of the lesion were reparable, and, lastly, he excluded gross injuries to the brain, and gross lesions generally, from the ætiology of the disease which bears his name, and pointed out its common association with microcephaly. Strictly speaking, the term "Little's disease" should be confined to diplegia which is associated with difficult birth. Actually, it is used by the French school for every variety of cerebral diplegia present from the time of birth. Brissaud strove hard to confine it to the paraplegic forms occurring in premature children.

In 1875, Erb and Charcot described the clinical picture of "primary lateral sclerosis," which they attributed to a primary affection of the pyramidal tract in the spinal cord; and cases occurring in infants were soon discovered and described as "spastic tabes in infancy." These were examples of the paraplegic form of diplegia, Little's work being apparently unknown both to Erb and to Charcot at the time of their earlier writings. But the subject of Little's work was soon brought into prominent discussion and a heated controversy was waged as to whether the primary spastic paraplegias of childhood were of spinal or of cerebral origin. The battle went against the supporters of the spinal theory, and in the end, owing to the excellent work of Wolters, and of James Ross, of Manchester, it became universally accepted that the lesion was cerebral in site. During this controversy much pathological material was collected, with the result that the anatomical finding was generally accepted to be an atrophy and sclerosis of the convolutions, remarkably symmetrical,

sometimes general, producing a walnut-kernel type of brain, sometimes local and incident upon any part or parts of the convexity, but most often affecting the Rolandic region and in the paraplegic forms most noticeable in the paracentral region, without any sign of vascular lesion or of meningeal involvement. This was the " Lobar Atrophic Sclerosis " of the French school. It was recognized as the end-result of some previous pathological process, no explanation of the nature of which was forthcoming. The important fact was also emphasized that the degree of the atrophic sclerosis present often did not correspond with the severity of the clinical picture; the paralysis might be severe and the atrophic sclerosis slight. The French authors agreed that the symptomatology was determined by other than the anatomical conditions as yet brought to light. In other words, the unknown initial pathological process may at the time of death, be much more extensive than the atrophic sclerosis which follows in its train. The correctness of this conclusion has been supported by all the subsequent evidence.

In 1885 Dr. Sarah McNutt wrote a thesis upon this subject which requires examination in detail, for it is important not only on account of the excellence of her clinical report and pathological examination, but also, and chiefly, on account of her false deduction, which, aided by the support given by Sir William Gowers, had the most amazing and inexplicable effect upon public opinion as to the pathology of the disease. It introduced a glaring error which has persisted to this day, and which, though pathologically untenable and resting upon no evidence whatever, seems to have defied all the attempts which writers during the past twenty-five years have made to eradicate it.

Dr. McNutt described a case of double infantile spastic hemiplegia in a child born with instrumental labour, which from the time of birth showed signs of very typical general spastic diplegia and which died at the age of 2½ years. The pathological findings and the microscopic anatomy are beautifully described and figured, so well in fact as to leave no possible doubt as to the nature of the condition present. The brain was small and of the walnut-kernel type, the arrangement of the convolutions is primitive, and there is macrogyria. There was a deep wide groove on either side extending from the region of the orbital lobule, in the direction of the Rolandic fissure to the paracentral region, which almost separated each hemisphere into two parts. At the bottom of this groove the ascending frontal gyrus was buried and was atrophied almost to extinction, while the adjacent precentral and postcentral gyri were severely atrophied. No ganglion cells nor nerve fibres were to be seen in the atrophied cortex, and no nerve cells were to be found even in the grey matter of the convolutions, where the cortex was best preserved. Dr. McNutt's pathologist described the condition as one of a sclerosis commencing in the deeper layers of the cortex and invading the cortex from within outwards. The pyramidal tracts were degenerate above and wanting below.

One can have no hesitation in being positive that the pathological process in this brain described by Dr. McNutt had begun long before birth, from the primitive arrangement of the convolutions and from the condition of the pyramidal tracts. It was a typical condition of lobar atrophic sclerosis. Had Dr. McNutt remained content with her description of this case, no harm would have been done. But she proceeded to refer to another case which she had published and figured, of a child delivered by the breech with easy labour, who from the fourth day onwards had severe convulsions, and on the twelfth day began to develop left hemiplegia, which became complete. Death occurred on

the twenty-second day. Upon necropsy, the left hemisphere was found normal, but the right hemisphere was covered by a clot which had broken up the central convolutions, the optic thalamus, and the corpus striatum, and which was only separated from the lateral ventricle by the ependyma. Dr. McNutt then lays down the proposition that "it is legitimate to conclude that had this second child lived it would have developed by cicatrization the shrinkage of the central convolutions and the convergence of the neighbouring gyri, which was found in her first case, and which characterizes the most marked cases of this disease." She then generalizes meningeal hæmorrhage as the universal cause for infantile spastic states dating from the time of birth and associated with difficult labour. And this opinion expressed by her has gained the widest acceptance, so that one reads on every hand that meningeal hæmorrhage is a proved cause of diplegia. Yet her case of diplegia did not date from birth, for the anatomical findings showed that the disease must have been installed early in fœtal life. And her case of meningeal hæmorrhage was not the result of difficult labour, for the birth was described as having been particularly easy. It was not a case of meningeal hæmorrhage but of a large intracerebral hæmorrhage which ploughed up everything from the basal ganglia to the cortex, ultimately bursting upon the surface. This cerebral hæmorrhage did not occur at the time of birth, but twelve days after birth, when the signs of the hemiplegia first appeared.

Yet this is the evidence upon which the case for meningeal hæmorrhage as a cause for diplegia rests. There is no other evidence. Freud in his excellent monograph published in 1897, sought clinics, museums and literature in vain for other evidence and found none, and that, too, has been my experience.

It is true that meningeal hæmorrhage is not uncommon after difficult labour, and doubtless it is a common cause of convulsion occurring after such births. Cruveilhier noted it a century ago. Wehye made 957 autopsies upon children from a few days to 6 months old, and found signs of undoubted meningeal hæmorrhage in 122 of them, though these cases had shown no symptom or sign whatever of damage to the brain. Similar results have also been obtained by Mraček in another large series of cases.

The evidence demands that meningeal hæmorrhage should be deleted as a causal factor for any infantile spastic state. Yet meningeal hæmorrhage is still the most striking among the proved factors of diplegia in most text-books of neurology. Strümpell, in 1885, brought forward indubitable cases of post-natal diplegia and infantile hemiplegia due to polio-encephalitis, and he argued this initial pathology for infantile spastic states in general. There does not appear to be much evidence in favour of fœtal encephalitis, though Virchow described it and von Limbeck considered it to be the cause of lobar atrophic sclerosis, and much more recently the Longs have published some remarkable necropsies which seem to show that encephalitis may occur in intra-uterine life. So that it seems difficult entirely to refute this possibility. Though Strümpell's work received little support for many years except from Pierre Marie, yet most of it has stood the test of time. For now encephalitis is universally held to be a common cause of infantile hemiplegia, and of post-natal diplegia with severe initial symptoms.

In 1894 Brissaud started an entirely new conception of the origin of diplegia or rather of certain forms of diplegia. He considered it was prematurity of birth which was the essential causal factor and he avowed that birth before term arrested or retarded the essential functions of evolution of the fœtus.

In his words, "what takes the pyramidal tract three weeks to grow in intra-uterine life will take it three years or never in extra-uterine life in the prematurely born and thence arises the spastic paralysis." He confined this pathology to what he called "Little's disease," those purely paraplegic forms which show a tendency to amelioration or recovery. Now this is essentially picking and choosing, for there is every gradation between the purely paraplegic and the more severe forms of diplegia, and these cannot be separated pathologically. It is obvious that the immense majority of prematurely born children who survive complete the development of their nervous systems in good time and do not develop Little's disease. The major incidence of premature birth in connexion with paraplegic forms, though incontestably true, is not universal, and therefore it does not help Brissaud's argument; and there is the much more subtle explanation that the dyscrasia which produced the pyramidal trouble also caused the premature birth. In all autopsies upon paraplegic diplegia, some defect has been found throughout the pyramidal system from cortex to lumbar region. But on Brissaud's hypothesis we should find no abnormality of the cortex and upper parts of the pyramidal tracts. Notwithstanding this, Brissaud had many adherents, including Marie and van Gehuchten; indeed, van Gehuchten has gone so far as to say that premature birth at the end of the seventh month produces general rigidity because the pyramidal tract has only by then reached the medulla, whereas premature birth in the eighth month produces paraplegic rigidity because by this time these tracts have reached to the dorsal region. After birth some slow renewal of the arrested growth of the pyramidal system occurs, therefore these cases tend to improve and sometimes to recover. Meanwhile the steady accumulation of pathological evidence called for a conception of primary neuronic degeneration as the initial morbid process in diplegia. Cotard, as early as 1868, drew attention to the atrophy of the pyramidal cells out of all proportion to the shrinking of convolutions and sclerosis. Warda, in 1895, found absence or degeneration of cortical neurones in a case of pre-natal diplegia that was certainly not secondary to any sclerosis or other gross lesion. Mya and Levi, in 1896, found arrest of neuroblastic growth with degeneration of such neuroblasts as had developed their processes. In some of the most severe of the pre-natal cases there was found to be no macroscopic lesion at all but an almost universal degeneration of cortical cells, pointing to the advent of a rapid degenerative change shortly before birth. Cases of post-natal diplegia with insidious onset and clinically identical with the pre-natal cases, two of which I published in *Brain*, for 1900, were recorded as having their onset throughout childhood even as late as the twelfth year (notably a remarkable case published in *Brain*, by Campbell, with onset at the twelfth year), and in all these cases primary neuronic degeneration was the lesion found. Further, the occurrence of optic atrophy in about one-sixth of all cases made any other explanation than a primary neuronic degeneration difficult. Déjerine always supported a neuronic pathology and he very rightly contended for a clinical and pathological separation between the diplegias and the double hemiplegias.

In 1897, Freud, of Vienna, issued his monograph; this still remains the most complete and authoritative exposition of this subject. He seems to have been the first to use the term cerebral diplegia as indicating a bilateral symmetrical defect of the brain, not the result of gross lesions, in contradistinction to the hemiplegias and double hemiplegias which he considered always to be the result of gross lesions. He argued, and indeed

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I think he proved, that all diplegia dating from birth, which had been attributed to abnormalities of birth, really had its pathological origin long before birth in intra-uterine life. He piled up statistics to show that every writer upon this subject had admitted that 40 per cent. of all cases of diplegia dating from birth were without any possible factor in connexion with birth. He analysed most carefully the circumstances attending the difficult birth in diplegia, and he sums up in these words: "Premature, precipitate and difficult births are not causal factors in the production of diplegia ; they are only associated symptoms of deeper-lying influences which have dominated the development of the fœtus or the organism of the mother ; the only cerebral paralysis which results from difficult birth occurs when the brain is lacerated and this takes the form of a monoplegia or hemiplegia or rarely of a double hemiplegia, and the anatomical result of this lesion is entirely different from the findings in the brains of diplegia."

Freud wished to link up prenatally installed diplegia with the cases of insidious onset during childhood at any age up to that of puberty, thus arguing a cause for primary neuronic degeneration of the brain which might be incident at any period of fœtal life or during childhood, and he cites some remarkable cases, among which those reported by Feer are worthy of attention. Three cases of diplegia of the same clinical type occurred among the children of one mother. The first child was born with difficulty ; the birth was accompanied by asphyxia and the infant was diplegic from the time of birth. The second child was easily born but again presented signs of diplegia of a severer type from birth. The third child was particularly well and strong till after seven months, when signs of progressive diplegia set in. Krafft-Ebing reported a similar instance in which one child was diplegic from birth while in two other cases the disease slowly appeared at the ages of three and five years respectively. Many cases of this nature have been reported by Schultze, Pelizain, Newmark and others. Surely the same cause was active in the post-natal cases as in the pre-natal cases in these families. Pelizain was anxious to bring all cases of familial spastic paralysis into the same causal group with congenital diplegia, for in the families reported there were children who were born paralysed after difficult birth and children in whom symptoms commenced, though apparently healthy, at any age up to the fourteenth year. The one distinction which at first seems to separate the congenital cases from the post-natal cases, and which caused Paul Simon to insist that they were separate diseases is neither a pathological distinction nor one of symptomatology, it is the course of the disease. Most of the pre-natal cases tend to improve, some of them to a remarkable extent. The post-natal cases never improve but progress more or less rapidly to a fatal issue, while some few become arrested. Yet progressive pre-natal cases are known and I have seen several of them ; and both pre-natal improving cases and rapidly progressive post-natal cases have occurred in children of the same mother and often in members of the same family, and since in these examples the same cause must be acting there is no necessity to assume a different pathology for the two conditions. On the contrary, it seems to me that there is every reason, both clinical and pathological, for both conditions belonging to one class of diseases in which a primary degeneration of cerebral neurones may occur at any period during fœtal and during adolescent life.

In 1909 Anglade and Jacquin reported a necropsy upon a case of spastic diplegia which was of great interest. Clinically the case was one of general rigidity of the common type. They found a neuronic degeneration of the cortex

of the cerebellum in addition to a similar but much more extensive condition in the cerebrum. This, I believe, was the first record of primary involvement of the cerebellum in diplegia. The late Dr. Batten many times expressed to me his belief that the cerebellum was extensively involved in the astatic and hypotonic type of diplegia, several examples of which he showed at clinical meetings of this Section; and the fact that definite involvement of the cerebellum has been found gives a definite basis to this conception. Smallness of the cerebellum has repeatedly been found but it has been regarded as secondary to pre-frontal lesions. Foerster long ago referred the astatic-atonic forms to an absence of the fronto-pontine projection, but I have been unable to discover upon what pathological or physiological evidence his statement was based.

I now turn to a consideration of the ætiological factors of diplegia, long lists of which will be found in all modern descriptions, with the remark that 40 per cent. of all the cases are without even the most slender causal antecedents.

Hereditary and Familial Influences.—These are exceptionally rare in the common types of the disease. Yet there are recorded instances in which a mother who was the subject of pre-natal diplegia has given birth to a child afflicted with the same disease, an example of which was reported by Oppenheim. Familial incidences in which pre-natal and post-natal cases occurred indiscriminately have been many times recorded as in the reports of Krafft-Ebing, Feer, Newmark and Pelizain already referred to. Post-natal cases of a progressive type with a heavy familial incidence have been recorded by Souques, by Pribram and by Hochhaus, while this ætiology is also very obvious in the type of amaurotic family idiocy and familial spastic paralysis. Diplegia may also in rare instances be coincident in the same families with other degenerative diseases of the nervous system, notably hereditary ataxy and myopathy, and certain facts showing this relation have been pointed out by Koenig, Strümpell, Bernhardt, Franz Schultze and by myself. Consanguinity of parents has been a by no means rare occurrence.

Maternal Ill-health during Pregnancy.—This factor has been placed in the most important position by many writers and I think without any justification, so rarely is it met with, so often are these diplegic children the first-born of healthy young mothers, so frequently are perfectly healthy children born of very sick mothers, so little is there which is definite and tangible about the nature of the maternal ill health even when this does precede the birth of a diplegic child. The cause seems rather to be individual to the conception rather than transmitted by the mother and I would remove maternal ill health from among the causal factors of this disease. There is, however, one important exception and that is the presence of syphilis. The influence of syphilis in causing diplegia has been hotly contested. Its incidence is admittedly rare. Naef and Feer examined 179 consecutive cases and were unable to prove the presence of syphilis in any one case. Dejerine insisted that syphilis was only a factor in that it caused premature birth. Yet I have seen diplegic children with the most obvious signs of congenital syphilis and so have many others. The most conclusive evidence is that in pre-natal diplegias a condition of the brain is sometimes found in which granular ependymitis, decortication and the general resemblance to the brain of general paralysis is so striking as almost to prove the point. What is needed is an examination of the Wassermann reaction in a long series of cases, and as far as I have been able to search the literature this work has not yet been done.

Abnormalities of Birth.—I have already excluded these conditions as

causal factors, and have treated them as common, but by no means constant clinical associates of diplegia, indicative of something wrong with the fœtus, or of something wrong with those mysterious relations between mother and offspring which determine a speedy delivery at full time. My authority for doing this is based on the inconstancy of these so-called factors, the insuperable difficulty of referring one and the same pathological condition to such widely different causes as premature, precipitate, and prolonged birth and to asphyxia, the absence of any reasonable explanations or pathological findings to show how such events could produce the disease, the opinion of modern writers upon the subject and the complete negation from the conditions of the brain found in every case.

Deductions from the Pathological Material.—It is remarkable that no lesions have been found in any part of the nervous system except in the cerebral hemispheres. Since the brain increases in size and develops during fœtal life by the evolution and development into complete neurons, of the neuroblasts of which it is originally composed, the terms which are so frequently met with in the literature of this subject, namely, "agenesia" and "arrested development," simply mean that some harmful process has affected the brain during its period of development. I have attempted to prove in my foregoing remarks that such arrest of development due to an affection of the brain during fœtal life and not to any event connected with birth, is to be found in the brains from all those cases of diplegia in which the symptoms are present from the time of birth. And I have further argued that the first signs of the affection are expressed in neuroblastic and neuronic death or degeneration.

If we liken the brain from the commencement of its development to a garden in early spring, well sown with seeds—the neuroblasts, some of which germinate early, some meantime and some late, those germinating early perhaps of a hardier sort, those germinating late perhaps of a more tender kind, imagine what might be the effect of a noxious influence—a sudden sharp frost for example upon this garden. According to its severity and the time when it arrived, it might, if it caught all the seeds in a condition of germination and growth, leave this garden lifeless. Or it might spare those seeds as yet ungerminated and those seedlings which had attained sufficient growth and robustness to resist its influence and affect only certain kinds of the seedlings, perhaps according to the state of their development, to their death and destruction. Or the seedlings affected by the frost may not be all killed, some or all of them may be simply "cut" and their leaves for the time withered, and these may be able in some degree to renew their growth and attain a maturity in some cases perfect, in others stunted and imperfect, in all retarded. Lastly, this untoward weather may continue, and crop after crop may perish in a progressive destruction. Some such process as I have put before you in this homely simile of the seedlings and the frost will, I submit, best serve to explain both the ætiology, the symptomatology and the pathological findings in cerebral diplegia. This hypothesis will, in a measure, explain the restriction of the affection to the cerebrum, for this is the latest to develop, the highest and perhaps for these reasons the most vulnerable part of the nervous system. It will explain the tiny brain in cases of severe microcephalic diplegia, with no differentiation between grey and white matter and the most primitive type of convolutions, for here is to be witnessed widely spread neuroblastic death early in fœtal life, with no myelination, because no processes have been developed from the neuroblasts. It will explain the strangely symmetrical and even strictly systemic affection of the cerebrum;

the pyramidal region alone affected, the frontal and occipital regions alone affected, the parietal and frontal regions symmetrically involved, while the pyramidal and occipital regions escape, the corpora striata alone being affected as in the cases of choreic diplegia and congenital athetosis examined by Anton and Demange. It will explain those severe cases in which no signs of arrested development and little naked-eye change are found in the brain, for here the cerebrum has been hard hit at a time when its development is almost complete. It will explain the progressive improvement of the slighter cases with the possibility of regrowth in partially or temporarily damaged neurons and also those cases, both pre- and post-natal, which exhibit a progressive degenerative course.

This hypothesis is not altogether my own, though I claim originality for its development in this form. It was suggested to me by the writings of Brissaud on premature birth as the one and only cause of paraplegic diplegia. Premature birth was Brissaud's frost which cut the pyramidal seedlings. On my hypothesis it was the frost which caused both the premature birth and the pyramidal deficiency simultaneously.

The cause of the initial process is entirely mysterious, as is the case also in so many degenerative nervous diseases, as, for example, the hereditary ataxies, myopathies and progressive muscular atrophy. It may be of the nature of a toxic or of a deprivation process and is likely to be of more than one variety. If there is any difficulty in the conception of such a cause acting for a time only and subsequently allowing of regeneration of nerve elements, it is exactly paralleled by the neuronic affection of polyneuritis. If the action of the cause for a time only producing degeneration, followed by final arrest, is hard to believe, this is the invariable habit of the cause of peroneal atrophy where the degeneration commencing at a certain period of life, progresses for a time and comes always to a final arrest within comparatively narrow limits. The picking out of the systems of cerebral neurons by this disease may conceivably be due to the time of incidence of the essential cause; or to its physiological selective capacity for certain of the elements only, as occurs so commonly with poisons; or to a combination of these two events. And, further, the phylogenetic age of the several cerebral systems may be a factor. It would be fascinating if one could correlate the affection of the various regions of the cortex and the clinical types of diplegia with the times at which their neuroblasts were at their most active stage of development during fœtal life, but I have sought in works on embryology and have not found details as to when the systems of cerebral neuroblasts develop. It is true that the neuroblasts whic h subsequently develop into the basal ganglia commence their growth long before those of the cortex and we have a clinical type corresponding to this region in congenital bilateral athetosis, and atrophy incident upon these regions as shown by the cases of Anton and others. The pyramidal system is comparatively late in commencing and in attaining its development, and we have positive pathological evidence to show that it is affected at different epochs in its development in different cases. For example the pyramidal tracts may be altogether undeveloped, or they may reach to the crus, or to the medulla, or to the cervical region of the cord. or they may have attained to the usual degree of maturity present at the time of birth but may be degenerated. Clinical evidence and pathological evidence show that the affection falls most heavily upon the pyramidal system in the majority of the cases and this major incidence has for many years and by great authorities been considered to be associated with an especial vulnerability of this system owing to its late period of development. But the pyramidal system has been found completely normal in

this condition of lobar atrophic sclerosis as in the cases recorded by Ganghofner and by Railton, although it may be argued against these results that modern methods of examination were not used. And Dr. Greenfield, a few weeks since, showed me a specimen of his in which the central regions of the cortex was comparatively intact, while the frontal and parietal regions were severely atrophied, the occipital region being also relatively unaffected. I seek to explain such cases on the grounds that the cause for the condition was active at a time when the pyramidal and occipital neurons were invulnerable owing to the stage of their development, while the parietal and frontal neurons were affected because they were in a more vulnerable stage of their development, or, alternatively, that there was a selective capacity on the part of the destroying agent for those neuronic systems that were affected. I would put the following conditions forward in explanation of the universal affection and of the various local affections of the brain which are found in diplegia:—

(1) The severity of the cause, which may affect all the cerebral elements indiscriminately, giving rise automatically, if acting early, to a small primitive type of brain, destitute of neurons, and causing clinically a condition of idiocy with diplegia and blindness. (2) The time of development at which the cause is active, which may determine those systems which are affected and those which are spared, in the local atrophies of the brain. (3) The physiological selective capacity of the cause to affect some systems and not others. (4) A cause which may be temporary in its action and which for this reason only picks out systems which are vulnerable at the moment. (5) A cause which may be continuous in its action and which results in a very progressive diplegia.

If my reasoning be legitimate then we must class together all the clinical results which primary neuronic degeneration and its sequel, general or lobar atrophic sclerosis, produce, from microcephaly with idiocy alone, through the wide range of pre-natal and post-natal diplegias to the simplest forms of paraplegic rigidity.

The one anatomical lesion which has been found in all cases of diplegia since it was first pointed out by Cotard, in 1868, is disappearance of the neurons of the brain or their degeneration. I hold that this is a primary degeneration of the neurons and that it is the essential and primary lesion in all cases of diplegia.

Arrest of development of the brain and of its outgoing paths is often found. This is the natural consequence of the foregoing neuronic lesion when this occurs during the earlier periods of development of the brain and consequently it only occurs in pre-natal cases.

Atrophic sclerosis of the brain is the natural consequence of the neuronic decay. It is more marked the earlier in life neuronic decay occurs and the longer the patient has lived after the degeneration has been installed. In some varieties of diplegia, of which the Waren Tay-Sachs type is an example, no sclerosis follows the neuronic degeneration and this is perhaps due to a peculiar nature resident in the cause.

Gross lesions are rarely found. In one of my cases there was a gross lesion of the nature of a scar in one parieto-occipital region, but the primitive type of brain and the general atrophic sclerosis on both sides, as well as the symptomatology, which was strictly bilateral, showed that this gross lesion was an accidental accompaniment. It is under such circumstances as these that all the gross lesions are found in diplegia. In this connexion I must briefly refer to porencephaly, one of the causes of which is certainly embolism, for I have

seen a porencephaly in which the detailed history of the occurrence of an embolism in the course of mitral stenosis many years before, was available. And if embolism can cause porencephaly, then thrombosis and arterial obstruction of any kind can produce it also, and this is, in my opinion, the usual cause of porencephaly. But there must also be other causes, for I have seen a porencephaly in an adult, who never at any time showed any signs of cerebral defect and who died of tuberculous meningitis. In this case the porencephaly must have arisen from a cyst formation, which neither interfered with the cerebral elements nor caused any rise in the intracranial pressure. In diplegia, small, and sometimes multiple, cyst formation is found, which is called porencephaly. I believe that these formations originate from the collection of fluid in degenerating and sclerosing tissue and that they are secondary to the sclerosis. In the few instances in which large porencephalies are found in the brains of diplegic subjects, I think that these are due to associated vascular lesions, as was proved in one of Freud's cases. The results of encephalitis do not for the most part resemble diplegia sufficiently to give rise to confusion between the two conditions. When the similarity is close, the history of an acute onset with pyrexia and perhaps convulsion, rapidly followed by a paralysis which is most marked at first, and thereafter tends to lessen, should leave little room for error in diagnosis. Finally, I submit that the essential anatomical cause of diplegia is a primary neuronic degeneration due to factors which are at present entirely elusive, with the exception of rare cases in which syphilitic infection of the brain is certainly the provoking agent. This conception of the pathogenesis seems to me to be compatible with all the pathological evidence which has been recorded and to explain best the symptomatology and clinical aspect of this class of diseases.

Pathological Findings in the Central Nervous System of a Case of Myasthenia Gravis.[1]

By Dr. Y. A. Barrada.

(Introduced by Sir Frederick Mott, K.B.E., M.D., F.R.S.)

I am greatly indebted to Sir Frederick Mott for kindly placing at my disposal the material of this case, and for many suggestions and valuable advice in the investigation of it.

The main object of this paper is to demonstrate some changes found in the central nervous system of a case of myasthenia gravis that may throw some light on the mechanism underlying the symptoms manifested in this disease.

The patient joined the Army on August 13, 1914.

Major Walshe's report from Orwa-el-Waska is as follows:—

"July 26, 1918.—While on the Palestine Front in June, 1918, he began to notice his legs were weak, so that he could not throw his leg across his horse, but had to be helped on. A very short walk "did him in completely"; he felt weak as a child. No pain or paræsthesiæ accompanied this progressive weakness of all his limbs. Two weeks later fluids began to return through his nose, his voice went, chewing his

[1] From the Pathological Laboratory of the London County Council Mental Hospitals, Maudsley Hospital, Denmark Hill, S.E.

food became almost impossible, and after two or three mouthfuls he had to stop. There were no ocular symptoms, no history of infection of any kind, no sore throat.

"On examination: Viscera healthy, general condition good. *Nervous system*: Cerebration and special senses normal. Cranial nerves: Third, fourth and sixth normal at present; fifth masseters and temporals exceedingly feeble; the finger can be inserted between his teeth with impunity, so feeble is his bite. No sensory change. Seventh, marked double facial paresis; ninth, eleventh, paresis of palate, larynx and pharynx. The whole musculature is very weak and readily fatigued; no group of muscles in the limbs or back is specially picked out. *Sensation* normal. *Reflexes* all normal. 'An early case of myasthenia gravis' was diagnosed."

August 28, 1918.—Admitted to H.S. *Mahero*. Report of medical officer on ship:—

"This man suffering from myasthenia gravis is liable to attacks of respiratory failure. . . . He is unable to swallow thin fluids, as they regurgitate through his nose, and power of mastication fails if he endeavours to eat solids."

September 6, 1918.—Admitted to Maudsley Hospital. Extract from medical officer's report:—



FIG. 1.

"*Previous history*: Leather dresser; always healthy; 12 months ago weighed 13 st. 2 lb. *Family history:* Good. *Present condition:* He lies on his back in bed with his head propped up; the head has a tendency to fall back. The forehead is smooth. Ptosis of the right eye (partial). The upper lip is retracted, the corner of the mouth drooped: lips parted (fig. 1). On smiling the corners of the mouth remain drooped and the smile has the appearance of a sneer. *Voluntary power:* Unable to wrinkle the forehead or close the eyes firmly or whistle or blow out the cheeks. Is not able to masticate food. He is on slop diet. No regurgitation of food while in hospital. His tongue is soft and flabby, tremulous, and has longitudinal furrows. His speech is good in the morning; but towards evening the muscles of articulation tire and the voice becomes slurred. The intercostal and other respiratory muscles are affected. The expansion of the chest is only ½ in. The muscles get rapidly fatigued. He has diplopia, which is not constant in position. The heart sounds are feeble, pulse slow, 63 per minute, easily compressed. *Reflexes:* Abdominals present; cremasterics not obtained; knee-jerk absent; plantar flexor. *Sensory system*: No loss of sensation; sometimes he states he does not feel in one spot; later he is sensitive at the same place. *Blood count*: Red corpuscles, 5,800,000; white, 6,718; small lymphocytes, 27 per cent.: large lymphocytes, 12 per cent.; polymorphs, 62 per cent.; a slight

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PLATE I.



FIG. 1.—Low power. Masseter muscle.



FIG. 2.—High power. Orbicularis oculi.

increase of lymphocytes. Patient is able to get out of bed and walk with assistance a few yards.

"February, 1919.—Patient was feeling well on the 4th and went to a cinema and came back still feeling well and happy. About 2 a.m. the next day he complained of pain in left chest, and did not sleep well; 11 a.m.: he had an attack of dyspnœa, which continued in spite of continuous inhalation of oxygen up to his death in the afternoon. His muscles of respiration gradually failed, until he had hardly any chest movements. The notes state that he died of syncope."

The post-mortem examination was made by Major Collins the same day. The weather was cold and, therefore, the microscopic changes which will be described were not due in all probability to post-mortem changes.

The parts examined were: the thymus, pieces of the orbicularis oculi, the masseter, the diaphragm, and the heart muscles. Of the central nervous system sections were prepared from the leg, arm, and face areas of the ascending frontal and parietal convolutions, the thalamus, the mid-brain, the pons and the medulla oblongata, the cerebellum and the spinal cord. Unfortunately no other tissues were obtainable.



FIG. 2.—Photomicrograph of section of the thymus gland.

Thymus: The gland is greatly enlarged and weighs more than 70 grm. It is completely surrounded by a thick fibrous capsule. Microscopically there is no distinction between cortex and medulla, and only a few fibrous bands can be seen traversing the cellular tissue, which consists mainly of lymphocytes and epithelioid cells, the former predominating throughout, and only in small areas are they comparatively less dense. The epithelioid cells have large vesicular nuclei and faintly stained cytoplasm. In clear areas, where there are comparatively few lymphocytes, the protoplasm of the adjacent cells could be seen continuous with one another. The sections exhibit plenty of spaces filled with more or less clear eosin-stained material, and containing some epithelioid cells undergoing colloid degeneration, and few lymphocytes. These spaces are lined by the epithelioid cells, which, around many spaces, are arranged in a layer of fusiform cells with the long axis perpendicular to the circumference of the space (fig. 2). Evidence of mitosis can also be seen in the epithelioid cells. No eosinophil cells are found, and only few Hassall's corpuscles, and these are swollen and degenerated. The capillary blood-vessels are not very numerous. They are lined by unbroken endothelium and contain some lymphocytes. Few blood-vessels show perivascular spaces filled with lymphocytes and distinctly lined outside by a layer of endothelium.

Muscles: Nothing abnormal could be discovered in the orbicularis oculi, the masseter or in the heart muscle. In the diaphragm numerous foci of lymphocytic infiltration are found lying in between the muscle fibres. In some of these lymphorrhages (as Buzzard has named them) a blood-vessel may be seen with lymphocytes filling the perivascular space, and in a longitudinal section of the vessel, a perivascular infiltration of lymphocytes can be traced to a considerable distance from the main mass of lymphorrhage (fig. 3). These foci vary much in size, and where they are big enough, they push aside the muscle fibres, but do not invade them. The muscle fibres themselves appear quite normal throughout.

The nerve terminations in the masseter and orbicularis muscles were stained by Dogiel's method with *intra vitam* methylene blue (Plate I, figs. 1, 2). The muscles and the nerve fibrils and their terminals appear to be normal. The fact that this staining was so successful is an indication that the tissues were fresh and that the nervous system was placed in a fixative fluid before autolytic processes had had time to occur. This fact is of considerable importance in regard to the appearances of the cells when stained by Scharlach.

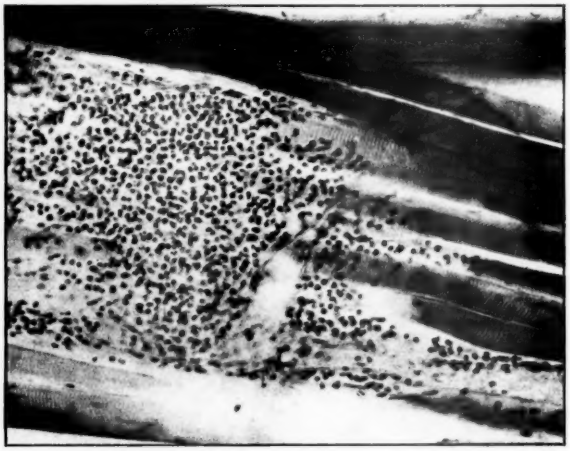
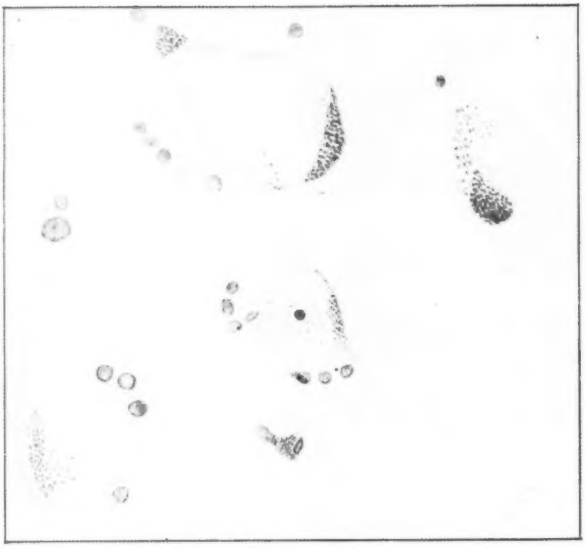


FIG. 3.—Perivascular infiltration of lymphocytes around a blood-vessel of the diaphragm.

Central Nervous System: Cerebral cortex with Nissl's and toluidin blue and eosin methods, the cells in all the layers of the cortex exhibit diminution in the basophil substance. They take up the stain lightly and present deficiency in Nissl granules especially around the nuclei (perinuclear chromatolysis). The nuclei retain for the most part the central position. Here and there one or two satellite cells may be seen in proximity to a ganglion cell, but there is no excess of neuroglia. In Scharlach preparations fine lipoid granules are observed in the cells unequally distributed in the different layers of the cerebral cortex. They are most marked in the outer molecular layer, where they nearly fill the whole cytoplasm around the nuclei. The Betz cells show these granules relatively to a lesser extent than the outer molecular layer but more than the cells of the pyramidal layers. The lipoid granules in the giant cells are aggregated in a part of the cell, generally towards the base; a few scattered granules are also present, which diminish as they recede from the main aggregate (Plate II). Lipoid granules are also observed in the perivascular spaces. The granules stain deep blue with Nile blue stain.

PLATE II.



Betz cells, from three consecutive overlapping fields.

The thalamus shows the same changes in the small and the large multipolar cells, but to a very marked extent, and indeed more than in any other part of the central nervous system. The Nissl granules are almost completely absent, the cytoplasm is vacuolated and the nuclei are swollen and eccentric. There are clear spaces in the cells which represent the location of the lipoid granules. There is marked invasion by satellite cells, with excess of neuroglia (Plate III). With Scharlach R. stain, the lipoid granules are seen occupying a great part of the cell and sometimes almost filling the cytoplasm. This may be partly due to autolytic processes occurring before the fixative fluid had time to penetrate the deeper structures of the brain; but against this these lipoid granules are not seen in brains of cases from shell shock. If these changes are due to the asphyxia preceding death one would have expected similar changes throughout the central nervous system, but this was not so. Again where the lipoid granules are most marked in the cells, there the Nissl granules are most affected and vacuolation in xylol preparations most obvious. The perivascular spaces also contain the lipoid granules. With Nile blue stain the granules are deep blue in colour.

In the third nerve nucleus, there is marked perinuclear chromatolysis, the cells are swollen, with eccentric nuclei (fig. 4). Invasion by satellite cells may also be observed.

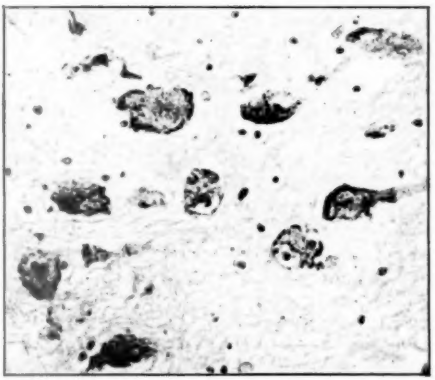


FIG. 4.—Photomicrograph of cells of the third nerve nucleus.

In Scharlach preparations, the lipoid changes are also marked in the cells, and the perivascular spaces contain the lipoid (Plate IV, fig. 1).

Near the middle line, anterior to the oculomotor nucleus and to one side of a blood vessel, is a homogeneous mass, probably coagulated lymph; similar but minute scattered patches are also observed in the mid-brain, some distance from the bigger mass.

The fourth nerve nucleus presents similar but less marked changes than those of the third.

The seventh nerve nucleus, on the whole, shows very little basophil changes, although a few cells may present varying degrees of chromatolysis.

The same may be said of ninth, tenth, eleventh and twelfth nuclei.

In the spinal cord, the anterior horn cells present only slight basophil diminution, but here and there, a few cells can be found to show marked chromatolysis with eccentric nucleus and swollen vacuolated cytoplasm.

The posterior horn cells on the other hand present distinct chromatolysis. With Scharlach the lipoid granules of the anterior horn cells form more or less sharply defined aggregates situated to one side of the nucleus; some Nissl granules may be observed at the periphery of the cell (Plate IV, fig. 2).

It may be noted that there is generally a correspondence between the amount of chromatolysis and of lipoid change; the parts in the central nervous system which suffer more from the former present more changes in the latter.

Cerebellum: The Purkinje cells were found more swollen than normal, with diminution of basophil substance; the granules were especially deficient around the nuclei.

There is a general congestion of blood-vessels throughout the central nervous system, but there are no hæmorrhages. No lymphorrhages could be found anywhere.

It may be concluded, therefore, that there is a general diminution of basophil substance throughout the central nervous system, and there is also similar widespread lipoid change.

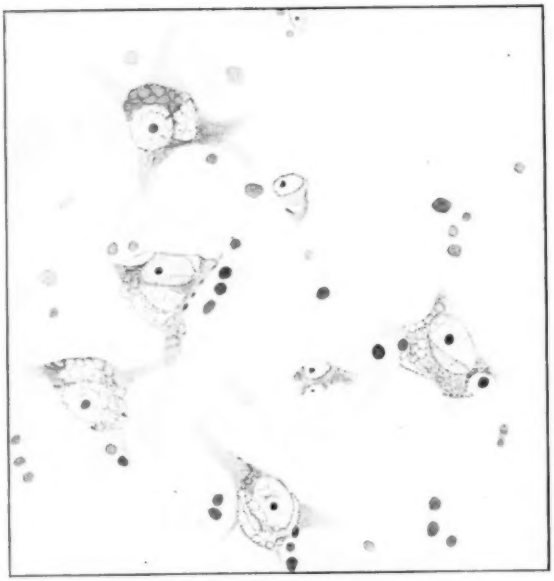
Remarks.

Since Weigert described, in a case of myasthenia gravis, cell infiltrations of the muscles which he regarded as metastases from a so-called lymphosarcoma of the thymus, many observers have subsequently reported similar changes in the muscles, which they, however, interpreted otherwise. Buzzard in particular [1], who thoroughly investigated a number of cases, described in detail these infiltrations and gave them the name "lymphorrhages." He found them in other organs than the muscles, but he is not inclined to ascribe the functional disturbances directly to them. He maintained that they were a constant anatomical feature of the disease, and indeed they were reported to exist in the great majority of recent cases. In this case the lymphorrhages were found only in the diaphragm. The muscles themselves were normal in a great number of cases, and where degenerative changes were detected, they did not involve all the muscles that had been affected clinically, except were there was marked atrophy, and they affected but a small proportion of the fibres. The fact that the thymus has not been found involved in many cases where lymphorrhages have been present would render unlikely the proposition that the thymus is the origin of these lymphocytic foci.

Before 1901, there was no mention of thymic changes in the autopsy records reviewed by Campbell and Bramwell [2]. Since then morbid conditions of the thymus in myasthenia have been forthcoming. These have been classified into (1) simple hypertrophy; (2) hypertrophy with degenerative and proliferative changes; (3) new growths. Bell [5] in 1917 collected fifty-six records of autopsies from the literature published since 1901, twenty-seven of which, i.e., nearly half, were reported with some form of thymic involvement, ten of these as tumours. In the case he described, there was present, in the region of the thymus, what he called a benign thymoma, composed of thymic tissue of fœtal origin. Four of the ten tumours of his collection conform more or less to his description and others with little modification would fall in this group. He states that this kind of tumour seems to occur only in myasthenia gravis. The thymus described in the present case resembles in most respects the one that Bell has described.

Nervous System.—A great number of post-mortem records of myasthenia have, hitherto, described the nervous system as normal. The most frequent lesions detailed, are hæmorrhages in different parts of the central nervous system, and these have always been considered of agonal nature. Among other less frequent lesions recorded by various authors, are different anomalies of the central nervous system and cranial nerves [6], degeneration of fibres of one or more cranial nerves, homogeneous masses (probably coagulated lymph), lymphorrhages in the medulla [7], lymphorrhages in the posterior root ganglia [1]. Among cellular changes Kalischer, in a case the diagnosis of which was not

PLATE III.



Large multipolar cells from thalamus.

PLATE IV.

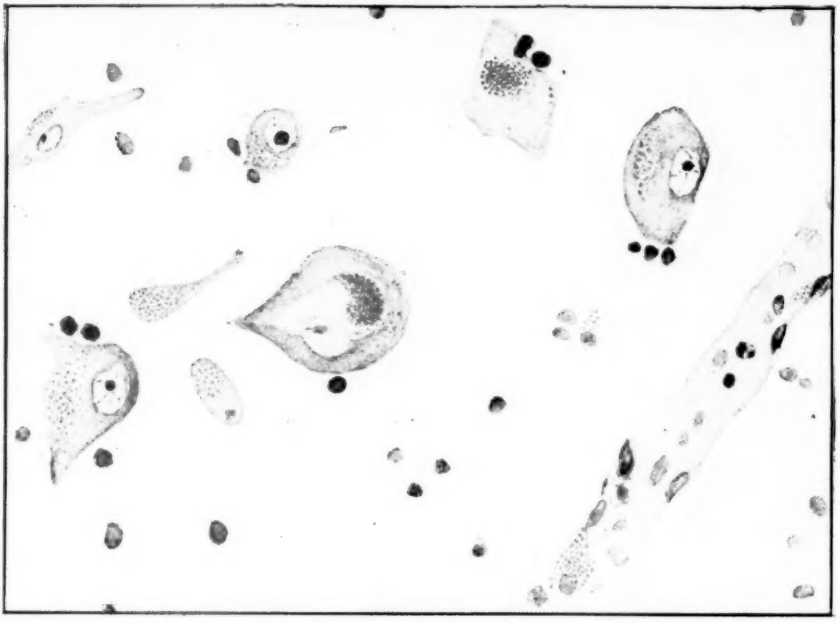


FIG. 1.—From the oculo-motor nucleus.



FIG. 2.—A group of anterior horn cells from fifth cervical segment of the spinal cord.

absolutely certain, found degenerative changes in the motor cells of the grey matter from the oculomotor nucleus downward through the whole length of the cord to the sacral region [2]. Chromatolysis of the nuclei of the medulla and pons were described by Widal and Marinesco, Murri, Guthrie, Leclerc and Savornat [8].

PATHOGENESIS.

Many hypotheses have been advanced to explain the pathogenesis of myasthenia gravis.

Oppenheim regarded it as a neurosis—a chronic progressive and fatal neurosis [6], but in view of the discovery of definite pathological lesions associated with the disease, this could no longer be maintained. Collins' sympathetic hypothesis, amongst others, has fallen into disrepute.

Buzzard suggested that the clinical manifestations of fatigue might possibly be explained by assuming a diminished functional activity on the part of the sarcoplastic, as compared to the fibrillar elements of the muscles, with the result that the fibrillar constituents, acting at a considerable disadvantage, become readily and rapidly exhausted when excited either by the will or by the faradic current. Knoblauch, after him, advanced the theory that the myasthenic reaction is due to the preponderance of pale, at the expense of red, muscle fibres in myasthenia cases, thus bringing myasthenia into relationship with Thomsen's disease [3].

Marinesco proposed the theory of "insufficiency of oxidation" with consequent accumulation of insufficiently oxidized products of dissimilation in the circulation and production of chemical alterations in the constitution of the musculature.

The view is gaining ground that myasthenia gravis is due, primarily, to some form of toxin which is either exogenous or endogenous. That myasthenia has been observed after botulism, after taking barium salts, after inhalation of petroleum fumes, chlorine gas, war gas, and after infectious diseases, proves that an external toxin can be the cause. Cases where no external cause can be found are ascribed to some auto-intoxication or metabolic perversion, some disturbance in the balance of the internal secretions, which exerts a deleterious effect on the functions of the muscles. In this connexion, Kauffmann put forward the view that the cause is a metabolic insufficiency of the hepatic function; and Chvostek ascribed the disease to hypersecretion of the parathyroids. Indeed the endocrine glands, the thymus, parathyroids, suprarenals, hypophysis and thyroid have been separately and collectively blamed by different investigators.

The fact that, in the myasthenic electrical reaction, the muscle reacts well to galvanism after complete exhaustion to faradism would suggest that the primary seat of fatigue is, at least, not only in the muscle. It is unlikely to be in the non-fatiguable nerve fibre in the absence of any satisfactory evidence of the nerve fibres being pathologically involved in this disease; so it may be in the nerve endings. Such supposition finds support in the fact that Grünn is reported to have obtained the reaction of Jolly in a frog injected with yohimbine [8], which is known to paralyse the terminations of motor nerves in the muscles of this animal [9]. This, however, does not exclude the participation of the central nervous system, or even its playing the primary rôle, in the phenomena of fatigue exhibited by the patients in voluntary action. In fact the pathological findings in the cerebrospinal axis in this case strongly suggest such a possibility. For, if the basophil substance is the energy

substance of the cells, its diminution results in rapid exhaustibility of neural energy, hence the quick onset of fatigue that is characteristic of myasthenia. The hypotonia that is observed in many cases may be accounted for by such changes as described in the cerebellum. Indeed, Strümpell was not far from being right when he stated that "there is not a single case which is associated with symptoms pointing to parts other than the motor nervous system."

The presence of lipoid granules in the nerve cells is another indication of depressed functional activity of the neurones incidental to a failure of complete oxidation processes [4, 10].

Pighini, in his special study of the cause of their appearance, concludes that lipoid granules in the nerve cells of old people are products of regressive metabolism, and that hypo-oxidation and pathological processes in which katabolism exceeded anabolism would account for their occurrence; they are probably, therefore, an expression of anabolic hypofunction [10].

It may be assumed, therefore, that under the influence of some intoxication, or more probably a subminimal deficiency of katalase, the oxidation processes in the cell and its dendrons become deficient, and the anabolic do not keep pace with the katabolic processes involved in functional activity, and consequently rapid fatigue which may be primarily central sets in; but if a very short time is allowed after tiring a certain group of muscles which is not markedly paretic, it can resume work with a good amount of strength. The hypofunction from defective oxidation processes, however, may vary in intensity, and it may amount to suspension of function; and thus may be explained the temporary paralyses that are met with and the improvements that often occur in the course of the disease.

My thanks are due to Mr. Geary for taking the photographs and for his constant and ready help throughout my work in the laboratory.

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Section of Neurology.

President—Dr. JAMES S. COLLIER.

Pyknolepsy ; A form of Epilepsy in Children, with a good Prognosis.

By W. J. ADIE, M.B.

ABOUT two years ago, a mother brought her little boy to me because he was suffering from "nervous attacks" which the doctor in the country called epilepsy ; these attacks, she said, began suddenly about two years before without apparent cause, and had recurred daily with great frequency—"dozens every day"—ever since ; this in spite of prolonged treatment with bromides, luminal and other remedies that had been ordered by various consultants she had seen. Apart from the attacks the boy was perfectly well ; he learnt his lessons with ease and was good tempered, intelligent, and everything that a healthy boy of his years should be.

After a long conversation with the mother I saw the boy and during the short time he was in my room he had three seizures, all of them exactly alike and, according to the mother, exactly the same as all the others he had had. Whilst sitting in a chair his limbs relaxed, he stared vacantly before him, the eyeballs rolled upwards, the lids flickered once or twice and then after a few seconds he looked about him, smiled, and was himself again.

Everything that I had heard and seen seemed to confirm the diagnosis of epilepsy, but several features of the case struck me as being rather unusual—in particular the great frequency of the attacks from the beginning, their sameness, their uniform mildness and their obvious harmlessness.

Could this be the pyknolepsy of which I had read ? Without any feeling of strong conviction in my own mind but desiring to afford the mother some crumb of comfort I ventured to tell her that it was just possible that the attacks would cease some day as suddenly as they had begun.

Nine months later I met the family doctor who asked me if it was true that I had told Mrs. X. that her son would have no more attacks. I hastened to defend myself but he cut short my protestations by informing me that the attacks had indeed ceased a few days after the visit to me and had never recurred. I heard a day or two ago that the boy is still perfectly well.

This may or may not have been a case of pyknolepsy, but after this surprising experience I set to work to learn all I could about this remarkable disease, which resembles epilepsy very closely in its cardinal manifestation yet differs from it most happily in its prognosis, and since that time I have kept a sharp look-out for similar cases—not without success. But my own cases are few and the period during which I have had them under observation is too short to allow me to formulate any conclusions that are likely to convince you.

My remarks this evening, therefore, are based upon a study of cases that have been described during the last seventeen years in foreign journals devoted to the study of neurology and children's diseases, in particular of those cases in which the period of observation has been a long one. Nothing has been written upon this subject in English, so far as I know, and it is because the available information is widely scattered and because I think the time has come when we should define our attitude in the matter, that I bring it before you to-night.

The first descriptions of the disease were given in 1906, by Friedmann, in a paper entitled "On Non-epileptic Absences or Short Narcoleptic Attacks," and in 1907, by Heilbronner, in a paper on "Gehäufte kleine Anfälle" (frequent, or better, aggregated minor attacks). In 1912 Friedmann returned to the subject and after expressing surprise that his first paper had given rise to so little comment he described some fresh cases, and gave a highly favourable account of the after-history of the patients on whom his original description was based. He should be more satisfied now, because the disease is often called Friedmann's disease, or the Friedmann-Heilbronner disease. Special papers have been written upon it by Mann, Engelhard, Sauer, Schröder, Redlich, Bolten, Cohn, Oppenheim, Stier and others. It is mentioned in every modern discussion on epilepsy and has even received consideration in the latest editions of the hand-books of Kraepelin, Oppenheim, Ziehen, and Lewandowsky.

According to Friedmann the disease has one symptom and one only—the attacks. These begin suddenly in healthy children between the ages of four and ten years, and recur with great frequency—from six to one hundred daily—in spite of well-planned treatment by bromides and other remedies. Ultimately, after persisting for periods which vary from a few weeks to many years they cease, often suddenly, sometimes gradually, and leave no trace of their former presence. Apart from the attacks the child is perfectly well and its physical and mental development is unaffected. The attacks themselves are remarkable for the monotony of their course and their uniform mildness. They consist in an inhibition of the higher psychical processes lasting from five to ten seconds. The power of speech and of voluntary movement is in abeyance, but automatic movements are retained; the child sits or stands with the limbs relaxed, staring vacantly before him, the eyeballs may roll upwards, the lids may flicker but there are no convulsive movements and consciousness is never entirely lost. After the attack the child is well at once and continues his interrupted game or task as if nothing had happened. The attack by itself, as you see, is indistinguishable from ordinary *petit mal*.

Now according to prevailing notions the prognosis in cases agreeing with the description I have just given should be bad, for the fits begin in early life, they are very frequent and they are uninfluenced by the ordinary remedies; yet, in Friedmann's second paper giving the after-history of the patients on whom this description was based, we find that the attacks, after persisting for five years in two cases, and for seven and nine years in others, ultimately ceased and remained absent for four and a half years, for five years, for nine years and for ten years in different cases.

One of Heilbronner's patients was still well six years after the attacks had ceased. One of Husler's patients was still free after three years; Stier's patients, who recovered after having attacks for $2\frac{1}{2}$, 3, 3, $3\frac{1}{2}$, 6, $6\frac{1}{2}$ and 8 years were still free after 2, $3\frac{1}{2}$, 4, 5, 5, 6 and 8 years and others have been described in which the period of freedom from attacks, and from treatment of course, had already extended over from one to three years. One of my own patients, who had frequent

attacks for two years, has been free for eighteen months, another who had attacks—every few minutes as the mother said—for six months has been well for two years and five months, another in whom they persisted for three years has had none for one year and four months, and I have others under observation in whom the period of freedom is still a short one or in whom the attacks still occur. To my own cases I attach very little importance. They interest me greatly, but I cannot expect you to think much of them for three reasons: (1) The period of observation is short; (2) the period of freedom from attacks is short, and (3) the patients are still young. But these objections will not hold against the cases I have culled from the literature, for in these the period of observation has extended to as many as sixteen years, the period of freedom from attacks has been as long as ten years, and the patients have reached such ages, taking a number of cases at random, as 14, 16, 18, 18, 20 and 22 years. These patients were perfectly well, mentally and physically, at the time the final observations were made and although the attacks had occurred daily for many years none of them ever had a single major attack. Do you think that these patients were suffering from epilepsy? This depends, you may say, on what I mean by epilepsy, so perhaps I should ask, were these patients suffering from the epilepsy of the text-books? Surely not. In epilepsy, according to Kraepelin, spontaneous recovery does not occur in more than 2 or 3 per cent. of cases. Gowers, writing on the prognosis with regard to a spontaneous termination, says that this is an event too rare to be reasonably anticipated in any given case, and our President (Dr. Collier) has expressed a similar opinion. In these cases, and in all the others that comply with the strict requirements that I shall mention later, spontaneous termination is a rule to which I have been unable to find a single exception. In epilepsy the prognosis is bad in cases with frequent minor seizures and in cases that do not respond favourably to appropriate remedies. In these cases the attacks are very frequent and the ordinary remedies have no appreciable effect upon them. In epilepsy slight minor attacks rarely attain a maximum frequency at the onset, or recur alone with absolutely uniform severity for more than a short time; almost from the beginning succeeding attacks vary in severity and the first major attack is rarely delayed for more than two or three years. In these cases there is no gradual onset, no climax; the very acme of the disease is attained at once and its most characteristic feature is the monotony and the uniform severity of minor attacks that may recur daily for many years without the occurrence of a single convulsive seizure. In the one disease, mental deterioration and psychical defects are characteristic, almost constant —some would say inevitable—sequels, that appear soonest and most often in its victims who have frequent abortive seizures: the other, in spite of its long duration and the frequency of the attacks, does not impede mental development nor give rise to psychical defects. These—I need not mention others—are differences that cannot well be ignored.

You may not agree with Friedmann that the cases that he and others have described are examples of a disease that has nothing whatever to with epilepsy, but I think you will agree that they deserve a distinctive name. Quoting Sir Clifford Allbutt:

"The right question is this: have I noted in a moving equilibrium, say in Man, that a certain series of changes, static and dynamic, has occurred more than once? If so, was the recurrence still fortuitous, or was the series a case of an orderly recurrent mode which hitherto had escaped attention? If so, the recurrence will be observed again and again by myself or by others in approximate uniformity. The cases of the

newly observed series will vary, some indeed so far as to trespass upon other nosological series already recognized, and no strict demarcation can be drawn around them; yet there may, nevertheless, be difference enough and constancy enough to make it worth our while, for the convenience of observation and thought, to erect the new series into a category of its own, and to stick a label on it?"[1]

I think these cases are worthy of a label, and as the choice seems to lie between non-epileptic absences, short narcoleptic attacks, aggregated minor attacks, and other names equally awkward, I propose that the name pyknolepsy, suggested by Schröder, be adopted by English observers. It is concise, it is non-committal; it refers to the main feature of the disease—the heaped-up, closely packed, aggregated attacks—and it allows us to coin a handy adjective, "pyknoleptic," by analogy with epileptic. Whether you regard pyknolepsy as a symptom, a disease, a morbid entity or what not, is after all merely a matter of taste. Following my guide in these matters again, "by disease I mean a mental concept of a series of symptoms recurring with fair uniformity," and for me pyknolepsy is a disease.

Having thus burned my boats, I propose to discuss the disease in slightly greater detail, because this bears directly on the questions to which you will certainly demand an answer, namely, "Is the prognosis good in *all* the cases that conform to my description, and is it possible to detect the cases with a good prognosis at or soon after the onset of the attacks?" I have arrived at my answers after a careful comparison of the features of the cases that recovered with those in which somewhat similar frequent minor attacks occurred at the onset of ordinary epilepsy.

(1) *The Age of the Patient at the Time of Onset.*—According to Friedmann, the onset in favourable cases occurs between the ages of four and ten years. Subsequent observations, in particular those of Stier, have shown that it is always between the fourth and twelfth years, whereas it is often above or below these ages in cases that resemble pyknolepsy for a time and yet prove to be epileptic. I do not doubt that favourable cases with an earlier or later onset will be encountered in the future, but for the present we must be bound by these limits.

(2) *The Duration of the Disease.*—This varied in cured cases from seven weeks to nine years with an average of about three years. A long duration, therefore, is consistent with a good prognosis. On the other hand, minor attacks of sorts have been known to occur alone in true epilepsy for as long as seven or eight years, hence a long duration of attacks classifiable as minor, but differing in the direction of greater severity from the attacks of pyknolepsy, does not justify a good prognosis. No case has been recorded of frequent minor attacks such as I have described, beginning in childhood and persisting alone beyond puberty, in which epilepsy has ultimately supervened. Once puberty is safely passed the ultimate prognosis, so far as we know at present, is always good in spite of persisting attacks (Stier).

(3) *The Mode of Onset, Frequency, Course and Mode of Cessation of the Attacks.*—In the favourable cases the onset is almost always sudden—"explosive" as Friedmann said, the attacks occurring with maximum frequency from the first day onwards; but in rare instances the minor attacks of true epilepsy also begin in this way; so an explosive onset, though almost constant in pyknolepsy and highly suggestive of it, is not in itself decisive in the differential diagnosis from epilepsy. The frequency of the attacks varies

[1] "Notes on the Composition of Scientific Papers," 1905.

greatly in different cases, from six to a hundred in a day, but is often remarkably constant in each case. Friedmann attached particular importance to this steadiness of course, because the attacks in his cases were repeated daily at about the same rate; but it has become clear that a waxing and waning in the daily number occur in favourable cases, and that changes in frequency from time to time are consistent with a good prognosis. The termination of the disease is usually sudden, but a gradual diminution in the number of the attacks and temporary periods of freedom have preceded their final complete disappearance in several instances.

With regard to the separate attacks: their great distinguishing feature is their extraordinary monotony; in each patient all the attacks are almost exactly the same in their course and severity. In my opinion this is their most important single characteristic, and, by using it as a test as I read the descriptions of series of cases, I have been able in every instance to foretell the sequel and to detect the cases that resembled pyknolepsy in some respects for a time but proved eventually to be cases of epilepsy. The attack is always short, a second or two up to ten or twenty seconds, and very mild. The limbs relax, but the child rarely falls, objects are rarely dropped from the hands, the colour does not change appreciably, there is no mental alarm or physical distress. At most the eyeballs may roll upwards, the lids may flicker, the head may be turned to one side or the arms raised by a feeble tonic spasm, but chewing or swallowing movements or clonic spasms of any kind never occur. At the end of the attack the child behaves as if nothing had happened, and is never languid or sleepy or confused for more than a moment or two.

I have said that bromides and luminal have no appreciable effect on the attacks. This has proved so constant that a favourable prognosis cannot be entertained in a case that responds favourably to these remedies.

In favourable cases it is rare to obtain the family history of alcoholism, epilepsy or nervous disease that is so common in epilepsy, but the children themselves may have suffered from spasmophilic symptoms, or from so-called occasional convulsions, for example at the onset of an acute disease or during an attack of whooping-cough, and a history of these does not prejudice the prognosis.

These are the facts. Now my study of a large number of cases with frequent minor attacks has taught me *first* that *all* the cases with a good prognosis have certain features in common; and secondly—(this is equally important for my argument)—no case with an unfavourable outcome presented all these common features. It follows that if a given case presents these common features the prognosis is good, that is to say it is one of pyknolepsy, for this, by definition, is a form of epilepsy in which the prognosis *is* good. An enumeration of these features, then, should serve as a basis for a short definition of the disease, a definition which will embody a number of requirements that must be met before the diagnosis of pyknolepsy can be made with certainty. I define pyknolepsy as *a disease with an explosive onset, between the ages of four and twelve years, of short, very slight, monotonous minor epileptiform seizures of uniform severity, which persist for weeks, months or years, uninfluenced by anti-epileptic remedies, without impeding normal mental and physical development and ultimately cease spontaneously, never to recur.*

Clonic movements, however slight, obvious vasomotor disturbances, mental distress, palpitations, and lassitude or confusion after the attack, are equivocal symptoms strongly suggestive of epilepsy, and for the present they should be considered as foreign to the disease.

Now to answer my questions: First, is the prognosis good in all the cases that conform to my description? Yes, so far as I know at present. I have read of cases of epilepsy with an explosive onset, of cases in which frequent minor attacks persisted alone for many years, of cases of undoubted epilepsy of long duration without mental changes of any kind, of cases that did not respond to the usual remedies, of cases, in short, that resembled pyknolepsy in some respects; but I know of no case that satisfied all the requirements that I have just laid down and proved nevertheless to be one of epilepsy.

To the second question—Can these cases be detected at the onset? the answer must be, No. There is nothing in the attacks by themselves to distinguish them from epilepsy, and a certain period of observation is necessary before a final decision can be made. In practice it will often happen that the disease has been present for a year or more when the patient first comes under observation, and a careful consideration of the history will give decisive information.

There is another highly practical question that I have not yet considered. It is one that will confront you very often if you study your cases of minor epilepsy closely, as I have done during the last two years, with a view to detecting cases that belong to the category we are now considering. It is this: what is the prognosis in cases that deviate, in the direction of slightly greater severity, from the description that I have given? when, for example, the onset is not explosive or the child falls in some of the attacks, or allows objects to drop from the hands, or changes colour or wets himself sometimes, or is confused for a short time after some of the attacks. All I can say now is that occasional falls have occurred in favourable cases—perhaps the child was in an attitude that rendered a fall inevitable when the attack came on, that objects have dropped from the hands—this, too, is easy to understand, that involuntary micturition did occur in one or two favourable cases; but these symptoms, though undoubtedly consistent with a good prognosis, were more often the first warnings of oncoming epilepsy, and must be looked upon with great suspicion. Until we know much more about pyknolepsy, it is wiser, I think, to include under this name only the cases that conform strictly to the description I have given, otherwise all sorts of frequent minor attacks will be called pyknolepsy, and the great advantage that is gained by limiting this name to those in which the prognosis is certainly good will be lost.

On the nature of the disease I have little to say. Friedmann thinks that it is a form of narcolepsy, but it has very little resemblance to the original narcolepsy of Gélineau. Heilbronner discussed the problem as if the alternative was epilepsy or hysteria, but we know now, that various seizures occur in children that have nothing to do with either. In several cases the attacks began after an emotional disturbance, but in the great majority they began without apparent cause. There is nothing hysterical in the ordinary sense about these young people, and I know of no form of hysteria in which one manifestation alone is present throughout long years. Mann found increased excitability of the peripheral nerves to electrical stimulation in two cases, and concluded that the disease was connected in some way with spasmophilia, but this is certainly not true. Nor is there any reason to believe that the disease is confined to children who possess the so-called psychopathic, neuropathic or vasoneurotic constitutions. Epilepsy alone remains. I shall not repeat my reasons for separating these cases from epilepsy, but if you think of all the conditions that have been included under the heading epilepsy or epileptic equivalents, perhaps you will agree that there is something to be said for a move

in the opposite direction; and when you consider the effect upon the relatives when the diagnosis of epilepsy is made and the utter hopelessness of this dread disease when it comes on in childhood, perhaps you will agree that the cases I have described are worthy of a distinctive name.

May I remind you, in closing, of the long periods of freedom from attacks in some of these cases, up to eight, nine, and ten years, the long periods of close observation by competent observers, up to sixteen years, and the relatively advanced—shall I say safe?—ages that have been attained by some of these patients, up to eighteen, twenty and twenty-two years—patients who suffered for years from the attacks that I have described and are now perfectly well in every respect.

The question whether pyknolepsy is a disease *sui generis* does not concern me greatly. It has been answered in the affirmative abroad, almost without a dissentient voice. I leave it for discussion. For myself I shall be well satisfied if I have succeeded in making it appear probable to you that there does exist a form of epilepsy in children in which the prognosis is good.

Section of Neurology.[1]

President—Dr. JAMES S. COLLIER.

Case of ? Myopathy Confined to the Face.

By JAMES S. COLLIER, M.D. (President).

W. H., A HAMMERMAN, aged 36, has never in his memory been able to close his eyes or to use his lips. He was told by his sister, when a child, that at birth both sides of his jaw were broken and that his peculiar facial appearance was the result.

There is complete atrophic paralysis of all the muscles of the face, except for a few fibres of the retractors of the right angle of the mouth. The "myopathic" face is very typical. He speaks as a ventriloquist, but his enunciation is very fair except for the labials. The ocular, trigeminal, lingual muscles and the muscles of the neck, body and limbs seem perfect. He has always been able to work hard at a laborious occupation.

No change has taken place in the condition of his face in his memory. He has an old traumatic cataract with perforating wound of the cornea on the left side. There are no signs of myasthenia nor of myotonia.

Cases of persistent bilateral obstetrical facial palsy have been reported by Duchenne; also by Edgeworth (*British Medical Journal*, 1894, i, p. 11).

DISCUSSION.

Dr. KINNIER WILSON said it was quite possible the case in reality was one of congenital nuclear defect—the "infantile Kernschwund" of German writers. Much depended on the exact clinical history in these cases, but he was of the opinion that obstetrical injury to the jaw, as stated by the patient's sister, would not explain the absolute facial diplegia from which the patient suffered.

Dr. COLLIER (President) said that it certainly was not one of bilateral obstetrical facial paralysis. There were no signs whatever that the jaws had been injured at birth. He suggested that the story given by the patient's sister was one of the commonly occurring examples of a fable put forward to explain a little understood fact. He was obliged to Dr. Wilson for his suggestion, which he was willing to accept as the real explanation of this case, the more so since such movement as was present in the right side of the face was much more easily elicited on emotional movement than on volitional attempt.

[1] Clinical Meeting held at the Hospital for Epilepsy and Paralysis, Maida Vale, W., on January 10, 1924.

A Case of Myotonia Atrophica.

Shown by J. P. MARTIN, M.D. (for WILFRED HARRIS, M.D.).

E. C., UNMARRIED woman, aged 38.

Family history: Cataract in paternal grandmother; cataract in father and in three of his sisters. History suggestive of myotonia in one of the patient's brothers now dead.

History: About 1912 the patient's face became very thin and never filled out again. During the last eight years occasional spasm of the tongue during conversation and for about five years spasm of the hands after grasping anything tightly. About two years ago the lower limbs began to waste and they are still getting thinner and weaker. Six months ago she felt the right leg numb and later the numbness spread up to the middle of the leg.

Present state: Advanced wasting of the muscles of the face, neck, forearms, thighs and legs; all the musculature of the body is very small. Myotonia on voluntary movements in the tongue, masseters, muscles involved in the hand grasp and in some of the muscles of the lower limbs. Myotonic reaction to percussion in the muscles of the upper arms and shoulder girdle.

Eyes: A few dust-like opacities in both lenses. Fundi normal. Pupils react poorly to light.

Circulatory system: Heart sounds normal. Blood-pressure, systolic 98 mm. mercury; and diastolic 66 mm. mercury.

Genito-urinary system: Free creatine is present in the urine.

Menses irregular every third or fourth month; menstruation did not begin till she was 18 years of age.

Nervous system: Tendon-jerks all absent; superficial reflexes normal. No disorder of the cranial nerves. On admission to hospital there was a loss of appreciation of pain and touch stimuli over the right foot and lower part of the right leg but this was dispelled by Dr. Harris by the use of suggestion and faradism.

Case of Tumour of the Acoustic Nerve.

By C. WORSTER-DROUGHT, M.D.

F. G., FEMALE, aged 37.

History.—About April, 1921, the patient began to complain of deafness in the right ear and numbness on the right side of the face, the two symptoms appearing almost simultaneously. During the past two years, the facial numbness has increased only very slightly, but the deafness has become more pronounced. Other symptoms that have been noticed from time to time are as follows: (1) In addition to right facial numbness, occasional tingling sensations on the right cheek, margins of the lips, and on the right side of the tongue. (2) Occasional tinnitus—described as "buzzing in the head"—which tends to become more constant. (3) Occasional vertigo and, more recently, a tendency to stagger on turning quickly or upon rising from the lying-down position. (4) Transient twitching of the right side of the face. (5) Attacks of nausea and vomiting of short duration at long intervals, and, more recently, (6) occipital

headaches. The symptoms, however, do not prevent the patient from carrying on her work as a clerk nor from enjoying life even to the extent of dancing.

Physical Examination.—Pupils normal, reactions normal; fundi and fields of vision normal. Rhythmical nystagmus upwards and outwards on deviation of eyes to right (towards lesion); ocular muscles otherwise normal. Motor functions of fifth nerve not impaired; relative conjunctival anæsthesia; large area of anæsthesia and analgesia (cotton-wool and pin-prick) on the right side of face extending from the eye to just below the corner of the mouth, forwards to the margin of the nose and backwards to the anterior border of the masseter muscle. The area of loss of sensibility to pin-prick is larger than, and encloses that to, cotton-wool (root anæsthesia). Facial movements normal. Eighth nerve: Complete nerve deafness on right side, confirmed by noise-machine to left ear and other tests. Labyrinthine reactions: Total absence of vertigo, nystagmus and past-pointing to cold air test on right side, both for horizontal and vertical semicircular canals; normal labyrinthine reactions on the left side; the galvanic reaction is also absent on the right side. (The case was also examined by Sir James Dundas-Grant, who considered that there was a lesion of the trunk of the auditory nerve internal to the labyrinth.) Tongue: Movement normal; sensation and taste are diminished over the anterior two-thirds of the right side. Arm-jerks, abdominal and plantar reflexes, knee- and ankle-jerks are all normal. No incoördination or tremor of hands; no dysdiadokokinesia. When placed in the Romberg position, there is a tendency to fall towards the right side; the same tendency is seen when the patient turns suddenly or rises from the lying-down position. Other systems are normal. The antra of Highmore and accessory nasal sinuses show no abnormality. Wassermann reaction in the blood serum is negative. Radiographic examination reveals no abnormality in the basal regions of the skull.

DISCUSSION.

Dr. WORSTER-DROUGHT said that the symptoms and signs in this case indicated a relatively early tumour of the acoustic nerve. An interesting feature was the simultaneous development of symptoms pointing to the involvement of the eighth and fifth nerves. The only indication of seventh nerve affection was the occasional twitching, due to irritation of the nerve. As with most cases tinnitus was late in appearance and was not constant. Apart from the nystagmus the only sign of cerebellar involvement was the tendency to lose the balance and fall to the right.

Dr. COLLIER (President), in reply to Dr. Worster-Drought's question as to treatment, said that he did not think it advisable to attempt removal of the tumour as long as this patient was able to enjoy life fully and was suffering so little inconvenience from its presence.

Case of Friedreich's Ataxia with Double Optic Atrophy.

By C. WORSTER-DROUGHT, M.D.

G. Y., AGED 23, complains of general nervousness, inability to keep his limbs still, and defective vision.

History.—The above symptoms have been present ever since the patient can remember, but appear recently to have become worse. Birth was accom-

panied by difficult labour, his mother being in labour for five days; no instruments were used. He did not walk until the age of one year and ten months, and at four years had a severe fall down a flight of stairs, sustaining concussion; following this he did not walk for nine months. His mother thinks that his defective vision dated from this time; she has noticed nothing else beyond the fact that he is nervous and "fidgetty."

Family History.—His father (killed in the Great War) suffered at very long intervals from "fits," the description of which suggests epilepsy. The patient has one sister, aged 21, who is quite healthy. His first cousin (paternal side) aged 20, is in a mental institution and is believed to be hydrocephalic. Otherwise there is no family history of nervous disorder.

Physical Examination.—Pupils: equal, central, regular and react normally. Fundi: double primary optic atrophy. Continuous nystagmus. Facial expression somewhat "passive." Other cranial nerves normal. Arm-jerks brisk and equal. Abdominal and epigastric reflexes brisk and equal. Knee-jerks present and equal; ankle-jerks absent. Plantar reflexes: Left flexor; right indefinite, mainly extensor. Atrophy of all intrinsic muscles of both feet with pes cavus. Sensation (cutaneous and deep) normal. Co-ordination: Arms (finger-nose test, &c.) normal. No intention tremor. Some difficulty in performing rapidly alternating movements of the arms and hands (dysdiadokokinesia). He tends to walk on rather a wide base, with slight ataxia, and exhibits a certain degree of Rombergism. Continual "restless" movements of head and limbs. Wassermann reaction negative in blood-serum. Cerebro-spinal fluid: 5 lymphocytes per c.mm., globulin and Wassermann reactions negative.

DISCUSSION.

Dr. WORSTER-DROUGHT remarked that the combination of signs in this case pointed to a diagnosis of Friedreich's ataxia, while the choreic restlessness described by some writers was a pronounced feature. He had brought this case before the Section on account of the extreme rarity of optic atrophy in this disease. Most authorities stated that it did not occur. Cases however had been recorded by Ormerod, Taylor, Cohn and others. All these cases had been quite typical in all other respects.

Dr. COLLIER (President) was of the opinion that this case was not one of Friedreich's disease, but that it belonged to the class of complicated peroneal atrophy. He said he had observed a family in which many members suffered with peroneal atrophy, complicated with optic atrophy and other signs unusual in that disease. The condition of the feet in this case was not one of pes cavus. They were simply drop feet from muscular wasting. He had shown many years ago that there was no atrophy of the intrinsic muscles of the feet in the pes cavus of Friedreich's disease. In Dr. Worster-Drought's case there was extreme atrophy of the muscles below the middle of the leg.

Symptoms following ? Encephalitis Lethargica.

By H. CAMPBELL THOMSON, M.D.

A. S., WOMAN, aged 40. Was ill in May, 1923, for about five weeks, but remembers nothing of this period. On getting up her vision was so defective that she knocked against things. Moving about, and any change of light, made her dazed and confused. She has had attacks of asthma for several years.

Her memory is very defective; she displays little interest in her surroundings, and is slow in responding to questions and instructions. The visual fields are much contracted, but she gets about without difficulty. The pupils are dilated and very sluggish in reaction to light and on convergence. The fundi are normal. Conjugate movement of the eyes is defective. The upper limbs show some intention tremor, slight ataxia, and weakness. The lower limbs are tremulous on voluntary movement; the tendon reflexes are all brisk; there is no abnormality in sensation, gait and superficial reflexes. The asthma and bronchitis have much abated while she has been in hospital, but the general condition has not changed.

Case of Amyotrophic Lateral Sclerosis.

By WILFRED HARRIS, M.D.

Mrs. K., AGED 66. Two years ago had influenza and double pneumonia, and has never been well since. Has noticed gradually increasing weakness of her hands and all over, with excessive perspiration. No pain or numbness.

On examination she shows bilateral tendency to foot-drop and wrist-drop, suggestive of a polyneuritis. She cannot walk on her heels, but can do so on her toes. She cannot extend the hands and fingers properly, extension on the ulnar side being weaker. On clenching the fists there is noticeable marked paresis of the radial extensors of the wrist, while the ulnar extensors are normal, causing adduction of the wrists to the ulnar side, more especially of the right wrist. No weakness nor atrophy of intrinsic muscles of the hands, or of the arms or shoulders. Flexion of the fingers and wrists is normal. In the lower extremities there is no weakness of the thigh muscles, but there is weakness of the peronei, especially of the right leg, and marked weakness of the tibialis anticus on the right side, less on the left. All the paralysed muscles show corresponding diminution to electrical test, both to faradism and to galvanism, K.C.C. > A.C.C. Knee-jerks present; no ankle clonus. Bilateral extensor plantars. No sphincter weakness. No cranial nerve weakness.

Hyperidrosis in Syringomyelia.

By WILFRED HARRIS, M.D.

J. K., AGED 47. Thirty years ago he fractured the inner condyle of the right humerus. Fifteen years ago he began to have pains like pins and needles in the right hand and arm. For a year or more he has noticed profuse sweating about the head and neck, principally on the right side, front and back of head, and on the left face; the colder the weather the more he sweats, and if he eats anything acid or pungent, or cheese, he says the right side of his face breaks out into a "fountain" of sweat. Constipation especially noticeable the last three years.

On testing him with a small meal of Gorgonzola cheese, I found the excessive sweating was not noticeable until ten to fifteen minutes afterwards. There is marked atrophy of the right hypothenar muscles and of the interossei, none

of the thenar. There is analgesia of the right ulnar nerve area, and considerable hypertrophy of the right ulnar nerve just above the elbow for about 1½ in. There are areas of analgesia and thermanæsthesia on the right second, third and fourth cervical areas, and on the chest and abdomen, especially on the left side down to the groin. Slight kypho-scoliosis. A year ago he says he burnt himself on the right palm unwittingly.

The knee-jerks are normal, and there are bilateral extensor plantars, with bilateral pes cavus. Cranial nerves normal.

DISCUSSION.

Dr. KINNIER WILSON said that this case strongly suggested the diagnosis of chronic hypertrophic neuritis of Déjerine and Sottas. He said that it should be borne in mind that one at least of the original cases described by these observers was diagnosed as syringomyelia. In favour of the diagnosis of hypertrophic neuritis, he thought, were the apparent hypertrophy of the ulnar nerves on both sides, and the bilateral pes cavus. A similar condition had been noticed in other recorded instances of the affection, and was not characteristic of syringomyelia.

Dr. WILFRED HARRIS (in reply) said that the hypertrophic neuritis of the right ulnar nerve was in his opinion due to the old fracture at the right elbow, the wasting of the muscles supplied by the ulnar nerve resulting from the chronic neuritis. The unconscious burn on the hand, the numerous areas of analgesia and thermanæsthesia and the paradoxical sweating were all points suggestive of a diagnosis of syringomyelia rather than of chronic hypertrophic neuritis.

Case of Poliomyelitis.

By WILFRED HARRIS, M.D.

J. W., AGED 13, and his elder brother were both taken acutely ill two months ago at Brighton with headache, delirium and pains in back and legs. The elder brother rapidly became paralysed all over, first in the legs and then the arms, and died in three days. Lumbar puncture was negative, and no autopsy was made. In the younger boy lumbar puncture is said to have yielded meningococci, the diagnosis of cerebro-spinal meningitis having been made in both cases. On recovery from the acute illness the patient was found to have paralysis in both arms, the left being the worse, with some weakness in the right leg.

He has complete paralysis and wasting of both deltoids, and of the left biceps, supinator longus and radial extensors of the wrist. The right shoulder joint is partially fixed by adhesions. There is no anæsthesia. The left foot is considerably smaller than the right, spastic in appearance with an extensor plantar reflex. The left hand is also slightly smaller than the right, this being presumably due to an infantile hemiplegia, possibly a birth palsy, as the mother knows of no acute illness in childhood which could have affected him thus; but his birth was difficult and prolonged, and she first noticed him to limp slightly at the age of three.

Case of an Adult Cretin.

By A. FEILING, M.D.

PATIENT, a female, F. H., aged 29. Height 50½ in. Weight 5 st. 11¼ lb. Nothing known of the father. Mother is alive and well. Two brothers alive and well. No other member of the family affected in the same way.

The patient was born in London and, except for a few years' residence in Warwickshire, has always lived here. She states that she has always been very small. Went to school and reached Standard VII.

Her appearance is that of a cretin. The mental state, however, is quite normal.

Case of Total Unilateral Ophthalmoplegia with Foot-Drop.

By A. FEILING, M.D.

PATIENT, a male, aged 58, began to suffer six months ago from headaches; three months ago he felt ill and tired and about two months ago had an attack of violent vomiting. After this it was noted that the left eyelid drooped. Examination at this time showed a complete left ophthalmoplegia with some degree of proptosis.

Admitted to hospital on November 2, 1923. A total left ophthalmoplegia affecting both internal and external eye muscles. No other abnormal signs found. Wassermann reaction in the blood and cerebro-spinal fluid negative; no abnormalities in the cerebro-spinal fluid. Examination of the throat and nose quite negative; X-ray examination of the skull negative.

Whilst in hospital he gradually developed weakness of the power of dorsi-flexion of the right foot. This gradually progressed to a complete foot-drop. There is wasting of the anterior tibial and peroneal muscles; no loss of sensation has been found.

At no time has any other abnormal sign been found.

Case of ? Amyotonia Congenita.

Shown by A. FEILING, M.D. (for R. C. ELMSLIE, M.S.).

FEMALE child, aged 4 years. No history of any similar disease in the family. Mother states that child was quite normal till the age of 15 months when she began to be weak. Weakness has become progressively worse. At one time the child could crawl and stand but she has never walked.

Previous History.—Chicken-pox and whooping-cough in infancy. At age of 15 months fell on her chin and was insensible for two hours.

She is now quite unable to stand or crawl or to raise herself up from the recumbent position. When placed in a sitting posture can remain there. Mental state satisfactory. Pupils, cranial nerves and upper extremities

normal. Flaccid weakness of all muscles of trunk, abdomen and lower extremities. Well marked hypotonia. Loss of tendon reflexes and of the superficial reflexes in the lower limbs.

DISCUSSION.

Dr. FEILING said that he showed this case chiefly from the point of view of diagnosis. Was it one of amyotonia congenita or one of the progressive spinal paralysis of Werdnig and Hoffmann? Personally he confessed to a great difficulty in making the differential diagnosis.

Dr. COLLIER (President) said that the case was in every way a typical one of the amyotonia congenita of Oppenheim, and differed entirely from what he recognized as the Werdnig-Hoffmann disease. He had seen several examples of the latter disease, and of these two had been examined pathologically and had proved to be cases of progressive degeneration of spinal neurones. He thought that Knud Krabbes' review had confused the issue in that he had included cases of Oppenheim's disease in his selected list of cases of Werdnig-Hoffmann disease, notably some of the cases recorded by Dr. Wilson and himself.

Cases of Progressive Lenticular Degeneration and Hepatic Cirrhosis.

By STANLEY BARNES, M.D.(Birmingham).

THESE three children are the offspring of parents both of whom appear to be healthy. There is no history of nervous disease or of cirrhosis of the liver in the family of either parent. The family (of whom three members are shown) consists of eight children, whose ages range from 16 years to 1 year old. All the children have been examined; two only (Cases I and II) show signs of nervous disease, whilst in four of them there is more or less indication of liver disease. The three eldest children show neither nervous nor hepatic disease.

Case I.—Phœbe B., aged 11. In February, 1920, she had a febrile illness with jaundice. There was no œdema or ascites. Since then she has had occasional attacks of vomiting, occasionally hæmatemesis.

In February, 1922, her tonsils and adenoids were removed as she had an offensive discharge from the nose. The attacks of vomiting continued, accompanied by feverishness; she was hysterical on occasions and would cry out for no reason.

The stiffness of the limbs came on during 1922, and with this there was increasing dysarthria and excessive salivation with constant dribbling.

She was admitted to the General Hospital, Birmingham, July 20, 1923. She was an emotional child of moderate intelligence. The mouth was kept half open and the face had a very vacant expression. The smile was stiff and slow.

Throughout, the muscles appeared to be unusually rigid, particularly those of the right side of the body. On passive movement there was always more resistance than the normal, and this was more marked in the arms than the legs. Any attempt at gripping with the right hand, after producing a powerful action, was of such a tonic type that relaxation was only possible after a few seconds of effort. Both arms, but particularly the right, were extended in decerebrate position: the legs, and particularly the right, tended

to adopt the equino-varus position. Although no tremors were present, all action of face, arm and leg was accompanied by athetoid movement. Speech was slow, slurred and monotonous, and has more recently become almost unintelligible; and there have been and are constant difficulty with mastication and persistent drooling.

At various times the liver has been felt to be hard and is palpable at a distance of about 1 in. below the costal margin. Sometimes the liver has been felt to be considerably larger, and at others smaller; but in all cases the consistence has suggested cirrhosis; the spleen has usually been palpable. A lævulose test for liver function showed deficiency; the Wassermann reaction was negative.

Case II.—Frederick B., aged 10.

In December, 1919, he had frequent attacks of vomiting with hæmatemesis on one occasion. On December 25, 1919, it was noticed he was jaundiced and a week or so later he developed ascites and œdema of the legs. He was treated at the Children's Hospital for cirrhosis of the liver, and the abdomen was tapped and four pints of fluid removed. The liver at this time was reported enlarged and nodular, the lower border being 1 in. below the costal margin. The spleen was not palpable. After leaving the Children's Hospital he had whooping cough and nervous debility. He became stupid, his memory bad, and had no inclination to work or play. There were occasional attacks of drowsiness.

In February, 1922, he had a febrile illness and following this the stiffness of the limbs was noticed, with dysarthria and constant dribbling. He suffered with diarrhœa and attacks of vomiting. He was admitted to the General Hospital in November, 1922. At this time there was rigidity of the right arm and both legs. On gripping with the right hand the grip could not be loosened without the assistance of the left hand. On standing, the right leg was held slightly adducted, the knee extended and the foot internally rotated. The right arm was held in the decerebrate position. Athetoid movements of the right arm were present. The expression was fatuous and vacant, he was highly emotional and the speech was slow, drawling, monotonous and blurred. There was constant dribbling of excessive saliva.

Examination of the nervous system showed no evidence of pyramidal disease and no ocular damage. A lævulose test indicated liver deficiency. The Wassermann test was negative.

He was again admitted to hospital in March, 1923, when further degeneration had occurred. The expression was one of complete vacuity, the mouth being held nearly half open, with constant dribbling. The speech had become almost incoherent and the lesion was now bilateral. There were athetoid movements of both arms. There was marked rigidity of the arms, but the right arm was less rigid than previously. There were no tremors. The legs showed an increased amount of rigidity, the right foot being held in a position of equino-varus. The liver was not palpable below the costal margin at this period.

Case III.—Elsie B., aged 8.

This child was examined in July, 1923, when it was found that the liver border was hard and palpable, $1\frac{1}{2}$ in. below the costal margin. There were no symptoms at that time, but in August she began to have attacks of vomiting, which recurred every four weeks. The vomit was intensely yellow, sour and acid.

On December 24, 1923, she complained of abdominal pain and shivering,

and on December 25 it was noticed her eyes were jaundiced. The abdominal pain continued, there was vomiting, and the jaundice became more marked. On December 26 it was noticed that the abdomen was swollen, and she complained that her boots were too tight. She also complained of pains in the right side of the neck and buzzing in the ears. There had been several attacks of vomiting, and on Saturday, December 29, the mother said she noticed an inflammation around the umbilicus.

She was admitted to hospital on January 2, 1924: at that time she was slightly jaundiced. She seems an intelligent child, and shows no rigidity of face or limbs. On examination of the abdomen there is seen to be a marked ascites. The lower border of the spleen can be felt 1½ in. below the costal margin. The liver has an upper border found by percussion at the seventh rib, and the lower border is hard and extends 1 in. below the costal margin. The urine contains a cloud of albumin and bile pigment.

The nervous system shows nothing abnormal.

Dr. KINNIER WILSON said the Section was very greatly indebted to Dr. Stanley Barnes for the trouble he had taken in bringing these extremely interesting cases to London. He agreed that the clinical picture was quite consonant with that of progressive lenticular degeneration. He pointed out several features of clinical interest in connexion with the cases, and emphasized the great importance of Case III. The facts in connexion with this latter case could clearly be taken to indicate that the liver affection preceded the lenticular degeneration. Its ætiological value, therefore, was very considerable; in fact, it might almost be regarded as the "link" that had hitherto been "missing."

He expressed the hope that a full report of the cases would be published in due course.

Case of Muscular Atrophy, resembling the Peroneal Type of Charcot, Marie and Tooth, in a Woman, commencing at the age of 46.

By GEORGE RIDDOCH, M.D.

ALICE M., aged 57, married. Eleven years ago she first noticed that her hands trembled when she used them. Three years ago her legs became weak, and she dragged her feet in walking. About the same time articulation began to be tremulous. From the beginning she has had no pain apart from occasional headaches during the past four or five months. Her disability has been gradually and steadily progressive.

At the age of 20 she had scarlet fever, but otherwise her health has been excellent before the onset of her present trouble. Her consumption of alcohol is moderate.

She married at 28, and has two daughters, aged 30 and 28 respectively, who have always been healthy. There were no other pregnancies. Her husband died eleven years ago of cirrhosis of the liver.

Her father, who is 82, became paralysed two or three years ago. Her mother died at the age of 39 of heart disease. Her only brother and sister are alive and healthy.

She is a well-nourished woman of fair intelligence, but poor education. Speech is normal, but articulation is tremulous. Hearing and vision good, discs and fundi clear.

Pupils and cranial nerves are normal, except that when she is speaking the lower part of her face is sometimes tremulous. Voluntary power in her upper limbs at the shoulders and elbows is excellent. There is weakness and wasting, more on the left than on the right side, of the muscles of her forearms and hands, especially of the flexors of her wrists and fingers, and the thenar and hypothenar muscles. In her lower limbs weakness and wasting are confined to the muscles below her knees, and the dorsiflexors of the feet and toes and the peronei are specially affected, being completely paralysed on the left side. There is no muscular fibrillation. Supinator, knee and ankle-jerks are absent on both sides. Abdominal reflexes are present and equal on the two sides and the plantar responses are flexor.

Sensibility to light touch is impaired in the peripheral parts of both lower limbs, left more than right, and that to pin-prick is diminished in her left foot only. Postural sensibility is defective in the fingers of both hands and, to a greater extent, in the toes, especially of her left foot. Recognition of the vibrations of a tuning-fork (C = 128) on her hands and lower limbs below the knees is impaired. Deep pressure pain is abolished in her calves.

There has been no sphincter trouble.

Both feet are a little swollen on their dorsal aspects. The Wassermann reaction is negative in her blood and spinal fluid, the latter showing no increase in protein or cells, and giving a negative colloidal gold test.

Case of Juvenile Tabes with Nystagmus, Dysarthria and Head-rolling.

By GEORGE RIDDOCH, M.D.

DORIS F., aged 20, unmarried. Five years ago she began to walk unsteadily, especially in the dark, and her left shoulder drooped. Her ataxia got steadily worse, and a year later she developed dysarthria and an involuntary lateral tremor of her head. She also began to drop things out of her hands if she failed to "keep an eye" on them. Recently she has tended to gasp for breath on slight exertion.

At the age of 9 years she had an illness that was diagnosed as "brain-fever," in which she was delirious.

Her father, mother, two brothers and one sister are alive and well. One brother died of peritonitis when a year old.

She is a bright, intelligent girl. Speech is normal, but there is definite dysarthria. Hearing and vision are good. Discs and fundi clear. Pupils moderately dilated; right slightly larger than left; their outlines are a little irregular; the reaction to light is sluggish in the right and absent in the left; the reaction on convergence is good. Ocular movements are full; there is no diplopia, strabismus or ptosis, but nystagmus is evoked on lateral fixation. Sensibility to pin-prick is slightly diminished on the left side of her face. The other cranial nerves are normal.

There is no weakness, wasting or dystonia of her upper and lower limbs. With her eyes shut her upper limbs, especially the left, are slightly ataxic. She has an unsteady gait and sways on attempting to stand with her feet together.

Supinator, knee and ankle-jerks are absent; abdominal reflexes are present and equal on the two sides and her plantar responses are flexor.

Sensibility to light touch and pin-prick is unimpaired in her limbs and trunk, and she has no pain. Postural sensibility is defective in the fingers of her left hand and the toes of her right foot. There is diminished appreciation of the vibrations of a tuning-fork (C = 128) in the fingers of her left hand and in her left foot. Bladder control is perfect. Her fingers are blue but to her they do not feel cold. Her heart, lungs and abdomen show no abnormality. The Wassermann reaction is negative in her blood and positive in her spinal fluid. The spinal fluid is normal in regard to albumin content, cells and colloidal gold test.

Dr. KINNIER WILSON said that this patient had been under his care at the National Hospital a year or two ago, and that the diagnosis then made was Friedreich's disease. Routine tests had been employed in the blood and spinal fluid, and they were completely negative. In his opinion there was no symptom that the patient showed which was incompatible with the diagnosis of Friedreich's disease. He felt very doubtful as to the likelihood of a Wassermann test being positive in the spinal fluid when the albumin content, cell-finding, and colloidal gold reaction were all negative.

Section of Neurology.

President—Dr. JAMES S. COLLIER.

Spontaneous Sub-arachnoid Hæmorrhage.

By C. P. SYMONDS, M.D.

THE title of my paper calls in the first instance for a word of explanation. From a study of the literature it seems that the term "spontaneous" in this connexion has acquired for some persons a mystical significance, and is conferred by them upon those cases of sub-arachnoid hæmorrhage for which no apparent cause is to be found either at the bedside or after death. *Omne ignotum pro magnifico!* Let us admit the existence of many cases, the origin of which remains obscure, and suffer the impatience of our present ignorance without seeking refuge behind a formula.

The term "spontaneous," as I interpret it here, covers all cases of sub-arachnoid hæmorrhage of origin other than traumatic. The word is used improperly, but I must refer you as precedent for the distinction to one of the earliest papers upon the subject published by Sir Samuel Wilks in 1859 and entitled "Sanguineous Meningeal Effusion (Apoplexy): Spontaneous and from Injury."[1]

Blood appearing in the sub-arachnoid space may obtain entrance to it by various channels. It may be derived from the rupture of vessels lying within the sub-arachnoid space itself. It may find its way into the sub-arachnoid space from hæmorrhage into the subdural cavity (as commonly occurs in traumatic lesions of the veins passing from the cerebral cortex into the great sinuses).

Or again, blood derived from the rupture of a vessel within the nervous substance is not infrequently discharged into the sub-arachnoid space either by breaking through the pia mater if the hæmorrhage be superficial, or, more usually, since cerebral hæmorrhage is, as a rule, deeply situated, the effusion may burst into one of the ventricles and make its way into the general sub-arachnoid cavity by the channels which connect this with the ventricular system.

Of these three sources of sub-arachnoid hæmorrhage I propose only to discuss the first and third, since, so far as I am aware, subdural hæmorrhage does not occur except as the result of injury, and is therefore excluded from present consideration.

The clinical evidence of sub-arachnoid hæmorrhage may be considered under two heads, first that derived from the examination of the cerebro-spinal fluid, second that which may be obtained by observation of the history, symptoms and course of the illness.

The changes which occur in the cerebro-spinal fluid in this condition were fully described by Froin in his thesis of 1904, and there is little to add to his original observations.

Shortly after the onset of the hæmorrhage the fluid obtained by lumbar puncture appears on withdrawal to be mixed with blood, and is usually under increased pressure.

[1] *Guy's Hosp. Rep.*, 1859, 3rd ser., v, p. 119.

If this fluid be collected in a series of three test tubes, the same degree of admixture is to be observed in each. The proportion of blood to cerebro-spinal fluid will vary on the one hand according to the severity of the leakage, and on the other hand according to the situation of the ruptured vessel. Blood extravasated into one of the basal cisterns will find its way more readily to the site of lumbar puncture than an effusion originating from the surface of one of the cerebral hemispheres, or from the wall of one of the lateral ventricles. Thus the colour of the fluid when first withdrawn may vary from an opalescent pink to that of pure blood—the highest count of red cells per c.mm. which I have found recorded being 3,380,000.

On being allowed to stand in the test-tube this sanguineous fluid does not form any coagulum. The red cells sink to the bottom of the tube, and the clear supernatant fluid appears coloured by the presence of altered blood pigments. This colour varies from rose-pink or brown to golden yellow. Subsequently it undergoes changes together with the other elements in the fluid, which I shall refer to later.

Thus the distinctive macroscopic features of the cerebro-spinal fluid in sub-arachnoid hæmorrhage are three:—

(1) An even admixture of blood which is the same in a series of specimens collected at the same puncture.

(2) Absence of coagulum.

(3) Pink, brown or yellow coloration of the clear supernatant fluid, which is apparent when the red cells have been allowed to sink to the bottom of the tube.

None of these three features is to be observed in cerebro-spinal fluid contaminated by the accidental injury of a vein during lumbar puncture. Here, on the other hand, when the fluid is obtained in a series of tubes, the first is seen to be more deeply stained than the last. On standing in the test-tube the fluid forms a coagulum, and the clear fluid in the upper portion of the tube remains colourless.

The explanation offered by Froin for these differences appears to be satisfactory. In sub-arachnoid hæmorrhage the effusion of blood forms a coagulum at and around the site of the leakage. The proportion of red cells entangled in this coagulum varies somewhat according to the severity of the leakage and its situation. If the latter be in one of the basal cisterns where the circulation of the cerebro-spinal fluid is free, large numbers of red cells are liberated, and rapidly become diffused throughout the sub-arachnoid space. If, at the other extreme, the site of the hæmorrhage be in the lateral ventricle, the path of outflow through the iter is relatively narrow and the great majority of red cells are entangled in the clot. Such as do find their way to the basal cisterns rapidly diffuse throughout the great sub-arachnoid lake. Hence two of the characteristic features of the fluid obtained at lumbar puncture—the even admixture of the red cells, and the absence of coagulum.

The coloration of the supernatant fluid appears to be due to pigments derived from the red cells in process of destruction. This process of hæmatolysis is in its most active phase a few days after the initial effusion, and it is at this time therefore that the colour change is most remarkable. The tint is variable. In the earliest stages there may be very little or no coloration. The rose-yellow or brownish tint appears to coincide with the height of hæmatolysis in a large effusion, and the spectroscope shows the oxyhæmoglobin or hæmoglobin absorption bands; this colour yields later to a golden yellow

which gives the chemical reactions proper to bile pigments. In the case of a small effusion the colour may never be darker than golden yellow.

The microscopic changes which are to be observed in the sanguineous fluid have been carefully studied by Widal, Froin and others. In the earliest stage the number of white cells in proportion to the red cells is that met with in the blood. Later there is an increase in the white cells, at first especially of polymorphs and large mononuclears, subsequently of lymphocytes; and a small increase of lymphocytes may still be observed when the red cells have disappeared from the fluid, and this is clear and colourless.

Whereas the most conclusive evidence of sub-arachnoid hæmorrhage is afforded by the state of the cerebro-spinal fluid, this does not as a rule aid us in distinguishing the *source* of the hæmorrhage—whether it is of primary meningeal origin, or comes from an intracerebral effusion which has broken into the subarachnoid space.

There are, however, other clinical features which, in all but immediately fatal cases, serve, as a rule, to distinguish these two groups, the meningeal and the cerebro-meningeal hæmorrhages.

A cerebral hæmorrhage of any size as a rule gives rise to focal symptoms, usually a hemiplegia which is persistent. If the effusion ruptures into the meninges the signs of meningeal irritation will ensue as an additional symptom. If on the other hand there is rupture into one of the ventricles, the sequelæ consist of profound coma, together with generalized muscular rigidity and hyperpyrexia.

In primary meningeal hæmorrhage, on the other hand, signs of local cerebral damage are generally absent, or if present are slight and transient owing to the readiness with which the effusion finds an outlet into the sub-arachnoid lake. The symptoms in individual cases vary according to the situation, the suddenness, and especially the magnitude of the extravasation.

(1) At one extreme are cases in which the onset is apoplectiform, and death ensues rapidly from cerebral compression. In these instances, though lumbar puncture reveals the existence of sub-arachnoid hæmorrhage, it is impossible to distinguish the meningeal from the cerebro-meningeal hæmorrhage.

(2) In a second group of cases the onset may be sudden, with initial loss of consciousness and a variable period of coma due to cerebral compression, which passes off as the effusion becomes spread out, and reveals the signs of meningeal irritation. In this group the absence of any persistent signs of local cerebral destruction is valuable evidence in favour of meningeal rather than cerebro-meningeal hæmorrhage.

The following case illustrative of this group is one which I saw at Lambeth Hospital through the kindness of Dr. Bailey.

A labourer, aged 47, having previously enjoyed good health, had a sudden seizure with loss of consciousness. He was admitted a few days later (the history is incomplete) with the tentative diagnosis of encephalitis lethargica. He was then noted to be "very drowsy, speaks incoherently, plantar reflexes flexor, knee-jerks present, pupils react normally."

On the day following admission Dr. Stebbing noted that he was very drowsy but would answer questions with his eyes half-closed. He found weakness of both sixth nerves, but confirmed the absence of any other physical signs.

Cerebro-spinal fluid obtained the same day was examined by Dr. Perdrau, who reported as follows:—

"Fluid clear and brownish, no clotting on standing. Total protein, 1 per cent., sugar less than normal. Chlorides, 0·68 per cent.; cells, 69 per cubic millimetre, and 15 red blood cells per cubic millimetre. Differential count of whites: 95 per cent.

lymphocytes; 5 per cent. polymorphs. No plasma cells. Spectroscopic examination showed two absorption bands of a hæmoglobin derivative."

When I saw him on the following day, I found that he was conscious but drowsy with sighing respiration. There were no signs of local destruction of the central nervous system. He showed, however, definite neck stiffness, and a positive Kernig's sign. Ophthalmoscopic examination revealed neuro-retinitis of the typical albuminuric appearance together with one very large hæmorrhage near the left optic disc, and a few small hæmorrhages in the right fundus. The systolic blood-pressure was 190, and the urine contained a trace of albumin. The Wassermann reaction in the blood was negative.

The patient made a gradual but uninterrupted recovery, and left the hospital after two months in apparently good health. His systolic blood-pressure at that time was 210.

On discharge he resumed work for a month, but was unable to keep it up owing to general debility. Nine months later he was readmitted, having been struck down with apoplexy in the street. He was found on admission to be comatose, with a right hemiplegia, and died within twenty-four hours.

The autopsy revealed a recent large hæmorrhage occupying the greater part of the left hemisphere. No signs of any previous cerebral hæmorrhage were discovered. The arteries at the base of the brain were not carefully examined. The kidneys showed the characteristic changes of chronic Bright's disease.

I conclude that in this case the original illness was due to rupture of some small vessel at the base of the brain with subsequent healing. I was unfortunately not able to be present at the autopsy, or I should have made careful search for it.

It is difficult at first thought to understand how such a ruptured vessel could heal, especially as in this case it had to withstand a blood-pressure of 200. That arterial leakage into the sub-arachnoid space may be a recoverable condition is clearly proved by the number of persons with intracranial aneurysm who have survived such accidents for months or even years.

I take it that there are two factors in sub-arachnoid hæmorrhage which tend to promote recovery. First, the pressure of the effusion is evenly distributed throughout the whole sub-arachnoid space instead of being confined to the cranial contents as in an intracranial hæmorrhage, and the risk of death from acute cerebral compression is therefore lessened; and in the second place the fine meshes of the sub-arachnoid cavity promote coagulation.

(3) In the third group of cases the leakage is so gradual or so slight as to produce little or no cerebral compression, and the signs of meningeal irritation are unmasked. In these cases, even if the onset be sudden, consciousness may not be lost.

A case which I have published in the *Guy's Hospital Reports* serves to illustrate this group:—

Colonel E., aged 70, a single, retired officer of the Indian Medical Service, had led an active, busy life up to the date of the illness to be described.

On July 29, 1922, he was in his usual health. On the following morning, however, he complained to his landlady, on being called, that he had had a very bad night, with pain in the back of the neck. An hour or two later he was discovered lying in the bath-room in a dazed condition with a small scalp wound in the right parietal region.

He was conscious when found, and said that the pain in the back of his head had suddenly become intense, "something seemed to snap there," and he fell.

He was able to walk downstairs with support, went to bed, and stayed there for the next few days, though he got up to shave and wash. During this time he had a very foul, coated tongue; he was constipated and "a little light-headed" at times. Besides the pain in the neck, he complained of considerable pain in his thighs, so that,

for example, when he began to walk and bent his knees he would stop suddenly with the severity of it.

His condition slowly improved until August 10, when he had bacon and eggs for breakfast. He had had his bowels open and his tongue was clean.

At 12.15 a.m., on the 11th, he again sent for a medical friend, complaining of intense pain in the back of the neck, which was arched backwards. Subsequently he became gradually unconscious.

When I first saw him the same afternoon he was in a state of coma with sterterous breathing. He lay with his head turned a little to the right. Both eyes were open, but with the left lid a little lower than the right. The pupils were small—the left slightly the larger—and did not react to light. There was some external strabismus of the left eye. Slight weakness of the left face. The pulse was 48, the systolic blood-pressure 150 mm. Hg. The peripheral arteries to the feel, and the retinal vessels to the view, appeared to be in good condition.

On examination, it was noted that there was a definite backward rigidity of the neck. Kernig's sign was also present. He grimaced as if with pain when an attempt was made to carry the passive movement past a certain point.

In addition to this there was some rigidity of the limbs. The upper limbs were flexed at the elbows and pronated, the lower limbs fully extended, feet arched and toes pointing downwards. There were occasional purposive movements of the right arm.

The comatose condition of the patient made sensory examination impossible. As to the reflexes, none of the tendon-jerks could be obtained; both the plantar reflexes were extensor. He had retention of urine, and catheterization was necessary.

On August 12, he rallied in the early morning, became conscious, asked for water, and expressed a wish to pass urine, which he was able to do at will. At 10.30 a.m. he reeognized his friends and talked with them. At 3 p.m. he suddenly became unconscious again and died six hours later.

The post-mortem performed the next day was limited to the brain. On removal of the skull and incision of the dura, the subdural space at the vertex seemed to contain free blood, but this was small in quantity and may have escaped from an accidental rent in the arachnoid. At the base there was no doubt that the blood had escaped into the subdural space, for it was thickly clotted in both temporal fossæ after the brain with the lepto-meninges had been removed.

The sub-arachnoid space over the greater part of the hemispheres contained no blood, but at the base this was abundant, obscuring all the vessels and nerves, covering the under surface of pons and medulla and extending in a thick layer down the spinal canal as far as one could see. When the cervical cord was cut through for removal of the brain, there was a considerable flow of blood from the spinal sub-arachnoid space.

After removal, a stream of water was directed upon the clot at the base, and it was gradually wiped and teased away. About the right side of the pons and medulla it was especially adherent, and at the angle of junction of pons and medulla on the right side, close to the origin of the sixth nerve, was a hard, oval clot, the size of an almond. This was dislodged with difficulty, as it appeared to extend into the substance of the brain stem.

On cutting the hard clot across, I observed a small cavity in its centre, and this I am inclined to think represented the cavity of a minute aneurysm.

A sagittal section of the pons and medulla revealed a clot the size of a cherry-stone to the right of the midline, which was directly continuous through the rent in the surface with the hard clot already described. The clot within the nervous substance was of a soft jelly-like consistency, and clearly of a date later than that of the external clot. It seemed, therefore, as if the hæmorrhage into the medulla had been from the surface inwards rather than in the opposite direction. Unfortunately the suspected aneurysm and adjacent parts, which were needed for further examination were, by an oversight, replaced within the cranium and lost.

The rest of the brain was of normal size and configuration, and on section showed no abnormality.

The cerebral arteries on the whole were in a very good condition, with the exception

of isolated plaques of atheroma in those of medium size, and extreme calcareous rigidity of the carotids.

In a second case, related in the same paper, the symptoms were very similar, but led to recovery, the evidence of the sub-arachnoid hæmorrhage being obtained from lumbar puncture.

This patient, a nursing sister, aged 52, was also able, after recovery, to give a vivid account of the onset of her illness.

Having previously been in her usual health, she suddenly felt vaguely ill while at lunch, and went to her room. "Then suddenly there was a whirling feeling at the base of my skull, and something seemed to snap."

She was found on the floor in a semi-conscious state and put to bed. Subsequently she complained of severe headache and pain in the back of the neck. When I saw her on the fifth day of her illness she presented considerable neck stiffness. The plantar responses were on the right doubtful, on the left occasionally extensor. Lumbar puncture revealed evenly blood-stained cerebro-spinal fluid. She made a gradual recovery.

The evidences of meningeal hæmorrhage, therefore, in addition to the state of the cerebro-spinal fluid, are the signs of acute meningeal irritation—headache, pain or stiffness in the neck and limbs, a positive Kernig's sign, and a dazed, irritable, mental condition. These signs of meningeal irritation may be preceded in severe cases by those of cerebral compression, or if the hæmorrhage be profuse, death may ensue from compression in the first stage.

There are two further clinical phenomena which may be observed in sub-arachnoid hæmorrhage and deserve separate consideration.

One is the occurrence of fever which is usually present in the first ten days, and is at its highest during the phase of most active hæmatolysis, with which it is doubtless associated.

The temperature chart shown is from a case which I shall refer to later, and appears, from a consideration of other cases reported, to be somewhat typical.

The association of pyrexia with signs of acute meningeal irritation has, it appears, frequently given rise to the suspicion of an infective meningitis, and this mistake in diagnosis has probably been committed more frequently than is discernible from the literature, the blood obtained at lumbar puncture being usually set down to an accidental cause. This hæmatolytic fever is analogous to that seen in other instances of bleeding into serous cavities with subsequent absorption.

The other phenomenon occasionally to be observed in meningeal hæmorrhage is the occurrence of subretinal hæmorrhages.

I have observed these in one case which I have reported, that of a woman aged 52, the subject of cerebral arteriosclerosis, who died from rupture of a small aneurysm of the right internal carotid artery within the cavernous sinus. In this case both discs were slightly œdematous, and in the left fundus were several large subretinal accumulations of blood, one of which partly obscured the edge of the optic disc. It was noted that these had not the ragged outline usually seen in large retinal hæmorrhages.

At post-mortem on the following day widespread sub-arachnoid hæmorrhage was discovered which extended into the sheaths of both optic nerves.

I have met with two similar observations, both in cases of ruptured aneurysm. The earliest is that recorded by Sir William Hale-White in 1895.[1]

His case was that of a labourer, aged 21, who had an aneurysm of the right internal carotid just before its termination, presumably of congenital origin, since there was no other evidence of disease.

After an initial seizure with brief recovery he developed the characteristic symptoms of progressive meningeal hæmorrhage.

[1] *Trans. Clin. Soc. Lond.*, 1895, xxviii, p. 7.

On examination of the fundi "there was no optic neuritis, nor atrophy, but on the outer side of the right optic disc was a large prominent, dark brick-red, sub-retinal swelling. This encroached a little on the disc and was four times the size of it. It was thought to be a subretinal hæmorrhage."

At the post-mortem "there was a large amount of clot on the under-surface of the brain, which extended to the fourth ventricle and was $\frac{1}{8}$ in. thick in the sub-arachnoid space all down the spine. The hæmorrhage had passed forwards in the sheaths of the optic nerves, which were much distended with blood-clot, and ultimately tore its way forwards under the retina."

The other instance is recorded by Doubler and Marlow. Here again the cause was an intracranial aneurysm of the internal carotid artery which had apparently ruptured.

The patient was a woman of 32, previously in good health, who died ten and a half hours after the onset of her illness. She was admitted in a state of semi-consciousness with neck stiffness and absent tendon-jerks. Lumbar puncture revealed a heavily blood-stained fluid.

"At the first ophthalmoscopic examination of the right eye, a large, red, apparently fresh hæmorrhage was seen located on the disc, partly obscuring its upper half. There were a few smaller, peripheral flame-shaped hæmorrhages not impinging upon the disc or any blood-vessel. . . . Both fundi were examined from time to time. The large hæmorrhage on the disc in the right eye grew until the disc was completely obscured. It partly filled the nasal side of the fundus, and appeared to be extending into the vitreous. At one examination it seemed as though the hæmorrhage could be seen to undergo enlargement."

At the autopsy both optic nerves and the posterior parts of the eyeballs were removed.

Cross section of the optic nerves showed the sub-arachnoid and subdural sheaths to be distended by blood which extended as far, anteriorly, as the lamina cribrosa. At no place did this hæmorrhage invade the nerve itself. The hæmorrhage upon the right papilla appeared to be a direct extension from the nerve sheath, through the lamina cribrosa.

Other observations of a similar nature have probably been made. I shall hope to hear of some this evening.

The fact that in the three cases which I have related the hæmorrhage originated in each instance from aneurysm of the internal carotid is probably not without significance. Both the size of the vessel and its situation would favour a pressure of fluid blood within the optic nerve sheath sufficient to cause extravasation into the sub-retinal spaces.

It would appear then that for the recognition of sub-arachnoid hæmorrhage at the bedside we have at our disposal reliable clinical data, and when to this is added an opportunity of examining the cerebro-spinal fluid, the true diagnosis may often be arrived at with reasonable certainty. When, however, we come to inquire more particularly into the exact sources and pathological causes of the hæmorrhage, we are confronted with greater difficulties.

It is natural first to suspect those causes which commonly lead to vascular degeneration or rupture, whether in the central nervous system or elsewhere, and of these the most important is arteriosclerosis. In this disease degeneration of the media, together with a rise of blood-pressure, may result either in direct rupture of the vessel, or in the formation of an aneurysm which in its turn commonly bursts. Examples of both these accidents I have already quoted. Actually, such cases must be of comparatively frequent occurrence, yet the literature of primary sub-arachnoid hæmorrhage is scanty, probably because most cases are put down as cerebral hæmorrhage without sufficient inquiry.

From papers directly dealing with sub-arachnoid hæmorrhage I have been able to collect descriptions of sixty-three cases in which adequate data are available, and to these I can add seven observations of my own. I have included only those in which lumbar puncture or post-mortem examination has afforded clear proof of the diagnosis, and I have been careful to exclude cases in which the meningeal hæmorrhage appeared to be secondary to an intracerebral effusion.

Of these seventy cases twenty-two are definitely recorded as having shown signs of generalized arteriosclerosis with or without chronic nephritis. Five of these recovered from their attack of sub-arachnoid hæmorrhage, seventeen died. Of these, in six a ruptured artery was found—five times at the base of the brain, once in the lumbar region. In three the source was a ruptured aneurysm, while in the remaining eight the source was not found. With regard to this latter point, it must be remembered that great care and patience are required to discover amidst widely-spread masses of clot a minute rent in the wall of an artery, for instance of the size of the anterior cerebral, and even a small aneurysm is easily missed.

Therefore, I think we may fairly assume that primary sub-arachnoid hæmorrhage occurring in an old person with diseased vessels is due to the same causes which commonly determine *intra*-cerebral hæmorrhage.

Amongst the remainder of my series of seventy cases of sub-arachnoid hæmorrhage there are twenty-one others in which the accident could be attributed to a reasonable cause or was associated with pathological changes elsewhere in the body. Thus, there is a case of hæmophilia, another of pernicious anæmia, another of rupture of a vessel in the wall of a cyst. There are also six cases in which the patient is either recorded to have had positive Wassermann reactions at the time, or was known to have suffered from syphilis a few years previously. Only one of these patients died, and in this instance no definite source for the hæmorrhage was found at autopsy. The remainder improved under anti-syphilitic treatment.

The pathological process at work in these cases is probably that which Turnbull has described as a somewhat uncommon form of syphilitic arteritis. As he has pointed out: "When the smaller muscular and elastic arteries are the seat of syphilitic inflammation, endarteritis is usually a marked feature, the lumen becoming almost obliterated." However "in some cases of intense inflammation with necrosis the adventitia and media become greatly weakened before the intima has greatly thickened." And it is in such cases that rupture is likely to occur.

Five instances of sub-arachnoid hæmorrhage occurred in patients addicted to chronic alcoholism, a condition in which vascular degeneration frequently occurs. In two of these cases there were associated hæmorrhages in other parts of the body.

In three cases the symptoms of meningeal irritation are said to have developed acutely in persons exposed to the rays of a tropical or sub-tropical sun. All three recovered. Sir William Willcox has published observations of extreme congestion of the brain and meninges in fatal cases of heat stroke seen in Mesopotamia during the war, and of other cases in which recovery was associated with the signs of cerebral lesions presumably due to hæmorrhage, but I am not aware of any recent instances in which a blood-stained cerebro-spinal fluid has been observed during life or sub-arachnoid hæmorrhage demonstrated at autopsy.

Finally, there is a group of five cases in which the hæmorrhage was proved post mortem to have originated from an aneurysm at the base of the brain in

the absence of any evidence of arterial degeneration or other disease. In each of these cases the patient was young. In two instances I have been unable to obtain access to the original descriptions. In the other three the ages were 36, 32 and 24.

The last of these I was able to bring to light quite recently. The case was that of a young man, aged 24, who came into Guy's Hospital last September, and died a few hours after admission. He was in a state of coma when first seen, and is stated to have had epileptiform convulsions while under observation. No previous history was available. Just before death he appears to have had a small amount of sugar in his urine. The provisional clinical diagnosis made was status epilepticus. At the post mortem there was abundance of blood in the sub-arachnoid spaces at the base of the brain with extension upwards into the Sylvian fissures and the neighbouring sulci. The rest of the bodily organs were those of a healthy young man. The diagnosis suggested was that in the course of an epileptic convulsion one of the cerebral veins had given way under the increased pressure.

Having obtained permission to examine the brain and with the possibility of an aneurysm before my eyes, I proceeded to dissect the main arteries out of the clot in which they were imbedded, working upwards from the circle of Willis, and discovered a small thin-walled aneurysm at the first point of bifurcation of the right anterior cerebral. The aneurysm lies actually in the apex of the Y formed by the two branches, which both appear to take origin from it. Its free wall, which is of the thickness of a sheet of notepaper, has ruptured.

Here, then, is a case in which the cause of a fatal meningeal hæmorrhage might have remained obscure had not the aneurysm been carefully looked for. The remarks of Sir William Gull, written in 1859, are so much to the point in this connexion that I may perhaps be forgiven for quoting them:—

"Aneurism of the cerebral vessels has been regarded as a disease of extreme rarity, and judging by the scanty records of it, we should conclude that the opinion was true. This apparent rarity, however, like all negative conclusions, is doubtful, and I think there is the more reason to suspect it as only apparent and due to careless inquiry, since the discovery of these cases has been much more frequent during the last ten years. There are several reasons why intracranial aneurism is likely to be overlooked. First of all, as here hinted at, it has not been looked for, and it is notorious that the eye can see only that it brings with it the aptitude to see. Again, when death occurs from rupture of the sac, recent coagula may so imbed and conceal it that unless strictly looked for it will not be found, for the sac is often small, and thin and transparent, except at the point of rupture. Further, also, when death has taken place from changes around the aneurism, as by pressure or softening, the sac itself may present such appearances that unless a minute dissection be made of it, its true nature may not be discovered. Whenever young persons die with symptoms of ingravescent apoplexy, and after death large effusion of blood is found, especially if the effusion be over the surface of the brain in the meshes of the pia mater, the presence of an aneurism is probable."[1]

In the case which I have just related the cause of the aneurysm was almost certainly that which has been alluded to by Turnbull as an "inherent weakness due to a congenital abnormality in the structure of the arteries at their points of junction."

Turnbull has recorded an instance of one of these aneurysms found at the junction of left anterior cerebral with anterior communicating, in a child aged 1 year and 7 months who died of broncho-pneumonia complicating gastro-enteritis.

The late Dr. Fearnsides, adding forty-four new cases to the literature of intracranial aneurysm in 1916,[2] found that of thirty-one cases in which non-inflammatory degeneration of the media was the cause, in fifteen (or 48·4 per cent.) no cardio-vascular hypertrophy was present.

[1] *Guy's Hosp. Rep.*, 3rd ser., 1859, v, p. 281.
[2] *Brain*, 1916, xxxix, pp. 284-296.

Whilst the literature of sub-arachnoid hæmorrhage is scanty that of intracranial aneurysm is relatively abundant. Beadles, in 1907, was able to collect 555 cases including 114 of his own, and many have since been published. The group of congenital origin has not been recognized until recent years although the fact was generally recognized that intracranial aneurysms might be found in young and healthy people. If Fearnsides' figures prove a reliable index then congenital aneurysms must be of not infrequent occurrence, for they were found fifteen times in a series of 5,432 consecutive examinations of the head—one in every 362 cases.

Moreover, in many cases of intracranial aneurysm there occur multiple leakages of blood due to partial rupture of the sac, before the final breach occurs. (This was so in 41·9 per cent. of Fearnsides' cases.) I suspect, therefore, that in many of the cases which have been recorded as obscure sub-arachnoid hæmorrhage the cause has been an unrevealed congenital aneurysm. When the patient has died this may well have escaped detection if the possibility of its existence was not entertained in the mind of the examiner.

In those cases, on the other hand, in which the patient has recovered from the first attack, he may appear to have regained his normal health for months or even years before further leakage takes place. In such cases, therefore, the possibility of a congenital aneurysm cannot be excluded without a history of prolonged subsequent observation, and in none of the published instances is this forthcoming.

I have published in the *Guy's Hospital Reports* as a probable case of congenital aneurysm the record of a man, now aged 20, who has suffered from three attacks of sub-arachnoid hæmorrhage.

He was first admitted to Guy's, at the age of 12, with the symptoms of acute meningitis. Lumbar puncture on the fifth day of his illness revealed a fluid stated to be normal save for a slight admixture of blood. He made a rapid and uneventful recovery. He was readmitted at the age of 17 for a similar illness. On this occasion the cerebro-spinal fluid was obtained on the sixth and ninth days of the illness, and showed the appearances characteristic of sub-arachnoid hæmorrhage. The third attack occurred in January, 1923, at the age of 19, when he first came under my observation. In addition to the signs of acute meningeal irritation, and the presence of much blood in the cerebro-spinal fluid, he showed on this occasion some swelling of the discs, a right third nerve palsy, and a left-sided hemiparesis. He made a rapid recovery, but still showed some left hemiparesis and complained of occasional right frontal headaches when last seen.

Actually, on looking through the seventy cases which I have collected under the heading of sub-arachnoid hæmorrhage there appear to be twenty-seven in which no satisfactory cause could be assigned for the accident. Of these seven died, and the autopsy notes record the existence of profuse sub-arachnoid hæmorrhage (as a rule basal) without the discovery of any ruptured vessel. The remaining twenty patients recovered and of these the after-histories are available only in three cases of my own.

It is noteworthy that the age incidence in the obscure group is relatively low compared with other groups. The youngest patient was 10, the oldest 56, with an average age of 28.

The distribution is shown more clearly in tabular form:—

Between	the	ages	of	10	to	20	...	...	6 cases
,,	,,	,,	,,	20	,,	30	...	...	11 ,,
,,	,,	,,	,,	30	,,	40	...	...	2 ,,
,,	,,	,,	,,	40	,,	50	...	...	2 ,,
,,	,,	,,	,,	50	,,	60	...	...	4 ,,

In Fearnsides' fifteen cases of aneurysm which appeared to belong to the congenital group the ages varied from 19 to 53 with an average age of 38.

These figures, as far as they go, may be taken as evidence in favour of the view which I have already put forward that in a number of the obscure cases the cause of sub-arachnoid hæmorrhage has been an unrecognized aneurysm of congenital origin.

Another theory has recently been put forward by Goldflam in explanation of certain of the obscure cases of sub-arachnoid hæmorrhage. He notes, as others have done, that many of the patients attacked are young persons in normal health at the time of onset. Taking this in conjunction with the negative post-mortem appearances in certain fatal cases, he suggests that the cause may be a functional disturbance of vasomotor control analogous to that which is said to occur in migraine, Raynaud's disease and erythromelalgia. His thesis appears to be that in the stage of active hyperæmia, which follows that of abnormal vaso-constriction, capillary oozing may take place upon the surface of the brain. In support of his theory he refers to thirteen cases of sub-arachnoid hæmorrhage personally observed, in no less than five of which the patients suffered from true migraine.

Unfortunately no detailed account of these cases is given, and this largely detracts from the value of the paper. Goldflam also appears to be unaware of the congenital group of aneurysms, for he rules out the possibility of aneurysm in ten of his cases on the ground that they were all under 30 years of age and presented no signs of vascular disease.

In the seventy cases which form the basis of the present paper I have searched carefully for a history of migraine in every instance, but have discovered mention of it only in two cases.

One is the case recorded by Ingvar of a man aged 34 who is said to have suffered from typical migraine for four or five years. He suffered also from chronic nephritis with a blood-pressure of 165. The case terminated fatally and although no actual defect was actually found, microscopic examination of the cerebral arteries showed widespread degenerative changes.

The other case is one which came under my own observation, and it is, I think, sufficiently remarkable to deserve your comments.

The patient is a man now aged 37, who gives the following history: His father is said to have died suddenly at the age of 42 from "hæmorrhage of the gums." I can obtain no details of this illness. He had not previously suffered from bleeding, and there is no evidence to suggest a hæmorrhagic diathesis.

The patient has since the age of 11 suffered from attacks which he describes as being of two separate varieties.

The one variety is clearly of an epileptic nature. There is an aura of pins and needles and twitching, sometimes commencing in the hand, sometimes in the foot, but always on the left side. This is followed by clonic spasms of the left arm and leg. He then loses consciousness. His wife states that loss of consciousness occurs as the movements begin to involve the right side of the body. He enters into a tonic state with cyanosis, followed by generalized clonic movements, and passes out of coma into a deep sleep, from which he wakes in about three hours with severe headache.

The other variety of attack he describes as follows:—

He will perhaps be walking in the street when he becomes aware of what he calls "a spot" obscuring his vision on the left side. This spot looks to him like an irregular watery patch always on the move, which gradually increases in size until his whole vision appears blurred, as if he were looking through a mist. At the same time he sometimes experiences a feeling of pins and needles in the left hand, face and foot. There is never any nausea, vomiting or headache. He does not lose consciousness and is able to carry on with his work as long as this does not involve reading, for which his vision becomes too dim. The whole attack lasts a few minutes.

These attacks, he says, have no relation whatever to the epileptic attacks, and do not alternate with them.

On May 30, 1923, he had a typical epileptic attack in a cinema. During the following week he felt poorly, and complained of a muddled head, but continued at work. On June 6 he played tennis for three hours. At 7 p.m. he went into his garage and cranked up his car. He then came out complaining of a severe pain in the back of the neck. His wife noticed a little twitching of the left hand and foot, and expected him to have one of his usual fits. Instead of this he continued to complain of severe headache, and pain in the back of the neck, and he vomited. The pain and vomiting continued for twenty-four hours and his medical man administered morphia. He remained in bed for ten days, and on getting about again he complained of pain in the bottom of the back. This was shortly followed by a recurrence of the headache, vomiting and neck pain. Lumbar puncture was performed by his medical attendant, and a pinkish fluid which contained no organisms is said to have been withdrawn.

As his condition did not improve he was sent into Guy's Hospital two days later. On admission he was semi-conscious, his temperature was 100°, he presented head retraction and a positive Kernig's sign; no other abnormal signs.

Lumbar puncture yielded blood-stained fluid. In the tube was a considerable sediment of red cells: there was no clot; the supernatant fluid was yellow and gave a negative response with Van den Bergh's direct reaction, and a positive result with the indirect test (the reaction for altered blood pigment).

The protein was 0·09 per cent. The Wassermann reaction was negative in blood and spinal fluid.

He made a gradual recovery and when seen six months later was in good health. He has had several migrainous attacks lately, but no attack of epilepsy since his acute illness.

If there is any theoretical basis for Goldflam's hypothesis here at any rate is a case in which his suggested interpretation would be welcome.

There are many points in this case which call for comment, but time forbids my dealing with them.

In general the prognosis and treatment in a case of sub-arachnoid hæmorrhage must depend upon the primary cause. The question of immediate practical importance once the diagnosis has been made is whether or not to attempt drainage of the sub-arachnoid space by means of lumbar puncture. This has been advocated by many authorities as a routine practice. On theoretical grounds, however, it would appear that due consideration should be given to the individual case with especial regard both to the probable source of the hæmorrhage and its amount.

There is no doubt that in sub-arachnoid hæmorrhage death may occur rapidly from cerebral compression. Drainage of the effusion by means of lumbar or cistern puncture in these cases would seem to be the most hopeful measure of relief. On the other hand, there is a danger in this procedure lest diminution of external pressure upon the point of rupture may lead to fresh bleeding. Cases have been recorded of intracranial aneurysm in which death has occurred immediately after lumbar puncture from this cause. In the early stages of sub-arachnoid hæmorrhage, therefore, the physician must be guided by the presence and severity of signs of cerebral compression.

If life appears to be threatened from this cause the indication for therapeutic puncture is urgent. Yet even to this rule there must be an exception. If the source of sub-arachnoid hæmorrhage be an intraventricular effusion just those conditions are likely to develop which we often see in the case of cerebral tumour—the formation of a pressure cone of cerebellum upon the roof of the medulla, in which case lumbar puncture may lead at once to a fatal issue.

Clinical evidence of intraventricular hæmorrhage should, therefore, be contra-indication for this procedure.

In the later stages when hæmorrhage appears to have ceased, but the

patient is suffering from the effects of the meningeal reaction to the clot, lumbar puncture may be employed to relieve pain and accelerate recovery. Yet even at this stage I feel that it should be used sparingly, with a watch upon the pressure of the escaping fluid and due regard to the probable existence of a recently healed scar only separating the arterial blood stream from the sub-arachnoid space.

In conclusion I would remind you that I have only been able to find in English text-books and journals scant reference to my subject, and would express the hope that some of you may be able to repair this omission both by directing attention to further sources of information and by drawing upon your personal experiences of spontaneous sub-arachnoid hæmorrhage.

DISCUSSION.

Dr. WILFRED HARRIS said he was indebted to Dr. Symonds for his suggestion that the bleeding in some of these cases was due to the formation of congenital aneurysms, but he considered that venous hæmorrhage was an important cause, especially in young subjects. He described the case of a man who lived for fourteen days with a very large collection of blood under the arachnoid. Another interesting example of these was the case of a man who during coitus had a sudden intense headache, and who showed the characteristic picture in the cerebro-spinal fluid.

Mr. LESLIE PATON said that there was one possible cause of sub-arachnoid hæmorrhage to which he would call Dr. Symonds' attention, and that was scurvy. Freud, in a paper published in the *Wiener med. Wochenschrift*, in 1884,[1] described a case of scurvy in which there were bleedings in the subdural space of the brain, in both optic nerve sheaths and all over the inner surface of the retina, and into the vitreous of the left eye ; in the right eye there were only small retinal hæmorrhages.

In dealing with these cases of sub-arachnoid hæmorrhages and their ophthalmic complications it was essential to bear in mind that many of the conditions which might cause sub-arachnoid hæmorrhage might be acting in the retinal vessels, and might produce hæmorrhages and changes in the retina which were not necessarily subordinate results of the sub-arachnoid hæmorrhage. In the cases in which the cerebral blood-vessels were diseased the retinal vessels were probably also diseased, and retinal hæmorrhages might be caused by disease of the retinal vessels. On the other hand, there was little doubt that occasionally sub-arachnoid hæmorrhages did produce direct ophthalmoscopic changes. These changes might be brought about in two ways, first by direct hæmorrhage into the nerve sheath, producing pressure in the nerve sheath and papillœdema. In this case, the pressure was accompanied by loss of sight, and usually the pupil reaction was lost. In the second type of case the sub-arachnoid hæmorrhage was due to the presence of a hæmatoma acting like a cerebral tumour, and giving rise to increased intracranial pressure. The comparative rarity of papillœdema in cerebral hæmorrhages was probably due to two factors. If the hæmorrhage was large enough to produce much increase in intracranial tension on its own account, the patient was usually dead before the increased intracranial tension had had time to produce any effect on the disc head. On the other hand, if the hæmorrhage was small, it was often absorbed fairly rapidly, without producing more than a temporary rise in intracranial pressure. The type of case in which a cerebral hæmorrhage was most likely to produce a papillœdema was that in which the hæmorrhage became encysted. A case of this kind, which he (Mr. Paton) saw more than twenty years ago, was under Dr. Taylor's care at the National Hospital. A large, encysted hæmatoma extended from the occipital region behind to the frontal region in front, entirely covering the parietal region. Its removal was followed by complete subsidence of the papillœdema with retention of perfect vision.

The direct appearance of hæmorrhage through the retina, making its way through the lamina cribrosa and appearing in the eye, was very rarely seen, but he could record two cases in which there had been large sub-hyaloid hæmorrhages into the blood obviously coming down the lymph channels beside the central vessels and in the

[1] Wien. med. Wochenschr., 1884, xxxiv, pp. 244; 276.

subpial zone, and forming large sub-hyaloid hæmorrhages, which in the case that survived ultimately burst into the vitreous.

Dr. F. PARKES WEBER supposed that the term "congenital aneurysm" did not signify that the aneurysm was really congenital, but only that it developed in a congenitally imperfect or predisposed artery. The rare cases of non-syphilitic aneurysms, not of inflammatory nature, occurring in hypoplastic aortas might be mentioned in that connexion. In regard to Goldflam's suggestion that migraine might be an exciting cause of sub-arachnoid or cerebral hæmorrhage—Dr. Weber remembered the case of a man, aged 33 years, subject to attacks of migraine and temporary blindness, who had an intra-ocular hæmorrhage (into the vitreous of the right eye), which, it was suggested, might have been due to extreme venous hyperæmia during the period of arterial relaxation following migrainous preliminary arteriospasm. In another case (a woman, aged 52 years), subject to migraine-like attacks, detachment of the retina had occurred in the right eye. No allusion had been made to the possibility of spontaneous cerebral or sub-arachnoid hæmorrhage in young and apparently healthy subjects being occasionally due to congenital angiomatous or nævoid telangiectatic conditions. But that explanation for the recurrent attacks of hæmorrhage in one of the cases referred to seemed at least as probable as the "congenital aneurysm" explanation. In a recent elaborate work Dr. O. P. Reuterwall[1] recorded his careful examination of the basilar artery in eighty-seven post-mortem examinations; in seven of these he found transverse rents in the internal elastic lamina which during life had undergone connective-tissue repair. Similar conditions would probably have been found in other cerebral and meningeal arteries had they been carefully examined. As these rents tended to be multiple, the internal elastic layer was probably peculiarly "fragile" in such cases. The ages in the seven positive cases varied between 38 and 70 years, and, though in all of them there were arterial atheromatous changes present, the rents had occurred at almost healthy or only slightly diseased sites. Obviously, the same causes (probably blood-pressure strain, slight local disease and a condition of "fragility") which produced the rents in the internal elastic lamina might, in some cases, give rise to complete rupture and hæmorrhage or to the formation of an aneurysm. Reuterwall's work threw light on the occasional occurrence of cerebral hæmorrhage and cerebral aneurysm in young and apparently healthy individuals apart from syphilis and local active inflammation—that is to say, in arteries only slightly diseased.

Dr. W. J. ADIE referred to four cases which had been under his observation, in two of which the typical clinical picture of cerebro-spinal fever was present. Two other cases had occurred in young children, beginning with a very sudden and intense headache. He mentioned the observation of Guillain as to the occurrence of a massive albuminuria in some cases. Mental changes might persist for a long time. He did not think that ruptured aneurysms could explain all the cases.

Mr. F. A. WILLIAMSON-NOBLE described a case in which the cerebro-spinal fluid was deeply blood-stained, and the condition suggested an acute cerebro-spinal fever, but the case on further observation proved to be an example of anthrax meningitis.

Dr. J. G. GREENFIELD referred to the question of the colour of the spinal fluid in these cases. He had not seen the one pink colour which Dr. Symonds had mentioned, but colours varying from brown to orange or yellow. The nature of the pigment was uncertain, but the amount of bile pigment was very small.

Dr. JAMES COLLIER (President) said that with regard to the occurrence of congenital aneurysms, the late Dr. Trevor had a collection of eighteen such cases at St. George's Hospital, in which sub-arachnoid bleeding had taken place. In his experiences these cases had varied in age from 18 to 72. He (Dr. Collier) laid stress on the occurrence of convulsions in certain cases; in two examples these had occurred as late as the twenty-first day after the onset of the first symptoms. Many of his cases had at first been treated as severe rheumatism of the neck. He had employed spinal puncture freely and repeatedly as a therapeutic agent with great relief to the patients, and he thought that the danger of this form of treatment was exaggerated.

Dr. SYMONDS replied.

[1] O. P. Reuterwall, "Ueber bindegewebig geheilte Risse der Elastica interna der Arteria basilaris," Stockholm, 1923.

Section of Neurology.[1]

President—Dr. JAMES S. COLLIER.

Case for Diagnosis.

By W. ALDREN TURNER, C.B., M.D. (Shown by M. CRITCHLEY, M.B.)

A. M., MALE, aged 34.

Two years ago he had writer's cramp in the right hand for six months. In July, 1923, the left arm began to move involuntarily. The movement occurs when the arm lies extended and supinated and consists of:—

(1) Pronation of forearm and internal rotation at the shoulder-joint.

(2) Flexion at the elbow-joint.

(3) Abduction at shoulder-joint and further internal rotation. Occasionally he gets a dull pain in the left forearm.

The physical signs are otherwise negative.

Case of Progressive Muscular Atrophy following Poliomyelitis.

By JAMES COLLIER, M.D. (President).

F. H., MALE, aged 14.

At age of 2 years and 4 months had an acute febrile illness in which he lost the power of the legs. This was followed by wasting in the legs. No pain was associated with this illness.

During the following years he gradually became weaker and developed marked deformities.

At the age of 10 years his arm suddenly became much weaker (there was no febrile illness in association with this).

On examination marked weakness of neck muscles, wasting of arms and of legs in which the only movement left is flexion of the left leg.

No fibrillation observed. Reflexes absent. Wassermann reaction negative.

[1] Clinical Meeting held at the National Hospital for the Paralysed and Epileptic, Queen Square, Bloomsbury, March 13, 1924.

Case showing Spasm of the Muscles of the Face.

By JAMES COLLIER, M.D. (President).

E. G., MALE, aged 44.

In 1920 patient first had an epileptic seizure. Up till examination on February 17, 1923, he had had a series of twenty such fits. There is no weakness of either side, but slight difficulty in speech after each attack.

On examination. — Well built man, unduly emotional, laughing and crying readily. Cerebration slow and intelligence below average. Visual acuity, right, 6/12, left 6/18. Pupils show only the slightest response to light; fundi show vascular changes. His face is expressionless, the lower half of the face is in constant changing spasm. Movement of limbs is slow, power good, no tremor. All reflexes are present, plantars are flexor in type and no sensory disturbance can be made out. Blood-pressure 246 mm. Hg; a trace of albumin in urine.

Since admission, periods of sub-coma lasting two to three days have occurred.

Case of Syphilitic Amyotrophy (? Congenital Lues).

By T. GRAINGER STEWART, M.D. (Shown by E. A. CARMICHAEL, M.B.)

W. B., MALE, aged 49. Complains of weakness of legs with paræsthesia of four years' duration, commencing in the right foot. No bladder trouble, no eye trouble. When seen ten weeks ago the muscles of the limbs and trunk showed fibrillation.

On admission he was found to have disseminated choroiditis; no nystagmus, pupils react well. Motor power in arms fairly good. No wasting. Fibrillation in muscles of shoulder girdle. No intention tremor. Unable to sit up without aid. Legs both weak. Right calf and thigh smaller than left; no fibrillation. Heel-knee and toe-finger tests well performed. Abdominal reflexes present, deep reflexes very brisk. Sphincters healthy. Sense of position slightly defective in both legs. Cerebro-spinal fluid on two occasions showed moderate lymphocytosis and Lange curve of paretic type. The Wassermann reaction was negative in both blood and cerebro-spinal fluid.

Case of Acromegaly and Syringomyelia.

By C. M. HINDS HOWELL, M.D. (Shown by M. CRITCHLEY, M.B.)

R. M., FEMALE, aged 38. Admitted for headaches and painless burns on hands.

Typical acromegalic deformities. Concentric diminution of both fields of vision. Wasting of hands. X-rays showed flattening of the left sella turcica.

May, 1921: Cerebral decompression performed.

February, 1924: A second operation in which a pituitary cyst was tapped. This has been followed by complete relief from the headache.

A Case of Parkinsonian Syndrome with Argyll-Robertson Pupils and Positive Wassermann Reaction. Disappearance of Tremor after Hemiplegia.

By S. A. KINNIER WILSON, M.D.

J. B., MALE, aged 67. For eight years has had lightening pains; three years stiff legs; two years tremor in arms and legs.

In January, 1922, he showed on examination: Parkinsonian syndrome, weakness of right third nerve, Argyll-Robertson pupils, typical diminution to painful stimuli over the chest and arms.

Tendon reflexes exaggerated, left plantar reflex of extensor type. Wassermann reaction positive in blood and cerebro-spinal fluid.

He gradually became worse till 1924, when tremor became increasingly violent. On February 19, he became suddenly unconscious, and on recovery twenty-four hours later he had right hemiplegia, dysarthria and aphasia, but no tremor.

On March 3, 1924, examination showed right hemiplegia with double extensor plantar reflex. Wassermann reaction strongly positive in blood and cerebro-spinal fluid. Rigidity of trunk and limbs but no tremor.

Case of Very Slow Growing Endothelioma removed by Operation.

By S. A. KINNIER WILSON, M.D.

J. S., MALE, 25 years.

In 1913 was struck on the right side of the head by a taxi-cab.

In 1914 tingling began in the left hand. Later in the same year he underwent a right-sided mastoid operation.

In May, 1914, twitching of the left hand spread up the arm involving the left side of the face and then spread into the legs, after which he became unconscious. On recovering consciousness he was paralysed in the left arm and leg for about an hour. Attacks of a similar nature recurred at varying intervals. Between the attacks there was paræsthesia in the left upper limb.

The optic discs showed a suspicion of early papillœdema in 1920, but were not definitely pathological till 1923.

1923: Definite slight weakness in left arm with defective sensation; left plantar reflex of extensor type.

In November and December, 1923, an endothelioma was removed by operations in two stages.

Case of Central Gliosis of Spinal Cord ?

By S. A. KINNIER WILSON, M.D. (Shown by H. J. MACBRIDE, M.D.)

MALE, aged 13. For the last two years has had gradually oncoming weakness in the right lower limb. This involved the left leg about a year ago, and since then he has been unable to walk. Over two years ago he had an "itching" sensation in right upper limb, and then weakness gradually came on in it. There has been incontinence of urine.

On Examination.—Unilateral sweating on right side of face; nystagmus; weakness and wasting of right upper limb; paresis with spasticity in lower limbs. Diminution to pin prick, heat and cold from 7 C to 11 D: loss from 1 D to 10 D. Cotton wool distinguished over this area; passive movement in legs defective.

Reflexes.—Arm-jerks absent on both sides. Knee-jerks and ankle-jerks exaggerated. Both plantars of extensor type. The spine shows no abnormality.

Case of Pontine Lesion of Traumatic Origin with Alternate Paralysis (approximating to but distinct from Millard-Gubler-Foville Syndrome).

By C. WORSTER-DROUGHT, M.D.

P. L., aged 35, in 1916 fell heavily from a motor lorry, his head and upper part of back striking the ground. The impact rendered him dazed and giddy, and subsequently he vomited. On the following day he complained of weakness in the right arm and leg, difficulty in speaking, and noticed that the left side of his face was "twisted"; some urinary incontinence also developed. His condition has changed very little since the accident.

Present condition.—Cranial nerves: pupils equal and normal in reaction; nebula right cornea. Optic discs normal. Fine rotatory nystagmus, more to right than left; ocular muscles otherwise normal. Left facial weakness of lower neurone type with secondary contraction. Some degree of nerve deafness, left ear. Tongue deviates slightly to right on protrusion.

Sensation.—Impairment to light touch, pain, and thermal stimuli over left side of face and right side of trunk including right arm and leg.

Motor functions and reflexes.—Stands with slight deviation towards the right and walks with difficulty, dragging right leg. Right arm and leg spastic, with considerable weakness. No appreciable difference in arm-jerks. Abdominal reflexes: right absent, left normal. Knee and ankle-jerks: right brisker than left. Right ankle clonus. Plantar reflexes: right extensor, left flexor.

Heart, lungs and other systems normal. Wassermann reaction in blood, negative.

Case of Bilateral Facial Paralysis apparently of Traumatic Origin.

By C. WORSTER-DROUGHT, M.D.

J. J. D., MALE, aged 27, while lying in a dressing station in France during 1918, suffering from the effects of "gassing," was involved in a shell explosion. He lost consciousness, and does not remember anything further until he regained consciousness in England, and noticed that he constantly dribbled and that his face felt stiff. His facial condition has remained unaltered.

Present Condition.—There is a bilateral facial paralysis; his forehead is immobile, he cannot close his eyes, and he is unable to pronounce labials. The only movements present are: (1) at the angles of the mouth: the left side showing a little more movement than the right—but on both sides it is very slight; (2) some platysmal movement can be detected on the left side of the

neck; (3) he is able voluntarily to move the left ear upwards and slightly backwards (this being apparently due to a functioning attollens aurem). These three movements can only be performed synchronously and not separately. To faradism, no reaction can be obtained in any muscle supplied by the facial nerve, excepting a sluggish reaction in the levator labii superioris, the lateral retractors of the angle of the mouth, the depressor anguli oris on each side and in the left platysma. The left attollens aurem gives a brisk response. To galvanism, a sluggish response is obtained in the left orbital group of muscles (ACC greater than KCC) and a slightly brisker response (KCC greater than ACC) in the circum-oral group on both sides. The left attollens aurem gives a brisk response and the left platysma a fair response (KCC greater than ACC) but no other facial muscle reacts. Taste is perfectly normal on both sides of the tongue and there is no hyperacusis. All other cranial nerves are normal. Sensation, reflexes, &c., also show no abnormality.

Case for Diagnosis.

By C. Worster-Drought, M.D.

W. F., aged 32, has complained of gradual weakness and wasting of the left leg, which began in 1917; during the past few months slight weakness of the left arm has also developed.

Physical Signs.—Pupils equal and all reactions normal. Slight nystagmus on lateral deviation of eyes occasionally observed. Fundi normal. Slight degree of facial atrophy with slight facial tremor on showing teeth. Some general weakness of left arm, but no intention tremor and no incoördination; left arm-jerks slightly greater than right. Abdominal reflexes: left absent, right very slight. General muscular wasting of left leg, especially of foot. Paresis of dorsi-flexors of left foot and peronei. Tibialis anticus and peronei react sluggishly to faradism. Left knee- and ankle-jerks brisker than right. Plantar reflexes: right flexor, left usually indefinite or sluggish extensor.

Sensation.—There is loss of cutaneous sensibility to all stimuli over the left leg, the area extending to half way up the thigh. Sensation is also diminished, but not absent over the left half of the trunk and left arm. The toes are cyanosed.

The blood yields a negative Wassermann reaction. Cerebro-spinal fluid: two cells per c.mm., protein 0·02 per cent., Wassermann reaction negative.

Case of Cerebro-Macular Degeneration.

By J. S. Risien Russell, M.D.

R. B., female, aged 7. July 25, 1921, the child screamed out; on the following morning the mother found that she was unable to walk. No previous illness or abnormality had been noticed. On November 6 she had a fit, since then she has deteriorated mentally. Parents are alive and well. One boy, aged 2 years, quite well. No deaths, and no miscarriages. Child is illegitimate.

On examination the child exhibits an unhealthy mouth; she is mentally backward for her age, and this makes the physical examination difficult.

Eyesight fair, fundi show an opaque appearance of both discs and retinal pigmentary changes, especially at the macula.

The reflexes are brisk, the plantars are doubtfully extensor in type, and she is doubly incontinent.

Two Cases of Cerebro-Macular Degeneration.

By W. J. Adie, M.B.

J. R., male, aged 11. Symptoms commenced at age of 7½ years. Marked mental deterioration, blindness, occasional epileptiform convulsions and frequent jactitations of head and neck. Tendon reflexes exaggerated. Plantar reflexes of extensor type.

R. R., male, aged 6. Symptoms began a few months ago, with impairment of vision and tremblings and occasional fits. Slight mental deterioration. Tendon reflexes exaggerated. Plantar reflexes of extensor type.

These patients are brothers. The fundi show characteristic changes in both cases. A sister suffers from the same disease.

Case of Progressive Muscular Atrophy (?) occurring Forty-two Years after Infantile Paralysis.

By W. J. Adie, M.B.

E. W., female, aged 42. At age of 6 to 12 months she had convulsions while teething, followed by paralysis of the left leg. Ten or twelve weeks later there was gradual loss of power in the right leg. In September, 1923, at age of 42, gradual loss of power in the left hand began.

On examination there was wasting and fibrillation in both thenar and hypothenar eminences and in both triceps muscles. The arm-jerks are brisk and there is no sensory loss.

X-rays showed enlargement of both transverse processes of the seventh cervical vertebra.

She has used crutches for thirty-seven years.

Section of Neurology.

President—Dr. JAMES S. COLLIER.

DISCUSSION ON THE VALUE OF X-RAYS IN THE LOCALIZATION OF CEREBRAL AND SPINAL TUMOURS, WITH SPECIAL REFERENCE TO VENTRICULOGRAPHY AND LIPIODOL INJECTIONS.

Mr. PERCY SARGENT

said that during a recent visit to Professor Sicard's Clinic in Paris, he had been greatly impressed with the possibilities of Sicard's method of investigating spinal lesions by means of radiography after injecting into the spinal theca a substance opaque to X-rays. The fluid used was "lipiodol," a thick inert oil, containing 40 per cent. of iodine. No bad effects had been noted from the presence of so strong a solution of iodine, probably owing to the fact that the oil parted with the iodine extremely slowly. Lipiodol injected into the muscles of the back could be clearly seen by means of the X-rays two years afterwards.

The technique was then described. With the patient sitting up in a chair, an ordinary lumbar puncture needle was introduced in the mid-line between the atlas and the occipital bone. As soon as the atlanto-occipital ligament was pierced, a little cerebro-spinal fluid was withdrawn, and one or two c.c. of the lipiodol were injected. When no block existed, the lipiodol fell very quickly to the bottom of the theca, and could be seen by means of the X-rays as a cone-shaped mass opposite the second sacral vertebra. When a complete block was present the whole of the lipiodol was arrested, and indicated clearly the relation of the point of blockage to the bones. The lower limit of the block could be ascertained by injecting, at a second sitting, some more lipiodol through a lumbar puncture, and taking a radiogram with the patient head down. As extramedullary tumours were usually small and intramedullary tumours often extended over many segments, the method might prove to be of value in distinguishing between those two kinds of spinal tumour. It was also useful in indicating a level for operation when the exact segmental site of a lesion was not clearly shown by the neurological signs. It had also been employed in cases of fracture of the spine as an aid in determining whether a gross extramedullary lesion existed such as might be relieved by operation. From the surgical standpoint, the method in cases of tumour gave the exact relation of the tumour to the bones, so that it could be approached with greater precision and through a smaller wound.

Sir JAMES PURVES-STEWART

showed a series of pneumo-radiograms of the head in which the normal cerebral ventricles and the outlines of the cortical gyri were clearly outlined. These had been obtained by the fractional injection of air into the lumbar theca up to a total of 70-80 c.c. of air, and not through a trephined ventricle.

In several cases of cerebral tumour in which localization had been somewhat obscure, he had found pneumo-radiography a useful accessory diagnostic measure. Thus, for example, in a right-sided temporo-parietal tumour, the right lateral ventricle was seen to be imperfectly filled and the right cortical sulci unfilled with air, contrasting with the well-filled ventricle and sulci on the other side; moreover, both lateral ventricles were dislocated distinctly towards the opposite side. He considered the method of spinal injection of air especially useful in supra-tentorial tumours, whereas in tumours below the tentorium, he considered it inadvisable on account of the risk of dislocating a tumour towards the medullary centres. As with other physical signs, a negative result in pneumo-radiography was of little or no significance, whilst a positive result was sometimes of high diagnostic value.

He also showed a series of radiograms of the vertebral column after injection of lipiodol into the cisterna magna. In cases of spinal tumour, the lipiodol gravitated downwards to the upper level of the obstruction, where it was clearly seen by radiography. In other cases, injection of lipiodol into the lumbar theca was carried out and the patient was then inverted. By this means the lower limit of a spinal compression was demonstrated.

Care must be taken when carrying out lipiodol injections into the cisterna that the patient's head should not be allowed to hang forwards, otherwise some of the heavy oil might flow downwards along the base of the brain as far as the middle fossa, where it might even produce transient cranial nerve palsy, e.g., paralysis of one facial nerve. In his opinion, lipiodol injections were an important additional means of focal diagnosis of spinal compression.

Mr. GEOFFREY JEFFERSON (Manchester).

A certain amount of attention has been paid in the past to the application of X-rays to the detection of intracranial tumours. In this country, at all events, it appears to have been rather a neglected subject. Oppenheim's discovery, in 1899, of erosion of the dorsum sellæ in a case of hypophyseal tumour aroused an interest which, very naturally, became centred on the sella turcica. Since that date sporadic references appear in the literature, usually in the form of a report on a solitary case, and that, perhaps, of a pituitary growth or a calcifying brain tumour. The importance of small opaque areas as corroborative evidence in the localization of a tumour, particularly of the supra-sellar group of tumours of Rathke's pouch, is becoming more widely and popularly known. Notable amongst the contributions which envisage the X-ray diagnosis of intracranial tumours as a whole are those of Artur Schüller in vol. xi of the "Allgemeine Chirurgie der Gehirnkrankheiten" (1914) and of Heuer and Dandy in the *Johns Hopkins Hospital Bulletin*, 1916, xxvii, p. 224. The most recent paper is that of Elsberg and Schwartz on the X-ray recognition of increase in size of the diploic venous sinuses in endotheliomatous growths (*Arch. Neurol. Psych.*, 1924, xi, p. 292). Of recent years the introduction of air replacement and X-ray as a laboratory aid to diagnosis has largely superseded the older problems in interest. I propose to touch upon various points in the X-ray diagnosis of cerebral tumours so far as I am able to illustrate

them from personal cases. The matter may well be divided under three headings: (1) the general changes in the skull brought about by increased intracranial tension; (2) the local changes in the bone produced by tumours in their immediate vicinity; and (3) ventriculography, a somewhat different category.

(1) THE GENERAL CHANGES IN THE SKULL.

General changes are naturally most marked in those cases in which obstruction to the cerebro-spinal pathway has resulted in ventricular back-pressure and dilatation. Here the fluid tension tends to bring about a more evenly dilating force upon the cranial capsule. These changes may show themselves on the X-ray plate in the following forms:—

(i) *Sutural Diastasis:* The extent of the opening up of the suture lines is a very variable quantity. It is well seen in children up to the age of 10 or 15 years, and I have seen it in a young man of 23. I cannot say at what age it may no longer be expected, but I have not noted it usually in adults. It is most marked at the coronal suture. In the ordinary lateral view it may be seen as a definite widening or sometimes as an ill-defined white streak upon the film or plate. The widening of the sagittal suture is usually at least as well marked, but for technical reasons it is difficult to detect. One may learn a good deal from the comparison of the X-ray of a skull during life and the picture of the calvarium of the same skull after death. The latter may show clearly a huge diastasis with obviously very rapid bony proliferation occurring in the attempt of the skull to keep up with the increasing volume of its contents. (Slides illustrating these facts were shown.) Sutural diastasis will most commonly be found in cerebellar tumours in young people or in obstructive hydrocephalus of adhesive origin. From the practical point of view the periodical X-ray examination of a hydrocephalic skull may be of some value. For in general, separation of the suture is only seen in a pronounced form in acute or progressive cases. (I am not, of course, speaking of tumour cases.) The powers of bone-growth along the suture lines is so great that the bone-defect tends rapidly to be repaired. When a balance has been struck, in those cases which I may, perhaps, term "compensated hydrocephalus," diastasis is no longer a feature of the X-ray photograph. The results of the surgical treatment of hydrocephalus are not so encouraging as to tempt one to advise operation freely, and the absence of diastasis will encourage the surgeon in any given case to pursue a waiting policy.

(ii) *Ribbing of the Skull, or Convolutional Atrophy.*—The peculiar and unmistakable ribbing of the skull, aptly likened to the ribbed sand seen on many seashores at low tide, is a characteristic feature of many X-rays of tumour subjects. It is usually localized to the anterior third of the skull, and for this reason has not won universal acceptance as a definite result of intracranial pressure increase. It is not an infallible sign, and in the absence of choked disc, and so forth, must be regarded as of no special importance. My own experience leads me to the conclusion that it is quite certainly a sign of increased pressure. A certain slight degree of convolutional marking is normal in the skull. In only one case out of several hundred have I seen a high grade of ribbing in a case with normal pressure. It must be remembered that the inner surface of the skull is slightly imprinted by the pattern of the brain which it contains, and it is quite logical to assume that in cases with raised pressure the normal tendency to pattern the cranial capsule will be enhanced. The assumption in my own experience is at all events borne out by the actual clinical facts. According to Heuer and Dandy, and apparently

according to Spiller, this patterning of the skull is due to atrophy of the inner table. With this view I cannot agree. It seems to me much more probable that it is due to diploic compression, by which the two tables tend to approximate to one another at points corresponding with the convexities of the gyri. This may account for the absence of patterning in the crania of infant hydrocephalics and in the sclerosed bones of those advanced in years. In these two large classes, atrophy is not generally seen.

(iii) *Sellar Distortion.*—I shall not deal with pituitary growths themselves, as a sufficient literature on the radiographical appearances of these tumours exists and is widely known.

Sellar distortion is most frequently seen in patients with ventricular dilatation. The sella is normally in relationship with the floor of the third ventricle, and when hydrocephalus is present, the floor of the third ventricle bulges to a considerable degree. This phenomenon is one of which all are cognizant who are in the habit of making post-mortem examinations on tumour cases. Specimens showing it clearly are hard to preserve, for section of the pituitary stalk usually results in the rapid deflation of the ventricle. The results of the fluid pressure are, first, an opening up of the diaphragma sellæ and often actual compression of the hypophysis. (A photograph of the open sella from a case of cerebellar tuberculoma with secondary hydrocephalus was shown.) Later, the pressure may lead to erosion of the dorsum sellæ and posterior clinoid processes, but this sequence is not invariable and will depend on certain anatomical facts. In the first place the relationship of the floor of the third ventricle to the dorsum sellæ is variable; in the second place the strength of the moulding of the sella is variable, and in the third place it is obvious that the hydraulic pressure must often be insufficient or not long enough continued to bring about erosion. That these mutable factors are important is shown by the fact that a large number of cerebral tumours and a certain number of hydrocephalics present no notable change in sellar contour On the other hand, more than one surgeon has mistakenly operated upon the hypophysis on the evidence of sellar deformity and has tapped the floor of a dilated third ventricle instead of the pituitary cyst or tumour which he expected to find there. Crowding of brain tissue into the sella turcica may occur in hemispheral tumours, though its precise mechanism is here harder to envisage. (See Cushing, "Anosmia and Sellar Distension as misleading signs in the Localization of a Cerebral Tumour," *Jour. Nerv. Ment. Dis.*, 1917, xliv, p. 415.)

(2) Local Changes in the Bones.

Hyperostosis of the bones overlying an endothelial or meningeal tumour has won for itself a definite place in cerebral pathology. [Cushing: "Cranial Hyperostosis produced by Meningeal Endotheliomas" (*Arch. Neurol., Psych.*, 1922); Penfield: "Cranial and Intracranial Endotheliomata" (*Surg., Gyn., Obst.*, 1923, viii, p. 139.] Whether or not this bone furring or thickening will be recognizable on an X-ray film must always be partly a matter of good fortune and partly a matter of good management. Only superlatively good pictures can be relied upon to give us the evidence which we need. And in order that any help may be obtained in the recognition of basal growths, stereoscopic views are a necessity.

In this country we have rather neglected to avail ourselves of the help that radiography can give us in localizing endotheliomata. Those arising from the olfactory groove and from the floor of the middle fossa often require special tricks of technique if they are to be discovered, and it is important that both the

neurologist and the radiologist should coöperate in familiarizing themselves with oblique views of these regions.

Spontaneous defects in the skull are still rarer, but I have been fortunate in meeting with one case—a posterior fossa cholesteatoma with a large round defect clearly visible.

Shadows cast by Cerebral Tumours themselves.

If we exclude the tumours of Rathke's pouch, calcification is an uncommon event, uncommon certainly when it is of such degree that opacities are found on X-ray examination. Heuer and Dandy found it six times in one hundred cases; four times the tumour was supra-sellar, one was a so-called osteo-sarcoma of the skull, and the sixth was an aneurysm. Personally, I have not seen a tumour shadow in my series. One may very easily mistake a patch of calcification in the choroid plexuses, pineal, or falx, for a tumour shadow. I have come across two examples of these shadows accidentally, in patients who had been X-rayed for some other purpose. The true nature of these opacities can generally easily be demonstrated by the antero-posterior view. The calcified areas will then be seen to be symmetrically placed on either side of the mid-line (if choroidal) or dead in the centre if the plaque is in the pineal gland or falx cerebri.

(3) VENTRICULOGRAPHY.

We must now turn to a different subject, to air-replacement of cerebro-spinal fluid as an aid to diagnosis. I have little to add to the remarks I made at the British Medical Association meeting last July (*British Medical Journal*, 1923, ii, p. 799). This air-replacement is not an end in itself. It is useful in those cases in which localization of tumours by neurological means is doubtful or impossible. Ventriculography is still in an experimental stage. The reason for this lies in the technical difficulties which surround it, both in actual performance and more particularly in interpretation. We must look forward to improvements; and the principal improvement will probably result through the use of stereoscopic X-rays and of the Potter-Bucky diaphragm. The stereoscopic X-rays enable us to be certain which ventricle we are looking at, and the Potter-Bucky diaphragm gives us clearer pictures with smaller quantities of air.

I have been disappointed with the results of injecting small quantities of air—25 c.c. or less should be enough to give a good skiagram of one ventricle—and therefore here improvement in the X-ray technique will be most useful.

Probably no one will challenge the fact that cerebral tumours produce some ventricular deformity so frequently that an accurate cast or tracing of the ventricular outline would be of great localizing value. I am aware that there are a few tumours which do not distort the ventricles, and in the case of these ventriculography can supply no help.

The chief objections to ventriculography are first of all its risk, and secondly the fact that, even when that risk is taken, we have no guarantee that we are going to obtain anything very helpful from it. These objections, however, should be surmountable with practice and experience. The method is justifiable only when used by a neurological surgeon who is fully cognizant of intra-cranial physiology, and who will therefore be able to deal with any unpleasant developments when they may occur.

My own experience to date includes thirteen injections in ten cases. One patient, a blind hydrocephalic child, died three days after the injection, but it

was so advanced in disease that it might have died from natural causes. Another case was that of a woman with an unlocalized tumour. I tried a double puncture of the posterior horns and failed to withdraw cerebro-spinal fluid for some reason unexplained. She died seven days later and a post-mortem was refused.

I am inclined to use the method sparingly; obviously, the better the neurologist the less the need for ventriculography. The upholders of the method have harmed their cause by claiming too much for it, and by publishing photographs of tumours which should have been localized easily enough by less dangerous means. But if in ventriculography we have a method which may help us to decide that a tumour is not present (e.g., a posterior fossa arachnoiditis with secondary hydrocephalus, pseudo-tumour), or to localize it when the patient is almost blind or otherwise non-coöperative, then it must be admitted that we have added a new weapon to our armoury. The chief danger appears to me to be its too indiscriminate use, particularly by surgeons who have had no great experience or training in cerebral surgery.

Mr. Adams McConnell (Dublin).

I wish to demonstrate first that injection of air into the lateral ventricle does furnish data for an exact diagnosis in some cases. [Slides illustrating distortion or dilatation of the lateral ventricles were shown, and it was pointed out that the cases presented either no localizing symptoms or symptoms of an equivocal nature.] These slides prove that in certain cases more can be learned from ventriculography than from any other method. In ten cases out of fifteen, I have obtained definite results. Such results increase the possibility of radical treatment and are sufficient to vindicate the value of the method if there be no intrinsic danger in its practice. Two patients died, one eight and the other fourteen hours after injection; both were perfectly comfortable till they sat up in bed, and both fell back dead as if they had been shot, both had large tumours in the posterior fossa with gross pressure deformity of the medulla. It is questionable whether air injection was the responsible agent in these fatalities or not, for sudden death is a common occurrence in such cases, but it is safer to blame the method and to consider whether such fatalities are avoidable. The most important thing in technique is to prevent any sudden change in intracranial pressure during the substitution of air for fluid. I find that this can be accomplished by the slow withdrawal of small quantities of fluid and by the slow injection of air. The simplest indication of intracranial pressure is the blood-pressure. With the needle in the ventricle one can play on the blood-pressure, raising it at will by injection of air, or lowering it by aspiration of fluid. Our aim should be to keep the blood-pressure steady during the whole procedure. Slow aspiration of small quantities and slow injection ensures a steady intracranial pressure and precludes any sudden variation in the relation of the intracranial contents.

During a recent visit to America I saw some marked reactions after ventriculography and these were due to the fact, in my opinion, that ten to twenty c.c. of fluid were withdrawn at a time, and a similar amount of air injected, the procedure taking but a few minutes. Reactions seem to be due to faulty technique. I emphasize this point, for the safety of the method depends on it. Other points in technique are of secondary importance, though they exercise an influence on the amount of information obtained.

I have used Keen's marking in the majority of cases, for I find it easier to

tap the trigone of the ventricle than to tap the posterior horn. The posterior horn varies in size to a marked degree; sometimes it is absent and sometimes it is occluded by pressure. The occipital route is often easy when there is internal hydrocephalus, but when the lateral ventricle is normal it is not so easy to visualize. The occipital route is the popular route in America. In some cases it is necessary to tap both ventricles. The interpretation of the radiograms is not always easy, but neither is it invariably easy in any region of the body. Even in these days radiography of the stomach is sometimes misleading and ventriculography is yet in its infancy. Correlation of indefinite symptoms with a radiogram inconclusive in itself may furnish a working hypothesis.

Dr. C. P. Symonds

remarked that a question of some interest in connexion with the present discussion was, whether an increase in the protein content of fluid obtained by lumbar puncture, or arrest of lipiodol, would give the earliest evidence of spinal compression.

He had recently had the case under his care of a woman who had for some years presented signs of a transverse lesion of the spinal cord in the upper dorsal region. She first came under his observation in August, 1921, and at that time the protein content in the cerebro-spinal fluid was 0·02 per cent. In September, 1923, although the physical signs of compression had advanced very little, the protein content was 0·27 per cent. in the fluid obtained from lumbar puncture, 0·01 per cent. in that withdrawn from the cisterna magna; 2 c.c. of lipiodol injected by the cisternal route were arrested at the level of the third dorsal vertebra. Mr. Bromley subsequently removed an extra-dural lipoma.

In another case seen quite recently of a man with a six months' history of gradually progressing transverse spinal lesion, the protein content of the lumbar puncture fluid was 0·09 per cent., that of the fluid from the cistern 0·01 per cent. Subsequently lipiodol was injected by the cisternal route and all of it had reached the lower end of the thecal sac in twenty minutes. At operation there was disclosed a fusiform enlargement of the cord (probably an intramedullary tumour), at the level which corresponded to the clinical signs. From the appearances at operation it seemed that the subarachnoid space must have been almost occluded.

Dr. Wilfred Harris (Chairman) and Mr. L. B. Rawling

both expressed themselves as being very favourably impressed by the possibilities of the injection of lipiodol in the exact localization of spinal tumours, but raised doubts as to the value of ventriculography in comparison with its possible dangers.

Mr. Jefferson (in reply)

said that the warning and the pessimistic views of the Chairman and of Mr. Rawling with regard to ventriculography would be shared by many. There was a feeling that to be a supporter of ventriculography was to decry oneself as a neurologist. He (Mr. Jefferson) could not agree with that view. Nor could he agree with Sir James Purves-Stewart's method of lumbar injection. The reaction to this manœuvre was severe, and large quantities of air must be used. As Mr. McConnell had pointed out, if air was placed into the spinal theca by lumbar puncture, and a subsequent X-ray showed only one ventricle filled, one could not conclude that the other ventricle

was obliterated by a tumour. Air-locks might readily form and prevent even distribution. He did, however, fully agree with Sir James's statement that the chief use of ventriculography would consist in the localization of hemispheral tumours, and this he had himself maintained at the British Medical Association meeting at Portsmouth last year (1923). Tumours in the posterior fossa should certainly very rarely need other means of diagnosis than the classical ones long current.

Mr. ADAMS MCCONNELL (in reply)

did not think that it was possible to draw definite conclusions regarding the shape of the ventricles unless they were filled with air. They could not be filled accurately by injection of the subarachnoid space of the cord. He considered lumbar puncture much more dangerous than ventricular puncture when a growth was present in the posterior cranial fossa.